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PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICIN

EDITED BY

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UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE FOURTH

SESSION 1910-11

PART II

EPIDEMIOLOGICAL SECTION
LARYNGOLOGICAL SECTION
MEDICAL SECTION
NEUROLOGICAL SECTION
OBSTETRICAL AND GYNÆCOLOGICAL SECTION

1805-1834
1907

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1911

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

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John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

Epidemiological Section.

October 28, 1910.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

The Enteric-fever Carrier.

THERE is nothing in the recent history of epidemiology so remarkable as the increasing degree of importance that has come to be attached to the living host as a breeding ground of the organisms that cause disease in man, and as an agency in the transmission of such disease. In some diseases the living host is not man himself only: in malaria and yellow fever, formerly believed to be largely due to miasmata arising from the earth, the mosquito now ranks with man in the cycle of infection that secures the transmission from host to host, and, coincidently, the continued existence of the infective organism. In other diseases, such as enteric fever, the living host, so far as we know, is man alone.

In the case of enteric fever, however, it was difficult to understand the continued endemicity of the disease on the hypothesis that infection could be derived, directly or indirectly, only from persons suffering from the fever; and this the more that, formerly, the possibilities of infection by the typhoid sick were, in fact, underrated. So that the view that the infective organism of enteric fever could live and multiply outside the human body naturally met with acceptance, as affording an explanation of the difficulty. But further investigation indicated that the extent to which this organism could lead a saprophytic existence outside the human body had been, at the least, greatly over-estimated. Following on this change of view came the discovery of the enteric-fever carrier, and with it the belief that the continuance of this fever is mainly, if not wholly, due to the enteric-fever case and the enteric-fever carrier.

The enteric-fever carrier is a person who, although he may be in good health, carries the infectious material of the fever in his body, from which it may be given off in the stools or in the urine. This infectious material, which is the cause of the fever, is now commonly admitted to be the *Bacillus typhosus*.

Facts pointing to the existence of the carrier condition in relation with enteric fever have been accumulating for a considerable number of years. The earlier observations of the presence of the *Bacillus typhosus* in bone abscesses and the like, in persons who had long recovered from the fever, did not lead directly to the conception of the carrier as now understood. Neither did the discovery of the urinary carrier, in 1898, receive at once the amount of attention its importance merited; nor did this discovery suggest the existence of the allied condition that characterizes the intestinal carrier, the recognition of which followed some years later as a result of the enteric-fever campaign initiated in South-Western Germany in 1903. Since 1903 this subject has been investigated by many observers, and its literature has already attained very considerable dimensions. Numerous occurrences of enteric fever, both in epidemic and endemic form, have been attributed by these observers to carriers. It cannot be said that, in all the instances of the kind that have been recorded, the facts have always been investigated with the thoroughness and care that should characterize epidemiological investigation; the presence of a carrier has, in some cases, been taken as in itself sufficient proof of the source of infection, and other possibilities have not been sufficiently excluded. But, on the other hand, there are many instances of carefully investigated occurrences of enteric fever in which the evidence incriminating a carrier is conclusive. In view of the results of such investigations there can be no doubt that the enteric-fever carrier may give rise to the disease in other persons. But, while this proposition may be accepted as true, there remain not a few matters in relation with the carrier which need consideration. These matters have to do with the two questions that are, from the practical point of view, of chief importance—viz., what is the proportion of carriers among us, and in what degree is the carrier a danger to the community in which he lives?

There is a considerable body of evidence as to the proportion of persons who acquire the carrier condition as a sequel to an attack of enteric fever. In the statistics on this subject, carriers are usually divided into two classes, based upon the less or greater length of time during which the *Bacillus typhosus* may be recovered from their stools or urine. The classifications of this sort that have been suggested vary in their terms and in their subdivisions, but the main distinction made is between the temporary carrier and the chronic carrier. Roughly, it may be said that the temporary carrier is a person who excretes the typhoid bacillus for a short time only, while the chronic carrier excretes the bacillus for an indefinite period, extending, it may be, over a long term of years. The border line between these two classes cannot well be defined exactly; but most observers regard a carrier as temporary if he ceases to excrete the bacillus within about three months of his attack of fever or of a relapse, and as chronic if the excretion of the bacillus continues beyond that period. It may be doubted whether the three-month limit affords a satisfactory basis of distinction between the two classes, inasmuch as, from the public-health point of view, chief importance attaches to the person in whom the carrier condition becomes permanently established, and excretion of the bacillus for a period of three or four months would not seem to justify assumption of permanent establishment of the condition. In recognition of this, as will be seen from some of the statistics[1] I proceed to quote, certain observers have considerably extended the period of three months.

As a result of the examination of 400 enteric-fever convalescents, Lentz found that 18 persons, or 4·5 per cent. of the total number, continued to excrete the typhoid bacillus for more than ten weeks; six of these, however, did not continue excreting the bacillus for more than thirteen months; so that, if the longer period be taken as the limit, the proportion of chronic carriers in these 400 persons was 3 per cent.

Klinger, who examined 482 enteric-fever convalescents, found that eight, or 1·7 per cent. of the total, continued to excrete the typhoid bacillus for more than six weeks after the patient's temperature had fallen to the normal.

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[1] The German and American data are taken from Dr. J. C. G. Ledingham's report to the Local Government Board on "The Enteric-fever Carrier," 1910.

The most extensive investigations on this subject are those reported by Frosch and by Prigge. Frosch records the examinations of 6,708 cases of enteric fever, of whom 310 were classed as carriers. Of these 310, 144 did not continue to excrete the typhoid bacillus for three months, while 166 continued to excrete the bacillus for three months or more; giving, on the three-month basis, 2·1 per cent. of the total as temporary carriers, and 2·5 per cent. as chronic carriers. Prigge's figures deal with 10,481 cases, and on the basis he takes of regarding as temporary carriers those who continue to excrete the bacillus for a year, and as chronic carriers those who continue to excrete the bacillus for a longer period, he arrives at percentages of 0·5 per cent. for temporary carriers, and 0·3 per cent. for chronic carriers.

The highest proportion of carriers observed in enteric-fever convalescents is that recorded by Semple and Greig, who found that 10 out of 86 persons examined continued to excrete the typhoid bacillus for more than six weeks after their temperature had fallen to normal. It is possible that this high figure of over 11 per cent. more truly represents the proportion of carriers among enteric-fever convalescents than the figures already quoted, since the latter are based on the results of only two or three examinations of each person, while, in the case of Semple and Greig's convalescents, examinations were made daily. On the other hand, it has to be borne in mind that the number of Semple and Greig's cases is small; moreover, all the persons were adult males and do not therefore afford a proper basis for comparison with an ordinary population.

The differences between the percentages of chronic carriers found among enteric-fever convalescents by these observers are, it will be seen, very considerable; ranging, even in the case of the extensive investigations of Prigge and of Frosch, from 0·3 per cent. to 2·5 per cent. It is doubtful, however, whether these figures, such as they are, fully represent the proportion of persons who develop the carrier condition as a sequel to enteric fever. The contrary would appear to be the case, in view of Kayser's examinations of a number of persons, made after a period of more than a year had elapsed since their attack by enteric fever. Kayser had examined the excreta of 200 persons, during and for some time after their convalescence from enteric fever, and had discovered three carriers among them—a proportion of 1·5 per cent. After the lapse of more than a year he conducted a further series of examinations of the excreta of 101 of these persons, and discovered three more carriers, which raised the carrier proportion to 3 per cent. An investigation on similar lines has been made by Brückner, who examined the excreta of 316 persons who were known to have had enteric fever in previous years. Of these 316 persons, 12 were found to be carriers; nine (i.e., 2·8 per cent.) being typhoid carriers, and three being paratyphoid carriers.

Consideration of the various statistics on this subject would seem, upon the whole, to suggest that if the present methods of examination be accepted as sufficient, the proportion of persons who may be expected to become chronic carriers as a sequel to an attack of enteric fever is about 3 per cent.

But it would appear that calculations based upon the number of persons who, consequent upon an attack of enteric fever, become carriers, are not likely to afford trustworthy means of estimating the proportion of carriers in a community. For there are observations which go to show that an attack of enteric fever is not necessarily a condition precedent to the creation of a carrier. An illustration of this is afforded by Klinger's examination of the excreta of 1,700 persons who were in more or less close relation with cases of enteric fever. Among these 1,700 persons he discovered 15 carriers, 11 of whom had no symptoms of enteric fever and did not subsequently develop the disease. These 11 persons proved to be temporary, not chronic, carriers. Similar facts have been recorded by other observers, and instances have occurred in which the temporary carrier condition, acquired in this way, has passed into the chronic stage.

That the carrier condition is not necessarily dependent upon a previous attack of enteric fever is also suggested by the proportion of instances in which inquiry fails to reveal any history of such an attack. The degree of importance to be attached to this consideration is no doubt affected by the difficulty of being certain that persons questioned as to their previous history in this respect may not overlook some slight illness which might have been a mild attack of enteric fever. Against this possibility, on the other hand, has to be placed the fact that the careful investigator does not omit to inquire closely as to any past occurrence of vague or obscure illness, suggestive of an unrecognized attack of the disease. The data furnished by Prigge on this point show that of 35 chronic carriers discovered by him among 10,841 persons examined, eight had no history of enteric fever. The larger figures given by Klinger on the same subject show approximately the same proportion of chronic carriers with no history of enteric fever. He deals with a total of 220 chronic carriers, as regards 20 per cent. of whom history of this kind was lacking. It is noteworthy that he could obtain no such history in so large a proportion as 56 per cent. of 211 temporary carriers recorded by him. The statistics of Fornet yield a higher proportion of carriers, both temporary and chronic, who had no history of enteric fever, than do those of either Prigge or Klinger. Fornet records that, of 91 carriers, the proportion of temporary carriers without a history of enteric fever was 74 per cent., and that of chronic carriers without such a history was 41 per cent. These proportions of carriers who present no history of enteric fever may be explicable by the difficulty of obtaining trustworthy information, to which I have referred; they are, however, high enough to warrant doubt as to the sufficiency of such an explanation.

Some degree of knowledge as to the extent to which the carrier condition is directly dependent on a previous attack of enteric fever may perhaps be looked for from the endeavour to ascertain whether any increase in the number of carriers takes place in a community that has recently suffered from a severe epidemic of enteric fever. If a material increase in the number of carriers in such a community takes place after an epidemic of the fever, it is to be expected, on the assumption that the carrier is a material danger to others, that there will be an appreciably greater amount of enteric fever in the community in years following the epidemic than in years preceding it. In considering this matter, it is desirable, with a view to simplifying the issue, to select instances in which the cause of the epidemic is not a continuing one, but ceases to operate after the termination of the epidemic; and I have, accordingly, taken as illustrations the epidemics that occurred in Worthing in 1893, in Maidstone in 1897, and in Lincoln in 1905. All these epidemics were due to a pollution of the public water-supply which has not, in any of the three places, recurred since the outbreak.

It will be seen from the table on p. 7 that in 1893 there were, in Worthing, 1,320 cases of enteric fever, with 166 deaths. During the ten years immediately preceding 1893 the deaths due to enteric fever numbered 15, while during the ten years following 1893 such deaths amounted to 11. It is also worthy of note that there were no deaths from enteric fever in 1894 and 1895, while there were but two such deaths in the five years immediately following 1893. As regards the number of persons attacked by enteric fever, the means of comparison are, unfortunately, less satisfactory, owing to there having been no notification of infectious disease in Worthing prior to 1890. The attack figures, however, point, so far as they go, in the same direction as the mortality figures. The average annual number of persons known

Year	BOROUGH OF WORTHING Population, 1891 : 16,606 ,, 1901 : 20,015		BOROUGH OF MAIDSTONE Population, 1891 : 32,145 ,, 1901 : 33,516		CITY OF LINCOLN Population, 1891 : 41,491 ,, 1901 : 48,784	
	Enteric-fever deaths	Enteric-fever cases	Enteric-fever deaths	Enteric-fever cases	Enteric-fever deaths	Enteric-fever cases
1881	1		5		8	
1882	1		3		12	
1883	2		6		8	
1884	2		4		7	
1885	3		6		10	
1886	1		4		8	
1887	—		3		5	
1888	—		1		11	
1889	2		1		11	
1890	1	10	3	16	7	
1891	—	11	3	15	2	
1892	4	21	2	15	6	
1893	166	1320	7	35	12	
1894	—	10	1	11	7	
1895	—	2	2	19	6	
1896	2	6	1	16	11	
1897	—	6	130	1888	6	
1898	—	3	2	34	7	
1899	1	11	3	23	7	
1900	2	10	1	28	6	19
1901	—	6	1	21	5	15
1902	4	9	—	11	3	12
1903	2	5	1	9	4	32
1904	—	9	—	5	3	17
1905	1	15	2	5	131	1054
1906	1	5	—	5	3	22
1907	1	6	3	7	6	17
1908	1	8	—	4	5	22
1909	—	7	1	3		

to have been attacked by enteric fever in Worthing during the three years 1890-2 is 14; while the average annual number of attacks was a little over five in the five years following 1893, and nearly seven during the ten years following that year. It appears, therefore, that in Worthing there was less enteric fever in the years following the epidemic year than in years preceding it.

In Maidstone in 1897 there were 1,888 cases of enteric fever and 130 deaths due to this cause. During the ten years preceding 1897 the deaths from enteric fever were 24 in number, while during the ten years following 1897 they numbered 13. The number of cases of the fever during the seven years preceding 1897 were 127, while during the ten years following that year they were 148; so that the average annual number of attacks was 18 in the first period and 15 in the second. There was, however, a somewhat greater number of attacks in the four years immediately following 1897, during which the average annual number was 26, than in the eight years preceding 1897; and this increase is not to be accounted for by increase of the population, which probably rose only by about 500 or 600 in the course of these four years. Nevertheless, it cannot be said that there was material increase in the amount of enteric fever in Maidstone in the years following the epidemic.

In Lincoln in 1905 there were 1,054 cases of enteric fever, with 131 deaths. In the ten years preceding 1905 there were 58 deaths from enteric fever, giving an annual average of six; and there were, in the five years preceding 1905, 21 deaths from the fever, giving an annual average of four. In the three years following 1905 the fever deaths were 14, which gives much the same average as in the five years preceding 1905. As compulsory notifications did not come into force in Lincoln until 1900, no record of enteric-fever attacks is available prior to that year. During the five years preceding 1905, 95 persons were notified as suffering from enteric fever, while, during the three years following 1905, 61 such notifications were received. The average annual number of attacks, therefore, was 19 in the first period and 20 in the second period. The periods are short and the value of the figures is thereby diminished in some degree. Such as they are, they indicate a slight increase of enteric fever in the three years following the epidemic year; and this slight increase is the more noteworthy as being in contrast with the marked and steady decline of enteric fever in this country since 1900, a decline in which[1] Lincoln shared up to 1904. Yet, even if due allowance be made for this consideration, the increase of enteric fever in Lincoln since the epidemic year cannot properly be regarded as material in amount.

It may be said, in objection to the contention that these data appear to indicate that an epidemic of enteric fever does not give rise to a crop of carriers, that the epidemics in question affected so large a proportion of the population as to create a number of immune persons sufficient to compensate the risk attendant upon increase of carriers. In Worthing in 1893 the fever is known to have attacked 8 per cent. of the population; in Maidstone in 1897 the proportion was nearly 6 per cent. of the population; and in Lincoln in 1905 it was 2 per cent. of the population. Even if, in view of the probability of there having been many unrecognized cases of the fever, considerable allowance be made in each instance for a corresponding increase in the number of immunes, it is difficult to believe that the proportion of these persons would counterbalance the effect of the introduction of a large number of infective carriers into a community still largely susceptible. On the basis of 3 per cent. of persons who have had enteric fever becoming carriers, the Worthing epidemic of 1893 would have added no fewer than 39 carriers to the then population of about 17,000; the Maidstone epidemic of 1897 would have added 57 carriers to a population of about 33,000; and the Lincoln epidemic of 1905 would have added 30 carriers to a population of about 52,000. These figures are so considerable that an appreciable effect would be looked for save in a population almost wholly immune against enteric fever. Moreover, it has to be borne in mind, as regards Worthing, which is a popular seaside resort, that large numbers of people visit that town and take up their residence there every year, more especially during the summer and autumn months; and, as regards Maidstone, that about one-third of the town, which did not receive the contaminated water supply, suffered comparatively little from the fever.

There is, however, one hypothesis which, if accepted, might afford an explanation of this apparent absence of increase of carriers after an epidemic of enteric fever. This hypothesis is one that has been put forward by Fornet, and is to the effect that the carrier condition is not the direct result of an attack of enteric fever, but is due to a re-infection in a person who has been rendered immune by a former infection. I do not propose to discuss this theory; it is enough to note that the facts I have mentioned are consistent with it, inasmuch as the occurrence of a large number of cases of enteric fever in a district would not lead to a corresponding increase of carriers in the absence of a subsequent introduction of enteric fever on a sufficiently large scale. The district would, in fact, contain many persons — who might be termed potential carriers—who would not become actual carriers until reinfected.

In connexion with the question of the degree of dependency of the carrier condition upon a previous attack of enteric fever, the age constitution and the sex constitution of persons who suffer from the fever, and of persons who become carriers, are of interest.

It is well known that fatal enteric fever is much less common among children under 15 years than among persons over that age. In England and Wales, in 1908, rather less than one-seventh of the total mortality from this cause occurred among children under 15, although this group forms nearly one-third of the population of the country. Owing, however, to the comparatively low fatality of enteric fever in children, these figures do not afford a correct indication of the relative proportion of children attacked by the disease, which is much higher than the relative proportion dying of it. The figures given by Sir Shirley Murphy for the three years 1906-8 show that in the County of London the attack-rate of enteric fever is much the same among persons under 15 years as among those over that age, being 29·9 per 100,000 in the former group, and 30·6 per 100,000 in the latter group. Of the total number of 4,351 cases of the fever in these three years, 1,280, or 29·6 per cent., were under 15 years of age. For Manchester, during the ten years 1897-1906, Dr. Niven gives, out of a total of 5,164 cases of enteric fever, 1,596, or 30·9 per cent., as being under the age of 15. The statistics of Klinger for the two years 1906 and 1907 show a slightly higher incidence upon children. In those years, in a population of about 2,300,000, 3,867 cases of enteric fever were detected; of these, 1,223, or 31·6 per cent., were under 15 years of age. With these figures showing the incidence of enteric fever as regards age, may be compared the data furnished by Klinger regarding the ages of the carriers detected in the course of the campaign against enteric fever in South-Western Germany up to the end of 1907. These were 431 in number; and of this number, 211 were temporary carriers and 220 were chronic carriers. Of the 211 temporary carriers, 75, or 35·5 per cent., were under the age of 15; of the 220 chronic carriers, nine, or 4 per cent., were under that age. It appears, therefore, that as regards the temporary carrier condition, the incidence on age is fairly similar to that of enteric fever; but that as regards the chronic carrier condition, the incidence on age is widely different from that of the fever.

The incidence of fatal enteric fever in this country is higher on males than on females; during the decade 1891-1900 the death-rate from this cause was 200 per 100,000 living among males, against 150 per 100,000 living among females. In 1908 the rate per 100,000 living was 94 among males, against 56 among females—a proportion of 100 to 60. The enteric fever attack-rates per 100,000 living in London during the five years 1901-5 were 62 for males and 46 for females; and during the three years 1906-8 were 34 for males and 27 for females. These rates give proportions of 100 males to 74·2 females in the earlier period, and 100 males to 79·4 females in the later period. Klinger's statistics, already quoted, show that of the 3,867 cases of enteric fever discovered, 2,177, or 56·3 per cent. of the total, occurred among males; while 1,690, or 43·7 per cent., occurred among females. Klinger's figures as to carriers, however, show a sex distribution of this condition differing from the sex distribution of enteric fever, more especially in relation with the chronic carrier. Of his 211 temporary carriers, among whom the difference is less marked, 89, or 42·2 per cent. of the total, were males; and 122, or 57·8 per cent., were females. The difference is a notable one; the excess of temporary carriers among females is about the same as the excess of enteric fever among males. It is, however, much less striking than the excess of chronic carriers among females, inasmuch as of 220 persons affected with this condition, 182, or 82 per cent. of the total, were females; and only 38, or 17·3 per cent., were males. As regards temporary carriers, the sex influence does not appear to have been materially modified by age; the disproportionate incidence on females was slightly greater at ages over 15. As regards chronic carriers this was also the case, but the number of chronic carriers under the age of 15 was too small to render this fact of appreciable value for inference.

There is, however, a consideration which suggests that female chronic carriers may not be so greatly in excess of male chronic carriers as would, from the observations of Klinger and others, appear to be the case. This condition, it may be contended, is more likely to be discovered in women than in men; for carriers are usually detected in consequence of investigations brought about by occurrences of fever which lead to suspicion falling on particular persons. The greater, therefore, the facilities for the carrier in the way of transmitting the infection, the greater is the likelihood of his giving enteric fever to others, and consequently of attracting attention, leading to his detection. It is evident that women, who usually discharge the ordinary duties of the household, including that of cooking, are, for this reason, in circumstances exceptionally favourable for the transmission of infection, and therefore exceptionally likely, when they are carriers, to be detected. It may be doubted, however, whether this consideration is sufficient to explain the very high proportion of more than four women to one man in the carrier statistics quoted. Moreover, the resemblance between this incidence of the carrier condition upon the sexes, and the sex incidence of gallstone disease, which is about three times as frequent in women as in men, suggests that the former may really hold good, in view of the relationship, whether causative or consequent, which has been observed between the carrier condition and gallstone disease.

In view of the fact that influences of sex and of age are not always capable, in our present state of knowledge, of satisfactory explanation, it would not be reasonable to lay much stress on the facts I have cited in relation with this subject. At the same time, the apparently different influences of sex and of age in determining susceptibility to enteric fever on the one hand, and to liability to the carrier condition on the other hand, at least suggest that the relationship between that condition and the fever may not be so direct as is commonly supposed.

The various statistics that I have cited regarding the proportion of persons who become carriers after an attack of enteric fever, of carriers among persons living in contact with the sick, and of investigations such as those of the campaign against enteric fever in South-Western Germany, do not enable us to arrive at a trustworthy estimate of the proportion of carriers in communities generally. Indeed, it is to be expected that this proportion would be found to vary widely in different populations, and, in so far as epidemiological observation is concerned, it is likely to be long before data are accumulated to such an extent as to afford a sufficient basis for accurate judgment. Some information, however, is available as to the number of persons in certain populations found, on bacteriological examination, to be excreting the typhoid bacillus. I do not quote figures of this kind regarding populations in which the researches had been prompted by the occurrence of exceptional amount of enteric fever, since the circumstances were, on that account, unusual. Statistics not open to objection of this kind are those of Minelli and of Rosenau, Lumsden, Kastle, and Park. Minelli conducted a bacteriological examination of the excreta of the inmates of the town prison of Strassburg, which had been free from enteric fever for several years. The inmates were 250 in number, and one proved to be a carrier. A prison population, however, is not a typical one, and the figures are small. More extensive statistics, relating to an ordinary population, are those furnished by Rosenau, Lumsden, Kastle, and Park, who examined 1,014 samples of fæces and 26 samples of urine from 993 persons living in different parts of Washington from the latter part of July to early November—this being regarded as the enteric-fever season. Three carriers were discovered; a man, aged 50, and two boys. The proportion, it will be seen, was about three per 1,000, much like that found by Minelli. If these results could be accepted as affording a trustworthy index of the proportion of carriers generally obtaining, it would follow that there are, at the present time, in Manchester some 2,000, in London more than 14,000, and in the whole of England and Wales about 108,000 carriers. But even the figures of Rosenau, Lumsden, Kastle, and Park are not ample enough to warrant inference of this sort; when such a rate as three per 1,000 is in question, the examination of many thousands of persons is necessary for its establishment. Moreover, what may be true of the population of Washington is not necessarily true of other populations of different habits and living in different circumstances.

The question as to the degree in which the individual carrier is a danger to the community in which he lives involves several considerations: With what degree of frequency does he excrete the typhoid bacillus; and how often is he in an infectious condition? Is his infectivity as high as that of a person suffering from enteric fever? And is the type of enteric fever contracted from a carrier as severe as the type of fever contracted from a person suffering from the disease?

As regards excretion of the typhoid bacillus, it is necessary to bear in mind that it is possible, owing to the difficulties encountered in isolating the bacillus from the fæces, that it may not be detected if present only in small amount. Subject to this reservation, however, it appears that the excretion of the bacillus by the carrier is ordinarily intermittent, and that, in many instances, these intermittencies are of considerable duration. In the case of the Brentry carrier, the bacillus was recovered from the fæces in December, 1907, but examinations made in the months of January, February, and March, 1908, failed to reveal the presence of the bacillus. The bacillus, however, was again found in the fæces in April, 1908, and in all the remaining months of that year in which examination was made (and May was the only exception). In 1909 the bacillus was found in the fæces in January.

February, March, April, and July. Since July this carrier has been lost sight of, and, in consequence, no further examinations have been made. Between December, 1907, and July, 1909, examinations were made on 40 occasions, on 32 of which the bacillus was found in the fæces. In this case, it will be observed, there was no period of intermission of long duration. Comparatively short periods of intermission have been observed in other instances, such as that of McC., recorded by Dr. J. C. G. Ledingham. McC. was found to be an intestinal carrier in November, 1907, and between that date and June, 1910, her fæces were examined on 44 occasions, on 37 of which the bacillus was found. In certain months no examination was made; if these be excluded from consideration, in one month only did the fæces give negative results. In the case of another intestinal carrier, G., recorded by Dr. Ledingham, 47 examinations of the fæces, made between September, 1907, and June, 1910, gave positive results on 31 occasions, and in this instance also, if months in which no examination was made be excluded, there was no period of intermission longer than one month. It may be noted, however, that in the case of both McC. and G. there was a period of three months during which no examination of the fæces was made. Another case of Dr. Ledingham's, however, showed a longer period of intermission. This was a Mrs. C., who had enteric fever in July, 1908, and from whose fæces the typhoid bacillus was recovered in October and in December of the same year. Subsequent examinations of the fæces in January, March, April, May, July, and August of 1909 gave negative results, so that Mrs. C. was regarded as being merely a temporary carrier. But in January, February, and March of 1910 the typhoid bacillus was again found abundantly present in the fæces, and this woman came to be considered as a chronic carrier. Much longer periods of intermission in the excretion of the typhoid bacillus by chronic carriers, however, have been noted by some observers. Monthly examinations of the fæces of such carriers, made by Prigge, resulted in his noting periods of intermission, in some instances ranging from one year to two and a half years, and in one instance he records a period of intermission lasting as long as three years.

It must, however, be admitted, as regards all these figures that I have quoted, that exception may be taken to them on the ground that the examinations, which were usually monthly or even less frequent, were made at intervals too far apart to warrant deduction that there was continuous freedom from excretion of the typhoid bacillus by the carriers under observation. Support of this contention is afforded by the results, to which I have already referred, obtained by Semple and Greig from daily examination of the excreta of enteric-fever convalescents, which led to their finding a much higher percentage of carriers than has been recorded by other observers.

If the excretion of the typhoid bacillus by the carrier be regarded as a danger to others, these phenomena of intermittency in its discharge raise a question of considerable difficulty. For it is apparent that an enteric-fever convalescent may seem, as indicated by bacteriological examination during and shortly after convalescence, to have ceased to excrete the bacillus and may yet, at a later stage, develop into a carrier. That this may be so is shown by the results of the late examinations made by Kayser, to which I have before referred, of former enteric-fever convalescents. It will be remembered that this late examination led to the discovery of three carriers in addition to the three that had been detected by earlier examination. One of these cases, that of a woman, may be cited as an illustration. This woman had enteric fever in July, 1904, and, consequent upon two examinations of her excreta yielding negative results in August, was discharged from hospital. Nevertheless, in December of the following year she was found to be excreting the typhoid bacillus. It is no doubt possible that in this and in similar cases daily examinations, as practised by Semple and Greig, might have led to the detection of this carrier condition while the patient was still convalescing, but a control on this scale is not practicable in ordinary circumstances. It does not, however, follow that examinations conducted on the customary scale have not their value; but it is clear that their value is diminished by occurrences of the kind recorded.

Some evidence as to how far the carrier is to be regarded as more or less continuously an infective agency is afforded by recorded observations of the occasions on which carriers have infected others with whom they have been in relationship. To certain of these observations I now refer.

The well-known Brentry carrier became an inmate of the certified inebriate reformatory of that name in April, 1906, and was employed in kitchen work. No case of enteric fever had occurred in that institution since 1899 until September, 1906, when a woman employed in the kitchen was attacked by the fever. On October 13 the carrier was entrusted with the dairy work, which she continued to perform until November of the year following, during which period a number of cases, traced to infection through the medium of the milk, occurred. In November, 1906, the month following that in which the carrier came to be employed in the dairy, three cases occurred, after which there were no more cases until May, 1907. No case occurred in June, but there were further cases in July, August, September, October, and November. There was, therefore, during the thirteen months that this carrier was continuously employed in serving out milk from the dairy a period of six months during which no cases of enteric fever occurred in the institution.

In a report recently issued by the Local Government Board, Dr. R. W. Johnstone gives an account of enteric fever that occurred in Folkestone and its neighbourhood from 1893 to 1909, in relation with a carrier. This carrier had been employed as a cowman and milker on several farms in succession, from which milk was supplied to persons living in Folkestone and its vicinity during these years; and the fact that enteric fever appeared in turn among the customers of the successive farms on which this man worked was ultimately explained by the discovery that he was a carrier. During the period 1896-1909 no less than 64 per cent. of the cases of enteric fever that occurred in Folkestone were persons who took milk from a farm on which this carrier was employed. The number of cases attributed to this carrier year by year during the period 1893-1909 is as follows:—

1893	...	1	1899	...	68	1905	...	—
1894	...	3	1900	...	38	1906	...	2
1895	...	4	1901	...	12	1907	...	3
1896	...	17	1902	...	16	1908	...	5
1897	...	21	1903	...	7	1909	...	14
1898	...	14	1904	...	5			

Total for the period 1893-1909 ... 230

This instance is, so far as I am aware, the most striking hitherto recorded, both as regards the number of cases of enteric fever attributed to infection by a single carrier, and as regards the degree of continuity of carrier infectivity. And yet it will be observed that there were very great variations in the number of his victims year by year, which ranged between 68 in 1899 and none at all in 1905. No sufficient explanation of these variations is afforded by hypotheses of widely differing opportunities of his conveying infection to the milk, of variations in the quantity of infective material gaining access to the milk from time to time, or of differences in the degree of susceptibility to infection among consumers of the milk in different years. The carrier's steady association with the milk, the number of persons involved, together with the length of the period over which the occurrences of fever extended, are not consistent with adequate explanation by the considerations mentioned. Nor were the variations in the quantity of the milk supplied in different years enough to suggest that in this way the differences in annual incidence of the fever could be accounted for. It appears, in fact, that these differences must have been mainly due to the carrier having been not continuously but intermittently infective.

The case reported by Soper in 1907 may also be referred to in this connexion, although the history of this carrier is incomplete. The first known occurrence of enteric fever attributed to this carrier was in 1900, when there was a case of the disease in a family to which she was the cook. It is notable that she had been cook to the family for three years before this occurrence. In her subsequent employment as cook elsewhere, she gave rise to enteric fever in six other families, in the following sequence: in 1901, one case; in 1902, nine cases; in 1904, four cases; in 1906, seven cases (in two families); and in 1907, two cases.

The carrier reported by Dr. J. R. Hutchinson in 1908 appears to have infected, in the course of eleven years, seven persons in the following sequence of time: one in 1898, two in 1905, two in 1906, one in 1907, and one in 1908. This carrier, Dr. Hutchinson states, was a widow who got her living by taking in lodgers, of whom she usually had two or three; she did all her own housework. It will be observed that after the occurrence of the first case of enteric fever in 1898, an interval of seven years elapsed without any further occurrence of fever traceable to this woman.

Huggenberg reports a case in which, between 1886 and 1908, 12 cases of enteric fever occurred in, or in relation with, a family the mother of which, who had had the fever in 1877, was a carrier. Her husband contracted the fever in 1886, but no further case occurred in the family for six years, when, in 1892, a son was attacked. Nearly seven years elapsed without the occurrence of another case. Two cases then occurred in 1899 and one case occurred in 1900. There was no case in 1901, but cases, six in all, occurred in 1902, 1903, 1904 and 1905. Nothing further happened until 1908, when another case occurred. This led to an examination of the excreta of members of the household, when the mother was found to be excreting the typhoid bacillus. If this woman is to be regarded as the source of the fever, there were intervals of seven, six, and three years during which she does not appear to have infected others. Huggenberg's case suggests that there may be periods, even of years, during which the infectivity of the carrier remains at a relatively high level. There appears, in his case, to have been such a period from 1902 to 1905. A similar suggestion is conveyed by the figures I have quoted regarding the Folkestone carrier, and by the facts of the case reported by Dr. Hutchinson. In the Folkestone case, the infectivity of the carrier appears to have increased during the years 1896, 1897, and 1898, and to have attained its highest point in 1899 and 1900, after which it rapidly diminished, but again increased, after a lapse of six years, in 1909. The case recently reported by Dr. Meredith Richards seems to point in the same direction. In this instance a case of enteric fever occurred in a country house in 1903, but was not followed by any others until the autumn of 1908, when three cases occurred, and the early part of 1910, when there were two more cases. The fever was attributed to a milker, found to be a carrier, who was employed in this capacity for the household from 1902 to 1906, after which he left the neighbourhood, but returned and resumed the same work in October, 1908, just before the occurrence of the second group of cases. It is noteworthy, as regards this man's apparent intermission of infectivity during the four years 1904-7, that from the autumn of 1906 to the autumn of 1908 he was employed in a dairy farm of considerable importance without, apparently, any untoward results to its customers.

These particulars as to the occurrence of periods during which carriers do not appear to infect others may be regarded as casting a certain amount of light on the degree of infectivity of persons in this condition, in so far as they show that the infectivity may be absent for varying periods, sometimes of long duration, and that, accordingly, the amount of infectivity is diminished. They do not, however, necessarily help us to arrive at a conclusion as to whether the quality of this infectivity is high or low.

The Folkestone case, to which I have referred, may be cited as an instance of a carrier apparently possessing a notable quality of infectivity, since he was held responsible for 64 per cent. of the enteric fever that occurred in that town during a period of fourteen years. But this instance, and some other instances that might be quoted, are examples of particular individuals where high infectivity probably led to their ready detection, and who are not therefore to be regarded as necessarily representing the condition of the average carrier in this respect.

It might, in contrast to these individual instances, be contended that the reversion of Worthing, Maidstone, and Lincoln, after an epidemic of enteric fever, to much the same degree of prevalence of the disease as had characterized these places prior to the occurrence of the epidemic, indicates that the infectivity of the average carrier is low, or even negligible. Indeed, unless it be assumed that carriers were not created in these places in the proportion apparently usual after enteric fever, as a result of the epidemic, this conclusion appears difficult to resist.

An argument of somewhat similar sort, tending to minimize the importance of the carrier as an infective agency, has been advanced by Linossier. He states that in 1909 nearly 100,000 persons visited Vichy, of whom, he estimates, about 10,000 were sufferers from gall-stone disease, who went there for treatment. In view of the recognized relationship between that disease and the carrier condition, many of these persons, he claims, must have been carriers; and he states that the conditions of life during treatment at Vichy are eminently favourable to the transmission of infection. Therefore, he says, the Vichy season should be the signal for an outbreak of enteric fever among the population there. Nothing of the kind, however, occurs, from which he infers that chronic carriers are not all equally dangerous, and that they are not a danger at all save in special circumstances.

Some assistance towards forming a conception of the degree of infectivity of carriers is afforded by consideration of the data furnished, as the result of investigation, as to the proportion of cases of enteric fever attributed to infection by carriers in the course of outbreaks or endemic prevalences of the disease. Particulars of this kind are given by Kayser for the town of Strassburg during a period of nearly four years (1903-1907), in the course of which 505 cases of enteric fever occurred in a population of about 152,000. Inquiry was made, in these cases, as to the source of infection, with the following results:—

26·7	per cent.	due to	milk infected by cases of enteric fever or by carriers.
14·6	,,	,,	infected water.
2·0	,,	,,	food poisoning (*Bacillus paratyphosus B.*).
19·0	,,	,,	contact with cases of enteric fever.
9·5	,,	,,	infection derived from carriers.
13·0	,,	imported cases.	
15·2	,,	unaccounted for.	

It will be seen, therefore, that 9·5 per cent. of the cases of enteric fever were definitely attributed to infection derived from carriers, to which amount has to be added an unstated proportion of the cases infected through the agency of milk. It is noteworthy that, excluding attacks due to milk infected either by a case of enteric fever or by a carrier, 19 per cent. of the total number of attacks were referred to contact with cases of enteric fever as against the 9·5 per cent. referred to contact with carriers.

In the course of the investigations conducted during these four years, 28 carriers, amounting to 0·18 per 1,000 of the population, were discovered in Strassburg; of these 28 persons, nine proved to be chronic carriers (0·06 per thousand of the population). Of these 28 carriers, 11 are stated to have given enteric fever to others, nine were regarded as having probably done so, and eight apparently did not. Particulars are also given by Forster as to the proportion of enteric fever due to carrier infection in Strassburg during the eighteen months January, 1906, to June, 1907. During that period 386 cases of the fever occurred in Strassburg, in 77 of which (20 per cent. of the total) the infection is stated to have been derived from carriers. But, in these figures also, a still higher proportion of fever is attributed to contact with the sick. The disease is stated to have been contracted in this way by 117 persons (30 per cent. of the total).

Statistics on this subject, compiled from observations recorded in the course of the campaign against enteric fever in South-Western Germany, are given for the year 1906 by Frosch. The total number of cases of enteric fever was 2,080, as regards 1,102 (53 per cent.) of which the source of infection was not ascertained. Of the remaining 978 cases in which the source of infection was ascertained, 49 (5 per cent.) were attributed to contact with carriers, and 593 (60·6 per cent.) to contact with persons suffering from enteric fever. Of the total of 2,080 cases, including those in which the source of infection was ascertained as well as those in which it was not, the proportion attributed to carriers was 2·35 per cent., and that attributed to contact with the sick was 28·5 per cent.

Objection has been taken to these figures given by Frosch, on the ground that the very large proportion of cases in which the source of infection was not discovered seriously diminishes their value. It has been represented that closer and more sustained investigation would probably have resulted in the tracing of more cases to carriers, leading to a material increase in the proportion of persons ascertained to have been infected in this way. It might perhaps be maintained, in reply, that such investigation would be likely to lead to discoveries increasing the share of agencies of infection other than carriers in perhaps as great, or even greater, proportion. It is true, nevertheless, that certain investigators, who have advanced the first-mentioned view, have obtained, as the result of their researches, a much higher proportion of cases referred to infection by carriers than that noted in any of the foregoing figures that I have quoted. In support of this view may be cited the investigations of Schumacher into the prevalence of enteric fever in a village of some 1,750 population, where 45 cases had occurred in the course of the six years 1903-8. The original inquiries instituted as to the causation of the fever had resulted in four being regarded as imported cases, in 20 being referred to contact with persons suffering from enteric fever, and in the source of infection remaining unexplained in 21 instances. With the aid of bacteriological examinations on a large scale, Schumacher re-investigated the matter, with the result that he believed himself able to account for all the 45 cases. He considered six to have been imported cases, 20 to have been due to contact with persons suffering from enteric fever (the same number as ascertained in the original investigations), 12 to have been due to contact with carriers, and seven to have been due to contact either with carriers or with cases of the fever. The previously unexplained cases therefore were, in his judgment, due almost wholly to contact with carriers or sick persons, and mainly to contact with the former. The proportion definitely attributed by him to carrier infection was 26·6 per cent. The proportion, it may be noted, referred to contact with cases of the fever (44 per cent.) was still higher. These data of Schumacher's, however, cannot be accepted as affording a satisfactory basis of deduction, owing to the smallness of the figures involved. Ampler data, however, pointing to the same conclusion as that suggested by that of Schumacher, are given by Mayer regarding the prevalence of enteric fever in the Bavarian Pfalz in the four years 1904-7. Mayer's figures relate to 495 cases of the fever, of which he attributes 160 (32·3 per cent.) to infection derived from carriers.

It appears, therefore, that the results recorded by different observers as to the proportion of enteric fever referable to infection by the carrier vary greatly. These variations may, as has been suggested, be due in some degree to differences in the amount of care exercised in conducting inquiry and to the extent of bacteriological research concurrently carried out. It is, however, perhaps at least as probable that the variations may also in part be accounted for by the likelihood of different communities containing differing proportions of carriers, of there being a greater proportion of highly infective carriers in one community than in another, and by the varying facilities for the transmission of infection in populations differing in the cleanliness of their surroundings and in their conditions and habits of life. Perhaps, also, in weighing evidence of this kind, the personal equation of the observer has to be borne in mind; for the greater the degree of belief he has in the potency of a particular agency of infection, the more readily is he likely to be satisfied as to the importance of the part it has played.

These complicating considerations render it difficult to arrive at a definite conclusion as to the degree of infectivity characterizing the average carrier. Indeed, the fact that would seem most clearly established by the statistics quoted is the greater importance, in the transmission of enteric fever, of the person suffering from the disease than of the carrier. These persons, whether in the early stage of sickness, before the malady has been diagnosed and isolation has been secured, or whether entirely escaping detection during their illness, are constantly recorded as having been responsible for a considerably larger share of enteric fever than are the carriers.

The remaining question of interest concerning the degree of danger which the carrier constitutes to the community is whether he gives rise to a type of enteric fever in others as severe as that due to infection derived from a person actually suffering from the disease. This question I do no more than refer to briefly, inasmuch as I am not acquainted with statistics on a sufficient scale as to the relative fatality of enteric fever among persons infected by carriers and by cases of enteric fever respectively. Instances of the fatality of the fever due to individual carriers may be quoted, as, for example, that of the Brentry carrier, who was responsible for 28 cases in that institution, of whom two died. In a previous outbreak, due to the same carrier, in a home, there were 26 cases of enteric fever, in addition to eight cases of illness suspected to be enteric fever; two of the cases died. Of the 31 cases of enteric fever reported by Dr. A. and Dr. J. C. G. Ledingham as due to carrier infection, nine died. Individual instances such as this, however, do not suffice to indicate what is the degree of fatality of enteric fever due to infection derived from the average carrier.

If I have given a correct account of the epidemiological data we possess regarding the enteric-fever carrier, and if I have attached due weight to the consideration these data involve, we are, it seems to me, driven to the conclusion that we are not yet in a position to say how far the carrier is likely, as a general rule, to be a danger to others. There are, on the one hand, many instances in which carriers have been shown to have been responsible for a serious amount of enteric fever, and even, as in the case of Folkestone, for the greater part of the fever in the district over a period of many years. There are, on the other hand, examples of carriers who would seem to have been the source of little or no infection. Consideration of individual instances, therefore, does not materially help us towards decision. Nor do the general data that I have quoted justify definite conclusion on the subject, because of concomitant uncertainties that debar their unqualified acceptance. The statistics as to the proportion of enteric fever attributed to carriers vary so widely as not to permit general inference of a satisfactory kind. It is possible that the higher proportions, claimed to have been the result of more assiduous investigation than usual, are the more accurate; but it may be objected that these were exceptional instances, not necessarily illustrative of average conditions. But certain statistics, although not lending themselves to absolute conclusion on the general question, do appear to warrant one of two alternative inferences. The figures that I have given regarding the relative prevalence of enteric fever in Worthing, Maidstone, and Lincoln, before and after the epidemic of the disease in each of these places, indicate either that the epidemic did not result in the addition of a material number of carriers to their populations, or that, if such addition did occur, the influence of the carriers for harm was a negligible quantity. In like manner the data as to the proportion of carriers to population in Washington necessarily lead to one of two conclusions: either their proportion is greatly less in most communities than in Washington, or the carrier danger is small. For, on the Washington basis, London would at present have, as already stated, over 14,000 carriers; yet, in 1908, there were in the whole of London but 1,357 known cases of enteric fever. It would appear, therefore, that even if all these cases be assumed to have contracted their infection from carriers, the average London carrier takes ten years to produce one case of enteric fever.

In the absence of sufficient evidence as to the degree of danger that the carrier constitutes to others, it was to be expected, and it is the case, that there are some who attach less, while there are others who attach more, importance to this source of enteric fever than the known facts justify. These differences of attitude were exemplified in the course of a long debate, extending from November, 1909, to March, 1910, and occupying many sittings, in the French Academy of Medicine.[1]

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[1] *Bull. Acad. de Méd.*, Par., 3me ser., 1909, lxii. and 1910, lxiii.

While some of the speakers ranked the carrier danger high, others held it in small esteem; one of the latter, Dr. Delorme, maintaining that infectivity in the carrier is an exceptional occurrence. At the conclusion of the debate a resolution was put to the effect that the chronic carrier plays a part in the production of endemic and epidemic enteric fever, only exceptionally. The resolution was rejected; but by no more than 18 votes against it to 15 votes in its favour. The judgment likely to find favour with most is perhaps that expressed, in the course of the debate, by Dr. Vincent, who said that a new conception, at the outset, rarely escapes both extreme partisans and impassioned detractors, and that the truth lies between.

It hardly needs to be said that over-estimation and under-estimation of the carrier factor in the causation of enteric fever are both to be condemned. Over-estimation of the carrier danger may not only lead to biased investigation of enteric fever outbreaks, with the consequent risk of overlooking other agencies of the fever, but it may also lead, and has in fact been known to lead, to the imposition of unnecessary and unjustifiable restrictions upon the unfortunate carrier. Soper's carrier cook, for example, known as "Typhoid Mary," has, ever since the discovery of her condition in 1907, been detained in an isolation hospital: a procedure so unnecessarily stringent as almost to suggest panic on the part of the public-health authorities concerned. Under-estimation of the carrier danger, on the other hand, conduces to overlooking or insufficiently investigating the possibility of this source of enteric fever. It has been a common experience of those who have had much to do with the investigation of enteric fever that even when the transmitting agency of the fever was clearly indicated as being water, milk, or food, the source of the infection that had gained access to the medium of transmission could not be discovered. With the knowledge that we now have of the carrier, the likelihood of making that discovery is enhanced; and for this we owe a debt of gratitude to the bacteriologist. There is, indeed, in the recent history of enteric fever, nothing that has yielded a greater advance in our knowledge than the co-operation of the bacteriologist with the epidemiologist; and it is much to be desired that means for co-operation of this kind should be readily available in those places in this country where they are now difficult of attainment or entirely lacking.

Yet, grateful as we well may be, and as we are, to the bacteriologist for what he has told us, we should be still more grateful if he would tell us something more. For while he has added to our knowledge, he has also increased our administrative difficulties. When and as we find our carriers, we have to ask ourselves, in each instance, what are the measures best adapted to obviate the risk of infection to others?—a question that cannot be satisfactorily answered in the absence of knowledge of the potentialities of the particular carrier. For it would seem that some carriers may be at most times, if not always, a danger; others may be dangerous only on occasions separated by intervals, often of long duration, of freedom from danger; while yet others may never be dangerous at all. Measures, accordingly, that might be necessary and appropriate in the case of carriers of the first class, would be unnecessary and vexatious in the third class. What, therefore, we would have the bacteriologist tell us is how we are to distinguish our types of carrier, so that we may know how to deal with each appropriately. In the search for the carrier, the detection of the *Bacillus typhosus* is accepted as proof of his condition; but it would seem that some types of this bacillus are harmful, others are harmless, while yet others may be in one or other of several intermediate grades. Are these variations due to the presence or the absence, as the case may be, of some other micro-organism influencing the action of the bacillus; and, if so, what is that micro-organism? Or, if they are due to other conditions, can we be enlightened as to the nature of these conditions? These are problems for the bacteriologist, the solution of which will be welcomed by the epidemiologist.

Pending the solution of these problems we may take comfort in the reflection that, for many years past, and particularly in recent years, there has been a steady decline in the mortality from enteric fever in this country. The death-rate per million from this disease has fallen from 322 in the seventies to 198 in the eighties, and to 174 in the nineties. The decrease has been even more marked in the present decade, and touched the lowest point hitherto recorded in 1907, when it was 67 per million. Doubtless the carrier has been with us all these years, and it is interesting to reflect that the measures taken against enteric fever, although based essentially upon the conception, now generally rejected, that the infective organism of the fever, which was classed as a filth disease, lived and throve outside the human body, were nevertheless measures which must have greatly diminished the chances of infection by the carrier. The provision of pure drinking water, the endeavours, in considerable degree successful, to improve the conditions of milk supplies, and the decrease in dry conservancy systems of excrement disposal in towns, have materially lessened the number of channels by which infection derived from the carrier, among other sources, could give rise to outbreaks of enteric fever on the larger scale. It may also reasonably be contended that the danger of contracting the fever from food infected by a carrier or from contact with him has been diminished as an indirect result of the improvements that have taken place in the scavenging, paving of yards, and other conditions that go to secure cleanliness in the neighbourhood of the dwelling. For clean surroundings of the home tend to promote cleanliness in the interior; a clean interior, in its turn, encourages habits of cleanliness in its occupants; and our chief defence against the risk of infection by the carrier lies in cleanliness on his part and on our own.

Epidemiological Section.

November 25, 1910.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

Some Points in Relation to Tuberculosis and its Prevention.

By J. EDWARD SQUIRE, C.B., M.D.

TWENTY-TWO years ago (January, 1889) I had the privilege of reading before the Epidemiological Society a paper on the "Prevention of Consumption." At that time few people thought of the disease as preventible; now hardly anyone doubts that it can be prevented. In these twenty-two years much has been added to what we may term the intimate knowledge of tuberculosis, chiefly pathological details; in the essential characteristics of the disease little has been added, nor have we had to modify our views of causation to any important extent. The paper I brought before this Society over twenty years ago would require important additions to bring it up to date, but what is there written would require little modification if it were to be republished to-day.

When we look to what has been practically accomplished towards the prevention of tuberculosis in the quarter of a century during which we have possessed the knowledge that this great scourge of civilized man is a preventible disease, and have been aware of the main facts on which prevention must be based, there must be, I think, a sense of disappointment, though there is also much which justifies hope for the near future. We see, indeed, a diminished death-rate, and, what is also important, a *progressive* diminution in the mortality from tuberculosis; but this decline was evident for several decades before the discovery of the *Bacillus tuberculosis* showed us the causation of the disease. This fact indicates that the diminution in the mortality from tuberculosis may be attributed to the general improvement in the sanitary and hygienic surroundings and habits of the people rather than to any special measures directed against this disease in particular. Nor has there been any deviation from the steady progressive decline of the tuberculosis death-rate to indicate that our more exact knowledge of causation has resulted in any marked effect on the mortality from the disease. It is time that we inquired seriously into the reasons for this apparent failure of preventive medicine to produce any marked effect on the prevalence of a disease which destroys so many in the prime of life, and which is admittedly preventible.

The question here forces itself upon us whether there has, in fact, been any notable diminution in the *prevalence* of tuberculosis, or whether the lessened death-rate may not be largely attributed to a diminished case mortality; whether it is due to prevention of the disease or to more successful treatment? It is probable that both these factors are concerned in the result. I do not pretend to assess the relative influence of preventive and of clinical medicine in this matter. It is, however, a point which it might be interesting to follow out.

The mere statement of the query, however, suggests at once a matter of great importance, though admittedly of some difficulty. We have the figures of the mortality in relation to the population, but we have no complete knowledge of the case-mortality of tuberculosis. Without complete notification of cases we must remain uncertain whether or no the prevalence of the disease has diminished. True, we have evidence of a saving of life, whether this is due to prevention or to more successful treatment; but no one who knows anything of tuberculosis—its mode of spread and its effects, physical and economic, on the affected individual—will contend that it matters little which of these causes has mainly contributed to the result. Every case is a possible starting point of other cases, and the cured consumptive has probably been the source of incalculable harm to the community. When also we consider the long duration of the disease, the prolonged incapacity of the invalid, and the economic waste caused by the disease even when recovery ultimately ensues, no one can allow that a life saved by treatment is equal to one saved by being protected from becoming affected.

Without universal notification throughout the kingdom we cannot know either the incidence or the case-mortality of the disease. The many objections to compulsory notification of tuberculosis cannot be discussed here; they are in great part difficulties in connexion with administrative action which should follow notification, rather than objection to notification *per se*. A solution of the problem might be found temporarily in the suggestion I put forward at the annual meeting of the British Medical Association at Sheffield in 1908,[1] that notification of cases of tuberculosis should be made compulsory throughout the kingdom—the results being furnished to the Registrar-General's Department for statistical purposes—but that it should be left to each local authority to decide what, if any, action should be taken in the way of administrative control of the tuberculous. This would allow the various local health authorities the same freedom as at present to formulate regulations for the control of tuberculosis in accordance with the needs and the resources of the locality, whilst insuring a general return of all cases of tuberculosis for statistical investigation.

Next in importance to obtaining all possible information about the disease comes the need to disseminate information amongst the people, without whose intelligent co-operation no scheme of prophylaxis can be efficiently carried out. Much educational work in this direction has been done by the different health authorities throughout the country and by the National Association for the Prevention of Consumption and other forms of Tuberculosis. It was perhaps inevitable that the educational propaganda should at first have resulted in exaggerated alarm as to the infectiveness of tuberculosis: possibly it is necessary to frighten some people before they can be induced to take precautions. This alarm, however, tends to be prejudicial to those affected with tuberculosis, who may be looked on by the half-instructed as dangers to society. The "big poster" campaign which has just been inaugurated by the special appeal committee of the National Association may insure a wider appreciation of the nation's losses from tuberculosis, but it will undoubtedly increase the difficulty—already sufficiently serious—which the "cured" consumptive finds in obtaining employment, and may in consequence tend to prevent patients seeking early treatment for fear of becoming branded as consumptives. It is, perhaps, time that we insisted less on the infectiveness of the consumptive and more on the danger of the conditions which produce susceptibility—that we talked less about spitting and more about the hygiene of the home.

Beyond the educational work, health authorities and preventive associations appear to have focussed their activities on the establishment of sanatoria for the curative treatment of early cases. There is, undoubtedly, value in sanatoria. These institutions may be utilized as schools to teach consumptives how best to regulate their lives so that they may obtain the fullest advantage from treatment at home, and may perhaps then continue at work with the least risk of increasing the activity of the disease or of being dangerous to others. When so utilized, a constant succession of patients enters for a short stay, and results are not to be measured by the immediate effect on the patient's condition.

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[1] *Brit. Med. Journ.*, 1908, ii, p. 566.

The usual aim of the sanatorium is, however, the cure of consumptives, so that they cease to be centres of possible infection and are rendered fit to follow their employment with advantage to themselves and without danger to others. Towards this objective—which is an important, and perhaps essential, feature in an efficient scheme of prophylaxis—such sanatoria achieve little unless cases are admitted in the early stage of the disease and can be retained sufficiently long to allow of arrest being effected. There has been lately a considerable amount of criticism as to the efficacy of the sanatorium treatment of consumptives, founded on the statistics furnished in the reports of such institutions. The comparative failure of treatment which these statistics in some measure disclose is due partly to the large proportion of cases admitted with comparatively advanced disease, and partly to the difficulty experienced in keeping the patients sufficiently long under treatment to effect a lasting cure. Both these matters deserve comment. It is undoubtedly a fact that a majority of cases are not admitted for treatment sufficiently early: the reports of most sanatoria make complaint of this.

The chief reasons for this are (1) the disease is not recognized in its early stage; (2) when it is recognized, the patient does not at once commence systematic treatment.

The medical practitioner cannot be held responsible when the non-recognition of early tuberculosis is due to the patient failing to seek advice; but in the case of a disease of such importance to the public health, preventive medicine cannot be content to claim a like freedom from responsibility. Realizing, as every medical man should now do, how closely the success of treatment depends on the stage of the disease at which this is commenced, it surely becomes a duty to endeavour to find out the early cases and to persuade these to commence treatment without delay. Some inquiries by the National Association for the Prevention of Consumption, with which I have been associated, have shown how little is done in London to search out early cases.

During the last five years that I held the appointment of Physician to the St. Marylebone General Dispensary (1902—7), the Resident Medical Officer, Dr. Hulbert, made for me close inquiries as to all members of the families and fellow-workers of all the tuberculous patients, and persuaded a large proportion of these to come to the dispensary to be examined by me. A short report of this work may be found in the *Transactions of the International Congress on Tuberculosis* *in Washington*, 1908 (vol. iii). Such examination of "contacts" is almost an essential if we aim at getting early cases amongst the poor for treatment. It is carried out by the Tuberculosis Dispensaries—such as have been established in Edinburgh, in Paddington, and quite recently in Marylebone. The systematic examination of the children in the elementary and secondary schools should also be useful in detecting early tuberculosis, but it would be well to have some expert to whom suspicious cases could be referred for examination. In a few of the metropolitan boroughs a partial examination of contacts is carried out, only those who complain of illness or appear to be ill being examined. This is, in my experience, of slight value in detecting *early* cases.

Clearly one urgent requirement for the prevention of tuberculosis is the visitation of the homes of the consumptive with the view of obtaining an examination of contacts; such examination should be carried out by experts in chest examination, for the experience of all medical officers of sanatoria shows that the evidences of early tuberculosis in the lungs are easily overlooked. Of the delay in commencing treatment which is due to failure to detect the disease when the patient first seeks advice I shall say little. The wider one's experience of the clinical examination of the chest in health and disease, the less is one disposed to be surprised that the evidences of early tuberculosis are so often unrecognized. One is, however, I think, entitled to regret that the assistance of the clinical expert is now so often replaced by some form of tuberculin test, or by examining the sputum for bacilli which are not likely to be present until softening has commenced.

The delay in entering the sanatorium when the disease has been recognized may be due to (1) financial considerations—the loss of earnings when the patient has others dependent on him, or, in some cases, the expense of paying the sanatorium fees; (2) the large numbers seeking admission to special hospitals and free sanatoria and the consequent long waiting for vacancies; (3) the fear of being labelled as consumptive and the consequent difficulty of obtaining employment after discharge. These matters must be considered and provided against if we wish to grapple seriously with the tuberculosis problem. But the treatment of early cases, though a most important factor in prevention, does not constitute the whole requirement. We need homes of refuge for the advanced consumptive of restricted means: a place where he can be separated from close and continued association with susceptible individuals, whose susceptibility is progressively increased by the increasing poverty produced by maintaining an invalid; a place where his wants can be adequately attended to, where his remaining days are made less dreary, where his relatives and friends may visit him, and, above all, a home, where he may rest without being branded as a pauper living on charity. There should be no more disgrace attaching to the acceptance of a home and attention when disabled by illness than there is in accepting a pension when disabled by age. Such homes for advanced consumptives would set free many beds in the special hospitals, which would then be available for early cases, and their cost would probably be covered by what they would save the poor-rates. Labour colonies for chronic cases fit for restricted employment are needed, and might be made self-supporting. We need also provision for the support of those dependent on the consumptive whilst he is in the sanatorium—for want of which many early consumptives cannot give up work to undergo proper treatment, and many that enter the sanatorium cannot afford to remain long enough for cure. We need also some employment bureau for obtaining work for the "cured" consumptive on his discharge from the sanatorium, for many discharged patients break down under the hardships entailed by the unsuccessful search for employment.

Obviously the complete scheme for dealing with the tuberculosis problem is almost beyond the scope and ability of individual charities, and should be under State organization and control. Such a body as the National Association for the Prevention of Tuberculosis might do much to convince the Government that a scheme can be organized and worked by co-ordinating existing agencies throughout the country. A suggestion of this nature has been laid before the Council of the Association, which may, I hope, bear fruit. I would urge on all health authorities, hospitals, charitable and philanthropic agencies which concern themselves with the consumptive, to do all they can to further any scheme which seems to promise success in co-ordinating their efforts towards the prevention of this disease. But I must not detain you longer on general considerations of prophylactic policy. I am not now attempting to work out the details of a comprehensive scheme for stamping out tuberculosis: this is not a task which any wise man would attempt single-handed, for there are too many interests involved. Let me rather direct your attention to a few special points on which my experience has led me to certain conclusions. There is a development of modern medicine which seems to me a rather unfortunate phase—namely, the undue reliance on so-called "tests" for specific diseases. I have elsewhere[1] expressed my doubts as to the reliability of the tuberculin tests in the human subject. Inoculation with tuberculin may in some cases be a useful addition to clinical examination, but surely few would agree that it can usefully replace the usual clinical methods.

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[1] *Lancet*, 1910, ii, p. 373.

One result of this reliance on the tuberculin test has been to produce a very exaggerated estimate of the proportion of the population which is affected with tuberculosis. It may be true that some 80 per cent. of the persons tested will react to the tuberculin test, but no one will agree that this proportion of the population is *suffering* from tuberculosis. Many who react never present any indications of the disease, or suffer any inconvenience or disability attributable to the implantation of tuberculosis. Such an attack of tuberculosis may well be disregarded, unless it can be shown that the affected individual is a danger to others.

This brings us to a matter which must be considered in connexion with the prevention of tuberculosis which has not as yet received much attention, and is perhaps not yet sufficiently realized by the majority of workers in public-health matters. We are now fully cognizant of the existence of so-called "typhoid carriers,"—individuals who, whilst apparently in good health, yet harbour in their bodies the causative micro-organisms of enteric fever, which they throw off in the excretions and thus may become the unsuspected cause of the disease in others. We are fully aware that during an epidemic of diphtheria there will be many persons who apparently escape the disease themselves, who yet have the bacilli in the secretion of the mouth and fauces, which, expelled by cough, may infect others. Every hospital physician will have noticed the many cases of follicular tonsillitis which come to the out-patient room when there is an outbreak of diphtheria in the neighbourhood, and most of them will have found the micro-organisms of diphtheria in swabs taken from the throats of these apparently harmless cases. I suggest to your notice that there are also tuberculosis carriers —individuals who, although they may not be in robust health, are yet not suspected of being tuberculous, but who yet are excreting tubercle bacilli and possibly infecting others. There may even be individuals who remain in good health who may nevertheless be carriers and disseminators of tubercle bacilli.

The "carrier" is evidently a person whose specific protective processes are not sufficiently active to immediately destroy the particular infective micro-organisms which may enter the body, but who is, nevertheless, so far immune as to escape illness from the infection. In this sense there may be—and I think there are—tuberculosis carriers, but there are also many cases where active tuberculosis is present but unsuspected.

To what extent the insusceptible members of a consumptive's household or of the staff of a hospital or sanatorium for consumptives may harbour tubercle bacilli and distribute them, I am not prepared to say, but I think the question is one which is worth further investigation.

Kayserling, of Berlin, in referring to the subject of tuberculosis carriers,[1] suggests that they give off no bacilli, but harbour them in the body for months or years, until some accident or illness renders the soil fertile and they become actively tuberculous and thus centres of infection. On the other hand, tubercle bacilli have been found in the mucus from the mouths of those in constant association with consumptives, and it is conceivable that these might be transferred—by kissing, for example—to others who might be susceptible to infection.

In connexion with the question of "carriers" it is interesting to bear in mind the various observations which have led to the belief in latent tuberculosis, the main fact being that infective (tuberculous) material may be present in the body without any pathological changes, either macroscopical or microscopical, being discoverable. The infectiveness of the material is established by the inoculation of animals. Amongst others Professor Francis Harbitz,[2] for example, in the records of the Pathological Anatomy Institute of Christiania, pointed out that a large proportion of the children examined were in this sense the subjects of latent tuberculosis. In most of the cases it was the lymphatic glands which were affected, but a similar condition may occur in other organs and tissues, even in the lungs. It may be, as has been suggested, that the bacilli in these centres of latent tuberculosis may have their virulence diminished; it may also be that, though virulent, their power for harm is counterbalanced either by the activity of protective agencies in the individual or because the bacilli are isolated and cut off from communication with the rest of the body. In any case, if this infective material were to gain entrance into some other individual whose tissues furnished a fertile soil, disease might result. We do not know how long tubercle bacilli may remain "latent;" Professor Harbitz suggests that it is only a few months, or at most a couple of years, but it seems long enough to offer sufficient possibilities of danger to others. Apart, however, from this conjecture, there is no doubt that many cases of tuberculosis of the lungs remain undetected and become sources of danger because their infectiveness is unsuspected.

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[1] *Tuberculosis*, Leipz., 1906, v, p. 240.

[2] "Untersuchungen über die Häufigkeit, Lokalisation und Ausbreitungswege der Tuberculose." Von Francis Harbitz. Christiania: A. W. Broggers. 1905.

I have several times drawn attention[1] to the cases of chronic bronchitis in elderly persons in whom tuberculosis has supervened on the bronchitis and, causing no new symptoms but merely an exacerbation of existing discomforts, remains undetected, though examination of the expectoration would demonstrate the presence of tubercle bacilli.

It is also important to recognize the fact, insufficiently appreciated, that many cases of "cured" or arrested tuberculosis of the lungs continue for months, or even years, to cough up—not continuously but from time to time—phlegm containing bacilli. These persons having been pronounced free from active disease, and being free from all feeling of ill-health, have relinquished the preventive precautions which they exercised during their illness, and become therefore unwitting dangers to others.

The foregoing considerations suggest the primary importance, from the point of view of prevention, of directing our attention to combating susceptibility; for possible sources of infection are innumerable, and can hardly be avoided since they may be unsuspected. Most of us are daily in contact with such sources of infection and probably receive bacilli into our bodies, but these produce no ill-effects whilst our protective agencies are vigorous. The healthy person has nothing to fear from infection, except from the rare accident of an overwhelming dose. Thus if we can retain the condition of resistance which appears to be natural to the healthy human animal, we need not shun the consumptive. The line of defence against tuberculosis which seems to promise the greatest measure of success is that which aims at increasing the resisting power of the individual who is exposed to infection. We are all agreed that the conditions which tend to break down the resisting power of the individual are to be found most prevalent in the homes of the poor. In these homes also we find a large proportion of the dangerous consumptives—the sources of infection. It is, then, in the homes of the poor that the war against tuberculosis must be fought out.

There is a natural tendency amongst officials of all kinds to require some numerical expression of the extent of a need before they can agree to consider the possibility of meeting it by regulations or enactments. Thus we require statistics showing the extent of the infectivity of tuberculosis, the proportion of contacts who become tuberculous, and the effect of inherited susceptibility as shown by a relatively larger proportion of infected individuals amongst the contacts belonging to the patient's family as compared with those not related to the consumptive. Reliable statistics on these subjects are, however, difficult to obtain, and we are still largely dependent on general experience unsupported by exact figures.

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[1] "International Clinics," 16th ser., 1906, iv. p. 90.

In a recent communication to the *Lancet*[1] the Right Hon. R. Farquharson, M.D., expresses grave doubt "as to the really dangerous infection of consumption." Of course much depends upon what is considered to be "dangerous infection." Since Dr. Farquharson proposes, as necessary prophylactic measures, the prevention of spitting, the disinfection of sputa, and the thorough cooking of meat and boiling of milk, it seems that he does believe in the communicability of the disease through inhalation and ingestion. It is true that over-insistence on the infectious nature of tuberculosis has caused the partially instructed to compare it with the well-known infectious diseases, such as measles and scarlet fever; whereas there is a very wide difference in the infectiveness of tuberculosis and that of the exanthemata. It would, perhaps, be more correct to say that there is a great difference in the susceptibility of the human subject to suffer from the infection of tuberculosis on the one hand and of measles and other exanthems on the other. Yet every one who has had any large experience of tuberculosis knows that many consumptives can trace the commencement of their illness to close association with an advanced case of consumption. Cases frequently present themselves where it is impossible to doubt that infection has spread from patient to nurse, from mother to daughter, or vice versa, from husband to wife, or from a consumptive clerk or workman to another working in the same room. I could give examples of all of these from my own experience if it were necessary to insist on what is the common experience of all who are intimately engaged in the treatment of consumptives.

In a record of the resident staff of the Mount Vernon Consumption Hospital, which I recently brought forward before the Medical Section of this Society, I showed that during the past fifteen years about $2\frac{1}{2}$ per cent. of the nurses and servants had become affected with tuberculosis whilst in residence at the hospital. These nurses and maids were unmistakably "contacts," but they were selected individuals living under conditions specially devised to prevent infection, and it is impossible to affirm that all became infected from the patients. Yet this report undoubtedly suggests that there is some risk of infection—small though it be—from continuous and prolonged association with consumptives even under favourable conditions. The influence of family predisposition or susceptibility showed itself in the fact that the proportion of cases of tuberculosis amongst the nurses and maids who gave a history of other cases of the disease in their families was just twice as great as amongst those with no family history of tuberculosis. The numbers were too small to be of much value, but we may find other evidence that some families are more prone to tuberculosis than others. For example, I have examined during the past year 900 death-claims of the Pearl Life Assurance Company (of which I am the consulting medical officer), and here also I find that the deaths from tuberculosis are more numerous amongst those whose family histories record other cases of the disease than amongst those with no such record. The figures are as follows: Total number of claims examined, 901; total deaths from tuberculosis, 140, or 15·53 per cent. Of these 901 claims, 53 gave a family history of tuberculosis—5·88 per cent.; of 848 with no family history of tuberculosis, 124 died of tuberculosis—14·62 per cent.; of 53 with other cases of tuberculosis in the family, 16 died of tuberculosis—30·18 per cent.; again showing an incidence twice as great amongst those with a family history of tuberculosis.

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[1] *Lancet*, 1910, ii, p. 224.

We are still without a numerical expression of the risk of infection under the conditions obtaining in the homes of the poor consumptive: but we shall, I think, not be far from the probabilities if we first double the percentage incidence of the hospital staff to represent the greater susceptibility of the relatives of consumptives, and then double this again to represent the extent of the increased risk due to absence of proper preventive precautions. This will give us 10 per cent. as representing the risk of infection; or, in other words, we should expect to find ten persons affected with demonstrable tuberculosis in every 100 contacts examined. This is a sufficiently large proportion to justify the examination of all who are in close association with consumptives in the poorer homes. I find, however, in the report of the Paddington Dispensary for the Prevention of Consumption that definite signs of pulmonary tuberculosis were found in over 20 per cent. of the contacts examined.

If we agree that the fight must be at close quarters with the disease in the places where it is largely bred and spread—i.e., in the home of the poor consumptive—we must first consider how we are to get into these homes. The poor are sadly over-visited and get accustomed to the intrusion of uninvited strangers, but they often resent the visit of officials who may claim admission as a right. For this reason there may be an advantage in making use of voluntary agencies to supplement the official sanitary inspectors or health visitors. The more complete is our knowledge of the number and location of the consumptives in any district, the better are we able to combat the spread of the disease: and information may be obtained by voluntary agents—such, for example, as district nurses, district visitors and church workers, local health societies, &c., &c.—from sources other than those appearing in the official returns of the medical officer of health. When the home is visited advice should be given as to ventilation, cleanliness, the disposal of sputa and the prevention of infection generally. All persons in close and constant communication with the consumptive should be examined occasionally to insure that they remain free from the disease, or that if they become infected this may be recognized and treatment commenced before the disease has become too far advanced. My experience in the examination of such contacts whilst engaged in this work under the Marylebone General Dispensary, as well as a large experience in the examination of children in the elementary schools, has convinced me that it is insufficient to examine only those contacts who give evidence of ill-health, as is done by the few metropolitan boroughs which take any steps towards the examination of tuberculosis contacts. I may also again insist that to test contacts with tuberculin alone is unreliable, and to wait for the detection of tubercle bacilli in the sputum is to miss most of the early cases and some of the advanced ones.

The organization for home visitation should also be in a position to offer any aid which may be necessary, such as extra nourishment, extra coverings, &c., or admission orders to a hospital or sanatorium or a convalescent home. It should, therefore, be in touch with the Charity Organization Society and all the various philanthropic agencies of the district. The machinery of the Medical Officer of Health's department is hardly sufficiently complete for this, so he seeks assistance of the local health society or association of public welfare, where such exists. It has been suggested that hospitals could do this visiting, and furnish the help that is required for the patients attending the institution: and in a few instances this has been attempted. There are obvious difficulties and disadvantages in this, at all events in London; for the patients of any hospital are drawn from too extended an area to be covered by any one agency, and patients keep moving from one dwelling to another and change from one hospital to another.

The organization for home-visiting must be founded on the basis of locality, and to be efficient as a prophylactic agency it should be in touch with all the tuberculous cases in its district. In London the division into metropolitan boroughs provides a convenient basis for such organization, which would be in close association with the medical officer of health of the borough, and should aim at getting to know of every case of tuberculosis in the borough, and the condition of the home, &c., thus being able to offer help, besides supervising the hygienic conditions of the dwelling.

Whatever organization undertakes the visiting of the homes, whether it be a hospital or a special anti-tuberculosis dispensary or the local health authority, its value as a preventive agent depends largely upon its visiting, supervising, and to some extent controlling as large a proportion as possible of the infected homes within the district. The object of the visiting authority, whatever its nature, should be to visit the homes of *all* consumptives living within the area covered by its activities. If prevention of spread of the disease is the objective, this agency cannot afford to have any infected home within its district which is not under its inspection. It is obviously not essential that the visiting authority should itself undertake the medical examination of contacts, nor the actual treatment of the consumptive individual, but it must be in close association and communication with the agencies and individuals who examine and treat. It should also be closely associated with the medical officer of health, to whom reports should be furnished of all cases. The medical officer of health (or rather the health authority which he represents) should have the right of entry for its officials, or persons delegated by it for this duty, to all dwelling and working rooms occupied or used by consumptives. He should have the power to remove a dangerous consumptive, and to retain in any institution any consumptive who, in consequence of his habits or of the conditions to which he proposed to return, was considered likely to be dangerous if discharged.

I am inclined to think that for London at least, and probably for all large towns, special anti-tuberculosis dispensaries, each responsible for a definite district, would at present best serve the purpose of home visitation. Such an institution would also co-ordinate all the agencies in the district for providing aid to the consumptive and getting admission to suitable institutions for the patient and contacts who seemed in danger of becoming affected. It would also provide for the examination of contacts and the treatment as out-patients of the affected. It is, however, impossible, even if it is desirable, to oblige all consumptives within the area to attend the dispensary as patients, since the consumptive, like any other sick person, has the right to choose where and by whom he will be treated. Nevertheless, the dispensary should be responsible for the supervision of all the centres of infection within its district; otherwise it is only partially protective, and is defective as a preventive agency for the district. It seems to me, therefore, essential that a local anti-tuberculosis dispensary should make provision for the visitation of the homes and workplaces, not only of its own patients, but also those of all consumptives living and working within the area of its control, no matter by whom the patient is being treated, unless such visitation is specifically undertaken by the doctor or institution under whose treatment the patient may be for the time being.

Many private practitioners working amongst the poor, and probably all hospital physicians, would gladly avail themselves of the dispensary organization for the hygienic control of the consumptive's dwelling, so long as they knew that there would be no attempt made to take the patient out of their hands. No doubt, also, practitioners or institutions might be glad to hand over certain of their consumptive patients to the dispensary for treatment as well as for general supervision.

I believe that this view of the duties of an anti-tuberculosis dispensary is not fully in accordance with the practice of such dispensaries as are at present in existence, but I feel sure it will commend itself to those who are interested in the broad question of prevention.

At present we must look to private enterprise to found and maintain such preventive dispensaries, but there is much to be said in favour of the local health authorities eventually undertaking the control of such institutions, and providing, maintaining, and working the anti-tuberculosis dispensary as part of the sanitary organization of the district for the health of which these local authorities are responsible.

If the attempt of individual hospitals to deal with the problem is unsatisfactory because they can only visit patients attending that institution, and because their patients are so widely scattered, anti-tuberculosis dispensaries will, in my opinion, fall short of the requirements for efficient preventive organizations if they only visit the homes of a proportion of the poor consumptives in their neighbourhood.

The medical inspection of children in our elementary and secondary schools offers great possibilities in the direction of improving the hygiene of the homes of the poorer members of the community, and in this way helping materally in the fight against tuberculosis. It is not in the personal examination of the children alone that medical inspection fulfils its greatest good. Even a superficial examination gives abundant evidence that much of the ill-health which prevents children from obtaining full benefit from instruction in school is due to home conditions. From the point of view both of treatment of these children and of prevention of such hindrances to education, it is essential that attention should be paid to the improvement of the causative conditions. The school nurses are instructed to visit the homes of children who are reported to be verminous, and in certain cases of ill-health, in order to impress on the parents the necessity for treatment. This home visitation might with advantage be greatly extended so as to include visits to instruct the parents in essential health requirements in their homes. Where the faulty condition of the child's home is found to be due to neglect on the part of the parent, there should be power to compel obedience to directions or to enforce penalties for disobedience. If the State undertakes the education of the child, it has a right to insist on the parents doing their part and, so far as in them lies, keeping the child fit to receive the instruction given.

At the same time the school ought to give instruction to the child in the essentials of hygiene; if each child has a right to be taught sufficient to enable it to earn its living in after-life, it surely should also be taught how to safeguard its health, without which renumerative work is almost impossible. To the argument that there is no room for this subject in the already crowded time-table of our schools, I would suggest that, after all, hygiene is of more practical utility than botany (for example), and that State-provided education should be organized so as to consider the ultimate good of the nation, and the welfare of the individual mainly in relation to the community of which he is a member. It is not enough to teach the child how to earn money for himself; education should also aim at making him a good citizen, and, after all, a healthy population is one of the valuable assets of a nation.

The inspection of school children is, of course, useful in making possible the detection of unsuspected cases of consumption, and thus in some cases insuring early treatment. It is also valuable in bringing under notice those children who, though not tuberculous, are yet by reason of general debility specially susceptible to infection. It is, however, not sufficient to recognize predisposition; measures should be taken to counteract or remove this predisposition. Open-air schools, country and seaside schools for town children, and convalescent homes are valuable in this connexion. It seems to me that a good deal more might be done in the direction of boarding-out town children in country villages where they could attend the local school, and thus have a few weeks or months in the country without interruption to their education.

We must, however, bear in mind that the hygienic condition of the cottage is not always satisfactory, and a knowledge of the essentials of healthy living should be taught to the villager as well as to the town dweller.

We must always be careful about sending consumptives to the villages; for, once introduced, the disease seems difficult to get rid of. I have recently stayed at a small village of 330 inhabitants, in a most healthy part of England, where one would hardly expect to find any cases of consumption. I am informed, however, that out of twenty-one deaths which have occurred in the last four years, seven have been from consumption. The schoolmaster, who has lived in the village for thirty years, says that he believes that there is not a single house in the village that has not had at least one death from consumption in it since he came to the place. In one house there have been six such deaths. It is difficult to explain how the disease became so persistently prevalent in this small community in so favourable a locality. Can it be that the cottages are all infected, or can we accept the suggestion that infection comes from the droppings of cows, which are never removed from the village street, and which may be derived from tuberculous cattle? I cannot express an opinion on the possibility of this latter theory. I believe that bacilli have been found in the fæces of tuberculous cows, and it is easy to conceive that, if present, they might be disseminated from dried excreta, as from dried sputa; on the other hand, we do not generally associate tuberculosis of the lungs with infection from bovine sources. The matter may, however, merit further attention and investigation, as also does the agency of flies, and possibly fleas, in the transmission of infection.

The possibility that the source of infection is to be found in the cottages suggests the necessity of efficient disinfection of rooms occupied by consumptives who are expectorating sputum containing bacilli. This is, I believe, generally carried out in London, when a room is vacated by an advanced consumptive; in one metropolitan borough disinfection is carried out periodically whilst the room is occupied by the patient. This latter practice seems desirable, and the clothing and bedding should also be disinfected. Disinfection, to be effective, should be carried out or supervised by a competent individual. I was told some little time ago of a consumptive in a country district who for some time before his death was unable to go upstairs to his bedroom, and died in the sitting-room downstairs, where he had spent the last few weeks of his life. Disinfection was carried out by the local "road-man," who burned a sulphur candle in the old man's bedroom, which he had not occupied. I cannot vouch for the accuracy of this tale, which, however, is at least *ben trovato*.

I am afraid that I have occupied a great deal of your time without having added anything to your knowledge of the subject of tuberculosis and its prevention. I have attempted to draw your attention to certain aspects of the problem rather than to dogmatize, or even to explain the points I have raised. The value of such a paper as this—if value there be—rests not so much on the personal opinions expressed in it, as in the sum of the opinions which it may be the means of eliciting in discussion. The expression of one's thoughts challenges thought in others, and may thus lead to valuable results. I ask you, therefore, to give a value to this paper by discussing and criticizing it.

DISCUSSION.

Dr. THEODORE WILLIAMS said he felt that he would like to be present and hear this paper, because he had heard others by Dr. Squire and derived many hints from them; moreover, the author and himself had recently been engaged together on an interesting report on the need for anti-tuberculosis dispensaries in London. His criticism of the paper was that it was too diffuse and embraced too many topics. He had hoped it would have dealt with prevention alone, because he agreed with the author that a life saved by prevention was quite equal to a life saved by treatment. The author said that every case was a possible starting point for other cases, and that the cured consumptive had probably been a source of incalculable harm to the community. He (Dr. Williams) could not admit that, for he held that the great mass of cases of consumption were not contagious, though some acute ones were; also those who had large cavities teeming with tubercle bacilli. Numbers of cases in which the disease was limited, and with little or no expectoration, were not infectious at all. Patients should always be told to be careful about the sputum and to take other sanitary precautions. Great stress was laid on advanced cases causing infection, but he confessed he had seen very advanced cases on the Continent living in bedrooms where no attention was paid to sanitary matters, where husband and wife slept in the same bed with little or no ventilation, and therefore with every opportunity for the communication of the disease, yet no infection took place, though every now and then cases of unquestionable infection did occur. Dr. Squire seemed to have discovered more cases of infection than he (the speaker) did, though he had often sought for them. When he first joined Brompton Hospital, which of course was before the discovery of the tubercle bacillus by Koch, every clinical assistant was trying to detect cases of contagion, and often at his visit he had three or four cases of supposed contagion to investigate, but they seldom passed muster. He approved of the system of visiting the homes of consumptive patients, and he regarded it as somewhat of a blot on the London consumption hospitals that that was not undertaken at an earlier date, for several of the hospitals had an instruction to that effect in their regulations, which had practically never been carried out. It was effectively done in Edinburgh. He would warrant that the number of cases due to contact, if examined by an independent and competent person, would be found to be very few. Part of the Edinburgh system was to have a young medical man living in or near the dispensary, whose duty it was to visit the patients. Could one imagine a worse training for a young man than to sit in a dispensary and see nothing but consumptive cases, the rest of his time being occupied in visiting them at their homes, with no opportunity of investigating other forms of chest disease, and out of touch with general diseases? He would be sorry to recommend any of his young friends to take such an appointment. He agreed there should be hospitals for advanced cases. In Edinburgh such cases were transferred to one of the infectious hospitals. He did not believe in sending them to homes for the dying, as nothing was done for them, and under good hospital treatment many of them recovered sufficiently to be admitted into sanatoria. Such cases at Brompton often yielded good results. With regard to consumption carriers, he felt that, if what Dr. Squire said was correct, he (Dr. Williams) and other physicians must have been very guilty in carrying about germs, though he was not aware of it, and hoped he had not infected anybody. According to Dr. Squire, everyone who had to do with consumptive patients should undergo constant disinfection. The question of infection was largely a matter of the care of the sputum and of the cleanliness of linen. To sum up, infection was possible but was rare, and the danger of infection had been largely exaggerated, greatly to the detriment of the poor consumptive.

Mr. M. GREENWOOD, Jun., said he felt more confidence in speaking on this subject after hearing Dr. Theodore Williams, because when those who were working at statistics brought forward evidence which seemed to suggest that the factor of predisposition was more important than the factor of infection, they were always met by the statement that statisticians had no clinical experience. Those who saw many cases of tuberculosis, it was said, *knew* that direct infection was a common occurrence. The effect of direct infection, he understood from so great an authority as Dr. Williams, was neither easy to demonstrate nor frequent in clinical experience. Statistical work suggested that the importance of direct infection alone had been exaggerated. The statistical evidence on the subject was not very large, but the investigations of Karl Pearson[1] and others supported the view that there was an inherited predisposition; and he did not think one would be far wrong in saying that the intensity of that inheritance was not very different from the intensity of inheritance of normal physical characteristics of man so far as they had been inquired into. One would not expect to get such a high correlation between the ascendants and descendants in the records of insurance companies as from the mass of the people, since the former were not taken at random—there was a selective process. He could not find, from any statements in the paper, whether the persons with a tuberculous family history were persons whose parents were tuberculous, or whether it was a question of *any* ascendants being tuberculous. One could not from the figures determine whether it was direct inheritance or simply collateral inheritance. But even from these figures, reducing them to a correlation co-efficient, one found that the value was significant. Further, if it were true that the inheritance of the tendency was of more importance than exposure to direct infection, one should find, in measuring the association of the presence of a disease in persons living under a common environment, like husband and wife, that the intensity of the relationship would not be high. If increased opportunities of infection were relatively unimportant, one would find that the correlation between husband and wife was not far from the value obtained between husband and wife for any physical character. Because if the tendency to develop tuberculosis were inherited, and mating in man were largely assortative, there would be some relation between husband and wife, and it should be of the same order as the known values of assortative relationships in other characters. He had analysed the statistics of German sanatoria,[1] and they showed that correlation between husband and wife with respect to pulmonary tuberculosis was of exactly the same order as the relationship between husband and wife for other physical characters. People said these statistics were bad, that they were vitiated by a number of errors, and that they were unreliable. If that objection were followed out to its logical conclusion, one had to assume that the statistics were vitiated in such a way that, by pure chance, they produced almost the exact value of the relationship between husband and wife which might have been predicted on the basis of assortative mating. He found it difficult to conceive that statistics hopelessly vitiated by error would yield such values. For those reasons, and in view of the similar results obtained from statistics of insanity where infection did not arise, it seemed to him that the evidence before them was that the inheritance of a tendency was of much more importance in the development of the disease than the presence of unusually great opportunities of infection. It ought not to be, but apparently was, necessary to remark that those who accepted this view did not deny the patent fact that tuberculosis could be communicated by experimental infection. They merely held that *soil* was of such paramount importance that even a large diminution in the quantity of available *seed* was unlikely to produce the effects claimed by many enthusiasts under ordinary conditions of social life. He only put it forward as a matter of discussion, because if it were true, then, with regard to diminishing the prevalence of tuberculosis in the general population, the measures advocated by Dr. Squire, which were very extensive and presumably costly, must fail. They might do good in other directions, but they would not forward that object. If his (the speaker's) position were arguable, their first efforts should be to collect much wider experience and larger data than at present existed to determine the preliminary question. There was a distinct case in favour of the view which he had just advocated. Until that view was conclusively disposed of, there was, in his opinion, no justification, when addressing the laity, for urging the adoption on a large scale of measures of the kind proposed by Dr. Squire.

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[1] Pearson, "A First Study of the Statistics of Pulmonary Tuberculosis," Dulau & Co. 1907; Goring, "On the Inheritance of the Diathesis of Phthisis, &c.," Dulau & Co., 1909.

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[1] Greenwood, *Proceedings*, 1909, ii (Epid. Sect.), p. 259; *see* also Pope and Pearson, "A Second Study of the Statistics of Pulmonary Tuberculosis, &c.," Dulau & Co., 1908.

Dr. DUDFIELD presumed that Dr. Squire, in speaking of notification of "tuberculosis," meant notification of the pulmonary forms of the disease. In that case he thought it would have been better to use the popular term "consumption." Certain authorities advised that notification should be restricted to "open tuberculosis"—to use continental phraseology—but he desired that notification should apply to all forms of "consumption" at least. Without a complete knowledge of the prevalence of the disease, it was impracticable to formulate preventive measures with any hope of success. Although there had been a marked decline in the total mortality from pulmonary tuberculosis, he had some doubts whether all ages had shared in that reduction. His experience suggested that, at the ages at or just after the prime of life, there had been an increase during the past few years. Owing to the now well-established fact that a tuberculous infection might occur in childhood, and leave centres of latent infection which became at later ages active foci of infection, it was difficult to determine the duration of the disease in any case, and the task of limiting its spread was rendered vastly more complicated. The fact of the site of infection in childhood being in the majority of cases abdominal afforded proof of the intimate relationship between tuberculosis in man and bovines. He desired to endorse what Dr. Squire had said about "dispensaries." Personally he had great expectations of the usefulness of that system. Dr. Squire had referred to the high percentage of "contacts" who had been found to be consumptive at the Paddington Dispensary. It should be stated that there had been no selection of "contacts" for examination, except such as was caused by refusals to submit to examination. Altogether 228 "contacts," resident in Paddington, were examined during 1909, in connexion with 340 primary (Paddington) "consumptives." It was remarkable that 50 of the "contacts" were found to have "definite" and 98 "suspected" consumption—the distinction between the two classes being the demonstration of the bacilli in the former. If the two classes of cases be grouped together, the proportion of "consumptives" among primary patients was 78·6 per cent., and among "contacts" 64·9. It would be difficult to adduce more convincing evidence of the need of examining "contacts." Up to the present time the experience at that dispensary tended to show that the need for sanatorium accommodation was not very pressing when dispensary treatment was available. Under the supervision afforded by the visits of the medical officers and nurse, patients made very excellent progress in their own homes. If they accepted Dr. Squire's views as to the danger to the community attaching to the "cured" consumptive, the need of "colonies" was beyond dispute. There was, moreover, a real need of "homes" for patients in the last stages of the disease. Personally he was of opinion that it should be possible to remove patients to such homes and detain them there when they were sources of danger to their relatives, and the public generally, either by reason of their home conditions or personal habits (neglect of all precautions). The exercise of such powers could only be justified if they were convinced of the infectious character of the disease. In spite of what had been said to the contrary that evening, he thought there was no real divergence of opinion on that point. The infection by patients of members of their families had been only too frequently observed by him. He had often recorded the deaths of wives (apparently in good health during their husbands' life-time) who had been infected by nursing their husbands, and succumbed to the disease within a few years after their death. All doubts as to such mishaps would, however, be cleared up by notification, which, to be efficient, must be compulsory. Voluntary notification was so incomplete that the data furnished thereby were misleading.

Sir SHIRLEY MURPHY noted that the Medical Officer of the Paddington Dispensary found 20 per cent. of "contacts" of patients attending that institution were tuberculous. He would like to know what proportions of tuberculous persons would be found among those living next door to these patients, so as to show results for persons of the same class not living in the same houses as the patients. To understand the figures correctly it was of course necessary to know what that gentleman's standard was in determining whether a patient was tuberculous. Dr. Dudfield said he was guided by the finding of tubercle bacilli in the sputum. How widely that bacillus existed he (Sir Shirley) could not conjecture, but he thought comparative observations of the sort suggested would be of value.

Dr. BUTLER said he supposed it would not be denied that tuberculosis was an infection, and that it could attack only those who were susceptible to the invasions of the tubercle bacillus. But it was equally apparent that not all people were susceptible, and that under given conditions the variability in resistance of different persons made it impossible to state what constituted an effective dose of the infection. In this man differed from the lower animals, among whom the same degree of variability was not apparent. No one who had had any considerable experience of consumption could fail to have observed that in certain families, often at long intervals, the different members as they attained a certain age became subject to attack; and so certainly was this a characteristic of particular families that the anticipation of the fate of the younger members attained to a degree of certainty, which, unfortunately, was rarely falsified by the event. It was difficult to avoid the conclusion that there was a hereditary susceptibility or predisposition which did not enable these families to resist such an ordinary exposure to infection as that to which the average person was subject, and which in the case of the average person was practically innocuous. Again, there were certain acquired conditions which had been clearly established as rendering people unduly liable to consumption. Pleurisy, pneumonia following the exanthemata, and especially the pneumonia associated with typhoid fever and measles, were well-known instances. The inference forced upon one from these considerations was that, given such an undue susceptibility to tuberculosis as these cases illustrated, the prevalence of the tubercle bacillus, under ordinary conditions, was sufficient to ensure infection, and that, in fact, such cases would acquire the disease. The difficulty was to know whether effort should be directed towards stamping out the infection itself by attempts to limit the distribution of the bacillus, or to the promotion of such measures as were calculated to increase the average resistance to infection of the general population. The author seemed to agree that the best hope lay in measures directly intended to increase resistance. He (Dr. Butler) had no doubt that the improvement in tuberculosis mortality was due to the great increase in resistance of the population, which in turn was due mainly to improved hygienic and social conditions—chiefly to better feeding and better housing. Speaking broadly, there was only so much money and so much effort which could be devoted to the object of reducing the prevalence of tuberculosis, and if attention was unduly directed to attempting to limit the distribution of the bacilli, it would necessarily be withdrawn from those more effective measures which had for their object the raising of resistance not only to tubercle, but to other infections. Every year people were succumbing to tubercle because they had not enough to eat, and because they could not afford the minimum hygienic conditions necessary for a healthy existence. It was most necessary to bear in mind the twofold aspect of the problem, for in whichever direction effort was made, it was costly, and only in a proper correlation of work with this twofold aim could an economic solution be found.

Dr. NEWSHOLME, after referring to the pioneer work of Dr. Squire on the subject of tuberculosis, expressed the opinion that balanced views as to the pathology of tuberculosis were almost essential if the direct measures of control over the disease were to be maintained in due perspective. This was especially true in regard to one's view of the importance of susceptibility, whether acquired or inherited. He had gathered from one speaker that additional expensive measures, undertaken with a view to prevent the spread of tuberculosis, could scarcely be justified until it had been shown that Professor Karl Pearson's conclusions as to the relatively small importance of infection were disproved. Dr. Squire also said that it was, perhaps, time that "we insisted less on the infectiveness of the consumptive and more on the dangers of the conditions which produce susceptibility," although he advocated measures against infection. They were bound to remember that the most perfect statistical methods would not atone for serious defects in the data, and until complete data could be obtained statistical inferences must be judged in the light of other lines of evidence, and could only be accepted if they pointed in the same direction as was indicated by more satisfactory methods of investigation. Statistical evidence must be compared with the results of experiments on lower animals, and especially with the results of experience of cattle, the conditions of whose life could be exactly observed. Experimentally, it had been proved that tuberculosis was infectious, and Dr. Cobbett's[1] experimental results showed that within certain limits dosage of infection was the main determining influence, susceptibility playing a minor part. The experience with herds of cattle proved that once a herd had been cleared of tuberculous members and the cowshed had been purified, that herd could, in the absence of fresh importation of disease, be kept permanently free from tuberculosis without any change in other conditions of environment. In view of such facts, hygienists could not permit statistics to block the advocacy of every known means for diminishing the dosage of infection. Similar remarks applied as to the statistics of inheritance of susceptibility to tuberculosis. No statistics had, so far as he knew, been given in which family infection had been separately considered from family susceptibility. As such statistics were probably unattainable, the problem of inherited susceptibility again must be judged by our knowledge of the pathology of tuberculosis. Dr. Squire's paper raised some important practical points as to tuberculosis dispensaries and as to the relative value of these and of the institutional treatment of phthisis. He (the speaker) had always advocated such dispensaries as means for securing early diagnosis, supervision of patients, the giving of social help, and the discovery of contacts. He could not accept the view, however, that the discovery of early tuberculosis in contacts was more important than successful arrangements for the hygienic treatment of open, and especially of advanced, cases of tuberculosis. Both measures should be adopted. Actual experience embracing the investigation and watching of some hundreds of cases had convinced him that the segregated treatment of cases with abundant expectoration, and especially of bed-ridden cases, was the one supremely important means for preventing excessive dosage of infection. The historical argument pointed to the same conclusion, and he might be permitted to note the fact that Robert Koch, in probably the last paper he wrote, which appeared in the last number of the *Zeitschrift für Hygiene*,[2] had summarized and confirmed the argument which he (Dr. Newsholme) gave before this Society on two occasions, and added further evidence pointing to the same conclusion. He shared the sorrow of another speaker as to the small amount of work done in London with regard to tuberculosis. With the exception of a few boroughs, London was ten years behind many of the other large towns in the country. There were indications, however, that this backward condition of things would soon be remedied.

Dr. SQUIRE, in reply, said Dr. Newsholme placed the emphasis on his (Dr. Squire's) remarks in the way that suited his argument, but he did not admit it rested where he (Dr. Newsholme) put it. He did not suggest that official visits usually caused friction, but that some people might resent the official visit, and therefore the voluntary visitor might be added to the official visitor. It was impossible to review all one's knowledge and opinions on such a large subject in one paper. With regard to the relative values of infection and susceptibility, he would not enter into that in detail. For some time insistence had rightly been placed on infection, but the public had come to believe that infection was the one and only danger. They must be taught to recognize the importance of susceptibility, especially as it seemed to be thought that every person known to be consumptive must be shunned as dangerous. He agreed with Dr. Newsholme that the measures found so effective in regard to bovine tuberculosis could not be applied in the case of human beings, for the good reason that such a large proportion of the population were already infected, that to isolate them would be to have a great shortage of people to do the work of the community. Thus it was almost beyond administrative capability to check the spread of the disease by stopping infection at its source. But he did not think it impossible to stop the harm which might be done by those people by increasing the resisting power of the persons moving among such a large number of people infected with tubercle. He did not admit the confusion in his mind which had been attributed to him in mixing up dosage, susceptibility, and infection. He mentioned in his paper that, as most people were by nature comparatively insusceptible, they had nothing to fear from the consumptive except the presence of an overwhelming dose; that would show he regarded dosage as important. The hour was too late to permit him to comment on all the speeches. He agreed with Dr. Dudfield on many points which he raised. With regard to statistics and hereditary predisposition, he would like to see a statistical department set to work to prove hereditary predisposition in scarlet fever. He believed it would be as strong as in consumption. and for that reason he would place less emphasis on hereditary predisposition than was given to it by some other authorities.

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[1] *Brit. Med. Journ.*, 1909, ii, p. 867.

[2] *Zeitschr. f. Hyg. u. Infektionskr.*, Leipz., 1910, lxvii, p. 1.

Epidemiological Section.

January 27, 1911.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

The Ætiology and Epidemiology of Paratyphoid Fever and "Food-poisoning."

By F. A. BAINBRIDGE, M.D.

INTRODUCTION.

DURING the last few years a considerable advance has been made in our knowledge of the distribution of the bacilli associated with paratyphoid fever and with outbreaks of "food-poisoning." It seemed to me, therefore, that some account of the current views regarding the relation of these organisms to disease in man might be of interest to the members of this Section; these views will be discussed in the light of the recent improvements which have been made in the differentiation of the organisms concerned in these infections. I propose to confine myself as far as possible to the epidemiological aspect of the subject: but a preliminary reference to the bacteriology of these bacilli is desirable, since they are closely related to one another and their identification requires care.

BACTERIOLOGICAL DIFFERENTIATION.

There are four organisms which have often been found at one time or another in association either with cases of paratyphoid fever or with outbreaks of "food-poisoning" (in the clinical sense of the term). They are *Bacillus enteritidis* (Gaertner), *Bacillus suipestifer* (or *Aertryck*), *Bacillus paratyphosus* A and *Bacillus paratyphosus* B. I propose to deal with these bacilli only and to leave out of consideration *Bacillus botulinus* and other organisms.

The means by which these four bacilli are differentiated and identified are shown in the accompanying scheme :—

Description of organism	Cultural characters, including sugar tests	Agglutination test	Absorption test
(1) *Bacillus suipestifer* (or *Aertryck*)	Identical	Usually indistinguishable	Differentiated by this test
(2) *Bacillus paratyphosus* B ...			
(3) *Bacillus enteritidis* (Gaertner)		Stands alone	—
(4) *Bacillus paratyphosus* A ...	Differs from the others	Stands alone	—

Bacillus paratyphosus A can be readily identified by cultural methods and by agglutination tests with a suitable serum. The other three organisms are identical in their cultural characters, but *Bacillus enteritidis* (Gaertner) can be differentiated from *Bacillus suipestifer* and *paratyphosus* B by agglutination tests. Finally, *Bacillus suipestifer* and *Bacillus paratyphosus* B, which are usually—though not invariably—indistinguishable by the agglutination method, can be differentiated from one another by means of the absorption method introduced by Castellani.

It seems clear that all these bacilli can be differentiated and identified, if suitable methods are employed. Many German writers, however, do not use the absorption method for this group of bacilli ; as a result, they draw no distinction between *Bacillus paratyphosus* B and *Bacillus suipestifer*, but include these two bacilli under the term "paratyphoid" bacilli.

The fact that English writers regard *Bacillus suipestifer* and *Bacillus paratyphosus* B as distinct and separate bacilli, whereas some German workers regard them as identical, is of importance ; it accounts to a large extent for the existing differences of opinion, both as to the distribution of these bacilli and as to their relation to disease in man.

HISTORICAL.

(1) As you are aware, *Bacillus enteritidis* was isolated by Gaertner in 1888, during an outbreak of "food-poisoning," both from the spleen

of a patient and from the meat which was regarded as the cause of the infection; this observation was the starting point of our definite bacteriological knowledge of "food-poisoning," although organisms of some kind had previously been obtained in similar outbreaks.

(2) The next step was taken by Durham and by De Nobele, who, almost at the same time in 1898, obtained from cases of "food-poisoning" and from the suspected food a bacillus which was identical in its cultural characters with *Bacillus enteritidis*, but could be distinguished from the latter by agglutination tests, then newly introduced. The bacillus was called *Bacillus Aertryck*, this being the name of the village in which occurred the outbreak recorded by De Nobele. Since that time it has been isolated in many outbreaks of "food-poisoning."

Further inquiry has also shown that *Bacillus Aertryck* is indistinguishable from *Bacillus suipestifer*—an organism which occurs in the alimentary canal of healthy pigs and in the tissues of pigs suffering from swine fever; and the two organisms are almost certainly identical.

(3) Finally, Schottmüller (1900-01) obtained two bacilli from the blood of patients whose clinical symptoms resembled those of enteric fever, and called them *Bacillus paratyphosus* A and B respectively. Similar bacilli had previously been isolated by Gwynn (1898) in a case of paratyphoid fever, by Achard and Bensaude (1896) from the urine of a case of clinical enteric fever, and by Widal (1897) from a thyroid abscess. During the last few years a large number of cases have been recorded.

EPIDEMIOLOGY.

(I) *Paratyphoid Fever.*

(1) *Bacillus paratyphosus* A.—With these introductory remarks, we may turn to the epidemiological significance of these organisms. *Bacillus paratyphosus* A may be very briefly considered. It has never been isolated in this country, and comparatively few cases have been recorded in Germany and America. In India, however, paratyphoid A fever appears to be not uncommon. Harvey has recently described ten cases in which this bacillus was isolated either from the blood or from the excreta; in their clinical aspect all the cases resembled enteric fever. Semple and Greig (at Kasauli) obtained *Bacillus paratyphosus* A from the blood or excreta of four cases, which also resembled enteric fever. One of these patients became a temporary carrier for some weeks after convalescence. In Sumatra, Baermann and Eckersdorff have recorded

eight cases of paratyphoid A fever, and a small epidemic of nineteen cases was observed in Paris. Up to the present no fatal cases have been recorded (apart from complication with other diseases), and the vast majority of the cases have resembled mild enteric fever, although *Bacillus paratyphosus* A has also been found in cases of acute enteritis. It has, however, been obtained on two occasions from the gall-bladder during operation for cholecystitis or gall-stones, and in one case from an abdominal abscess. With one exception, this bacillus has not been found in healthy men, in the lower animals, or in food or water. Both Morgan and Savage have obtained from the intestinal contents of healthy pigs an organism which is identical with *Bacillus paratyphosus* A in its cultural characters; but it is not agglutinated by paratyphoid A sera, and is probably a different organism. Since the distribution of *Bacillus paratyphosus* A is practically confined to patients with symptoms resembling those of enteric fever, or with disease of the gall-bladder, and since the presence of agglutinins for this organism in the serum of the patients points to an infection, it may fairly be regarded (in the absence of evidence to the contrary) as the causal organism of their illness. Very little is known concerning the existence of temporary or chronic carriers, but Harvey and Grattan have recently described a small outbreak (eight cases) of paratyphoid A fever which was apparently caused by a cook, who had recently had paratyphoid A fever and cholecystitis. The examination of his fæces and urine was negative, but the outbreak came to an end as soon as he was isolated.

(2) *Bacillus paratyphosus* B.—Schottmüller and other early writers believed that *Bacillus paratyphosus* B was the cause of the illness of those patients from whose blood it was isolated, and this view has been accepted by English writers (Boycott, Savage, Bainbridge) and by Seiffert and others. Recently, however, Conradi and Hübener have put forward the doctrine that the isolation of "paratyphoid" bacilli from the excreta (or even the blood) of a human being does not in the least indicate, in the absence of other evidence, that he has or has had paratyphoid fever. This conclusion is based upon their observations on the distribution of such bacilli in Nature. Thus Conradi found so-called "paratyphoid" bacilli in 29 out of 250 typhoid convalescents, in patients suffering from tuberculosis and other diseases, and in healthy individuals. Prigge and Sachs-Müke examined the fæces of 5,252 individuals, and found "paratyphoid" bacilli in 60 cases, apart from paratyphoid fever. Further, so-called "paratyphoid" bacilli have been found in sausages (Hübener, Rommeler), in milk, water, and in meat. Conradi also

noticed that food containing these bacilli could be consumed by human beings without ill-effects.

In criticism of the view of Conradi and Hübener it may be pointed out, in the first place, that the bacilli which they (and others) isolated were not submitted to absorption tests, and that it remains doubtful whether they were dealing with *Bacillus suipestifer* or *Bacillus paratyphosus* B. In point of fact, all such strains which we received from these writers have been proved by absorption tests to be *Bacillus suipestifer*. Secondly, most of those observations have been carried out in one district of Germany, and they are totally at variance with the results of other workers in Germany and in this country. In Berlin, Sobernheim examined 1,000 samples of fæces or urine from healthy men without once finding *Bacillus paratyphosus* B. Six hundred samples of human fæces were examined by Seiffert, with completely negative results. In this country Morgan investigated 303 samples of fæces from children suffering from summer diarrhœa; *Bacillus paratyphosus* B was not found. Savage failed to find *Bacillus paratyphosus* B in man, except in cases of paratyphoid fever. Dr. O'Brien and myself have recently investigated the fæces of a number of typhoid convalescents with completely negative results. The examination of the fæces of the lower animals, and of meat derived from such animals, has proved to be equally unsuccessful. Zwick and Weichel investigated 70 samples of apparently sound meat, and Müller examined 50 samples of meat from diseased animals (cows and calves) ; in no case did they obtain *Bacillus paratyphosus* B. The investigation of pigs' fæces by Morgan, by Savage, and by myself, also invariably yielded negative results.

In the hope of throwing further light on the distribution of *Bacillus paratyphosus* B, Dr. O'Brien and myself procured from various sources, mostly German, a number of strains of "paratyphoid" bacilli. Examination of these strains by cultural methods, by agglutination, and by absorption tests showed that they fell into two groups: one identical with standard strains of *Bacillus suipestifer*, the other identical with standard strains of *Bacillus paratyphosus* B. Further, all the strains of *Bacillus paratyphosus* B were derived from cases of paratyphoid fever or from chronic paratyphoid carriers, whereas all the strains derived from food or "food-poisoning" were identical with *Bacillus suipestifer*. Taking these facts into consideration, and bearing in mind that there is no conclusive evidence that the "paratyphoid-like" bacilli isolated by Conradi and others are really *Bacillus paratyphosus* B, it seems clear that the evidence strongly supports the view that the distribution of

Bacillus paratyphosus B is practically confined to man. It is probable, I think, that its normal habitat is the human alimentary canal (including the bile-passages), and that it is rarely (if ever) found in man, except in cases of paratyphoid fever and paratyphoid carriers. It may be admitted, however, that such carriers may cause infection not only directly but also by contaminating otherwise sound food, just as is the case with typhoid carriers.

This conclusion as to the distribution of *Bacillus paratyphosus* B is of epidemiological importance. In the first place, the evidence just detailed that this organism is found in cases of paratyphoid fever and under no other circumstances points to its being the actual cause of the illness of those patients in whom it is found. One ought to mention, however, that, as Schottmüller noticed in 1904, this bacillus is capable of giving rise not merely to paratyphoid fever but also occasionally to acute gastro-enteritis. An acute outbreak of this character has recently been recorded by Dr. Dudfield and myself. Secondly, the existence of chronic paratyphoid carriers (in man) brings us nearer to a rational explanation of the hitherto obscure epidemiology of paratyphoid fever. Lentz (1905-06) was apparently the first to call attention to these carriers; he recorded six cases, one of whom was under observation for at least a year. Similar cases have been observed by Hamilton (five cases), Prigge and Sachs-Müke (six cases), by Gaehtgens and by Meyer. Brückner recently examined the fæces and urine of 316 individuals who had previously had an attack of paratyphoid or enteric fever. He discovered nine typhoid carriers and three paratyphoid carriers; two of the latter were excreting paratyphoid bacilli three years after the original attack of paratyphoid fever. Prigge and Sachs-Müke found that all their cases give a positive serum reaction for *Bacillus paratyphosus* B. An analysis of the twenty-nine carriers recorded by these observers brings out two striking facts: twenty-six of the cases were women (the sex in one case is not stated), and seven of the cases were suffering from cholecystitis or gall-stones. Forster and Kayser, indeed, isolated *Bacillus paratyphosus* B from the gall-bladder of a woman; post mortem, gall-stones were found in her gall-bladder. It is obvious, therefore, that in their distribution as regards sex, in the frequent co-existence of biliary disorders, and in the presence of agglutinins in their serum, these carriers present a very close analogy with typhoid carriers.

The question of the capacity of paratyphoid carriers to cause infection has so far received very little attention. While investigating an epidemic of enteric fever, Wernicke (1907) found that 6 per cent. of the cases were paratyphoid fever; the cause of the epidemic was traced to a milkshop, in which resided (or worked) several typhoid carriers and one paratyphoid carrier. In the outbreak recorded by Dr. Dudfield and myself the evidence almost certainly excluded an ordinary food infection, and pointed to infection by a carrier; the blood of the individual whom we believed to be a carrier gave a positive serum reaction for *Bacillus paratyphosus* B long after the outbreak, although examination of the fæces (and urine) was negative. The recent outbreak of gastro-enteritis at Wrexham is also stated to have been caused by *Bacillus paratyphosus* B, and is attributed to infection by a human carrier.

Within the last year Sacquépée and Bellot have published an account of an epidemic of paratyphoid fever, and have given good grounds for attributing the source of infection to a carrier. In a corps of 250 soldiers in France, 19 cases of paratyphoid fever occurred in the course of eight days. The duration of the illness was from two to three weeks, and the clinical course was that of mild enteric fever; no deaths occurred. *Bacillus paratyphosus* B was recovered from the fæces or urine of 12 cases, and all the cases gave a positive serum reaction for this bacillus. An inquiry into the cause of the infection made it clear that the water supply could be excluded. It was found, however, that a man, who cooked the food for these soldiers, had suffered from paratyphoid fever a short time before the occurrence of the epidemic. His blood gave a positive serum reaction for *Bacillus paratyphosus* B, and this bacillus was obtained from his stools. Repeated examinations of his stools were made for some months, always with a positive result, and there can be no doubt that he was a paratyphoid carrier.

The foregoing evidence, incomplete though it is, as to the distribution of *Bacillus paratyphosus* B, the occurrence of persistent paratyphoid carriers, and the production of infection by such carriers, enables one to form a clear conception of the epidemiology of paratyphoid fever. The comparative infrequency of paratyphoid fever in this country (at least 3—6 per cent. of clinical enteric fever cases are really paratyphoid) and its favourable course make it difficult to obtain adequate material for investigation, but it is very desirable that, whenever such cases do occur, a careful search should be made for carriers.

(II) "*Food-poisoning.*"

(3) In dealing with the bacilli associated with "food-poisoning," one must bear in mind that the differentiation of *Bacillus enteritidis* (Gaertner) and *Bacillus suipestifer* by agglutination tests was not made until 1898. Except in those cases in which the strains were preserved until the introduction of the agglutination method the nature of the organisms isolated in the earlier outbreaks of "food-poisoning" remains doubtful. The available evidence concerning the association of *Bacillus suipestifer* with "food-poisoning" is further narrowed down by the omission of many writers to distinguish between *Bacillus suipestifer* and *Bacillus paratyphosus* B, and by their description of the bacilli found in such outbreaks as "paratyphoid" bacilli. On the present occasion I have taken into consideration only those outbreaks of "food-poisoning" in which convincing bacteriological evidence exists as to the nature of the organism which was present. In some outbreaks of "food-poisoning" *Bacillus suipestifer* was isolated both from the patients and from the food to which the illness was attributed; in others, it was obtained only from the food or from the patients. In most instances the bacillus was agglutinated by the serum (in high dilution) of the patients. Further, symptoms of illness were usually manifested only by those who had eaten certain articles of food; this food was almost invariably derived either from pigs or from cattle. In some cases the meat came from animals which had been ill, and were slaughtered for that reason; in other cases the animals were *stated* to be healthy.

Distribution of Bacillus suipestifer.—Recent observation has tended to raise doubts as to the validity of the foregoing evidence that *Bacillus suipestifer* causes "food-poisoning." In the first place, it has been stated that this organism has a wide distribution in Nature. Uhlenhuth obtained it from the fæces of 8 per cent. of healthy pigs, and similar figures were recorded by Seiffert. Savage, however, examined the excreta of a number of healthy cattle and horses, with completely negative results. I have recently failed to find *Bacillus suipestifer* in the fæces of fifty pigs. There is no doubt that *Bacillus suipestifer* occurs in apparently sound food, but the bacilli seem to be very scanty, and their presence is only demonstrable by special methods. Opinions differ as to their frequency in food, and the recent work of Sobernheim, Zwick and Weichel and Müller indicates that they are not often present. It is clear that, since it occurs in food, *Bacillus suipestifer* must from time to time be ingested by man; both Hübener and Conradi found that meat containing this bacillus could be eaten by human beings with impunity, and the bacilli could be obtained from the fæces of such people on the following day, but not subsequently. Prigge also noticed that those who ate such meat showed no sign of infection, and that their serum did not agglutinate the bacillus obtained from the meat. Although these bacilli are described as "paratyphoid" bacilli, my observations of strains derived from food leads me to believe that the writers were dealing with *Bacillus suipestifer*. The evidence for the distribution of *Bacillus suipestifer* in man is conflicting. Sobernheim and Seiffert failed to obtain it from the excreta of healthy men, whereas Conradi regards the presence of "paratyphoid" bacilli as of frequent occurrence. In this country it has very rarely been found in man apart from outbreaks of "food-poisoning," although Murray, Williams, and Rundle have recently isolated it in seven out of forty cases of summer diarrhœa. From our own observations, Dr. O'Brien and myself are disposed to believe that the usual habitat of *Bacillus suipestifer* is the alimentary canal of pigs and other lower animals, and food derived from such animals, although it is not very often present even in animals and food. The evidence as a whole suggests that *Bacillus suipestifer* has a regional distribution, occurring much less frequently in some districts and countries than in others. Whether outbreaks of "food-poisoning" occur more often in those districts in which this organism is most widely distributed is not known. But it seems probable that in this country and in some parts of Germany, *Bacillus suipestifer* is *not* widely distributed either in healthy animals or food or healthy men. There are other facts, however, which arouse scepticism as to its capacity to initiate an infection. The observations of Dorset and Bolton (confirmed by Uhlenhuth, Hübener and Xylander) have made it perfectly clear that swine fever is caused by a filter passer, and that *Bacillus suipestifer*, which was at one time regarded as the cause of the disease, is merely a secondary invader. Petrie and O'Brien showed that it played an equally minor part in an epizootic among guinea-pigs, which was probably caused by a filter passer. It is further stated that the consumption by man of the flesh of pigs dead of swine fever has rarely given rise to symptoms of "food-poisoning," although presumably *Bacillus suipestifer* was sometimes present in the meat.

Relation of Bacillus suipestifer to "Food-poisoning."—In forming an opinion as to the relation of *Bacillus suipestifer* to "food-poisoning," two groups of facts must be taken into consideration. On the one hand, this organism can frequently be isolated from the excreta of patients suffering from "food-poisoning," while it is very rarely found in healthy people, at least in England. The outbreak can often be traced to a given article of food, and the same bacillus can be found in the food. On the other hand, there is evidence that meat containing *Bacillus suipestifer* can sometimes be eaten without ill-effects, and that in other diseases it is merely a secondary invader and does not initiate the illness. It must be admitted, I think, that *Bacillus suipestifer* has some relation to "food-poisoning," and that it must be either a secondary invader or a causal organism of the disease. If *Bacillus suipestifer* is a secondary invader in "food-poisoning" in man, the primary cause of the disease may be either a filter passer or a ptomaine. There is no direct evidence for the presence of a filter passer; and, unlike filter-passer infections, "food-poisoning" is not contagious. Nor has the hypothesis that "food-poisoning" is caused by ptomaines received any support during the last few years. The evidence for this view is still slight and indefinite; and, so far as I know, no one has yet demonstrated the presence of ptomaines in food which had caused "food-poisoning." I am inclined, therefore, to think that *Bacillus suipestifer* probably actually causes "food-poisoning," although it is not yet possible to reach a final conclusion on the question. But it appears likely that it is always potentially dangerous to man, and that chance, or perhaps the number of bacilli ingested, determines whether food containing this organism does or does not set up disease. We know that *Bacillus typhosus* is occasionally taken in by the mouth and excreted without giving rise to clinical symptoms of disease. It has also been noticed that, when food containing *Bacillus suipestifer* was eaten without ill-effect, the number of bacilli in the food was extremely small. Even during outbreaks of "food-poisoning," individuals are met with who remain in good health although they have eaten the suspected food and are excreting *Bacillus suipestifer* or *Bacillus enteritidis* in their fæces. One may perhaps regard the occasional ingestion of this bacillus by man without ill-effect as showing that the individual was less susceptible rather than that the bacillus was non-pathogenic.

(4) *Bacillus enteritidis* (Gaertner).—The relation of *Bacillus enteritidis* to "food-poisoning" is not difficult to establish, since it can be identified with certainty, and there is abundant and reliable evidence as to its distribution. I have met with accounts of eleven outbreaks of "food-poisoning" in which this bacillus was obtained either from the patients or from the food to which the illness was attributed, or from both the food and the patients. The nature of the bacillus was ascer-

tained in all cases by cultural and agglutination tests. In one outbreak the cause of the illness was said to be brawn, in another pork sausages; the remainder were due to veal or beef, and in eight outbreaks the animal (whose flesh was eaten) was ill at the time when it was slaughtered. The fact that so many outbreaks were apparently caused by meat derived from diseased animals seems important, since their illness would tend to raise the virulence of *Bacillus enteritidis* and to favour its extension into the tissues of the animals. Apart from these outbreaks, *Bacillus enteritidis* has seldom been isolated from the organs of cattle which have died of disease or been killed in emergency. Müller, Edenhuizen, and Portet examined the organs of healthy or diseased cattle without finding *Bacillus enteritidis*. Nor was it obtained by Savage from the fæces of cattle, sheep, and pigs. Sobernheim examined the organs of a very large number of healthy animals, and twice isolated *Bacillus enteritidis*. This organism is of rare occurrence, not only in animals but also in man. It has been found only five times in human fæces—four times by Morgan in cases of summer diarrhœa, and once by Savage in a case of typhoid fever. Epizootics associated with the presence of *Bacillus enteritidis* have been observed by Boycott, Dunbar, Bainbridge, and others in rats and rabbits, and by McConkey in guinea-pigs; this bacillus was apparently the actual cause of all the epizootics.

During the last few years bacterial viruses have been extensively employed for the destruction of rats; many of these viruses are either pure cultures of *Bacillus enteritidis* or consist of foodstuff containing this bacillus. The administration and distribution of these viruses by unskilled persons must almost inevitably expose those who handle the virus to some risk of infection, and Handson and Williams have reported an outbreak of "food-poisoning" in which they consider that food had become infected by one of the viruses. No *direct* evidence of such infection was obtained, but the epidemic occurred among persons who took their meals in the room in which the virus had been placed.

Outbreaks of disease in man traceable to infection by a virus are, however, very infrequent. Many samples of these viruses are only slightly virulent even for rats, and the dose of virus which those who handle it are likely to ingest is probably small, particularly if the usual instructions are followed. Moreover, occasional individual instances of infection might easily escape notice and fail to be recorded. These reasons help to explain why *Bacillus enteritidis* has so seldom caused disease, in spite of its extensive employment as a virus.

There can be little doubt, from the evidence just detailed, that *Bacillus enteritidis* can cause "food-poisoning." Further information is needed, however, concerning its distribution in the lower animals and the possible existence of animal carriers.

It thus appears that *Bacillus enteritidis*, *Bacillus suipestifer*, and *Bacillus paratyphosus* B can all give rise to the clinical symptoms of "food-poisoning." In the case of *Bacillus paratyphosus* B the usual source of infection is probably food which has been contaminated by a human carrier; in the case of *Bacillus suipestifer* and *Bacillus enteritidis* (Gaertner) the source of infection is food derived from healthy or diseased animals, and containing these bacilli from the outset. The two latter organisms are probably responsible for the majority of food-poisoning outbreaks. In outbreaks associated with the presence of *Bacillus enteritidis* the incriminated food has usually been derived from diseased cattle; in outbreaks associated with *Bacillus suipestifer* the food has been derived from cattle or pigs. The possibility that such outbreaks may be caused by bacterial viruses must not be overlooked.

In conclusion, it is obvious that these facts have a practical value for those who are called upon to investigate outbreaks of "food-poisoning." It is of importance to ascertain what organism is present in such outbreaks, since that knowledge gives a clue to the most probable source of the infection. Depending upon the nature of the bacillus which is found, attention can be at once directed to the kind of food in which the bacillus usually occurs, or a search may be made for a human carrier.

DISCUSSION.

The PRESIDENT (Dr. Theodore Thomson) said the subject of the paper was of much interest and considerable complexity, and the author could be congratulated on its remarkable lucidity. Mainly, the paper fell into two heads: (1) paratyphoid fever, (2) food-poisoning, though he did not wish to dissociate the two subjects. To him they seemed of equal interest. It was true that in this country we had not had much experience of paratyphoid fever, and when it did occur it did not appear to be fatal. Its interest lay in the fact that paratyphoid fever, in the opinion of most physicians, was clinically indistinguishable from typhoid fever. Indeed, the sole distinction seemed to be a bacteriological one. Possibly paratyphoid fever might be more allied to typhoid than could at present be established. The second division of the paper was of importance because the disease gave rise to serious outbreaks of food-poisoning.

Dr. G. S. BUCHANAN remarked that two letters had been received concerning the paper; one from Dr. Llewelyn Williams, who had been engaged in investigating the outbreak at Wrexham to which the author referred, and the second from Professor Delépine, in course of which he said, "As you know, I have collected material for the purpose of investigating the meaning of these differences" (i.e., the cultural, pathogenic, and agglutinating properties of different organisms associated with meat-poisoning) "for a great number of years and have meanwhile indicated more than once that all the bacilli which I have isolated belong to a group of bacilli intermediate in characters between the colon and the typhoid bacillus, bacilli which I have been in the habit, possibly wrongly, of calling *bacilli of the colon group*, but I have always clearly indicated what I mean by this group, and have shown that the bacilli which are the most clearly connected with the causation of what is known clinically as food or ptomaine poisoning are those which approximate most closely to the *Bacillus enteritidis* of Gaertner. The difficulty of giving an exact name to these bacilli is that the description given by Gaertner originally is not sufficient to show whether several of the bacilli described by subsequent writers were covered or not by this description. Owing to the fact that many bacilli showing characters intermediate between those of the classical types can be obtained from cases of typhoid fever, paratyphoid, and infectious enteritis known as food-poisoning, I cannot help feeling that excessive refinement in the application of certain tests may lead to the creation of types of bacilli which do not indicate exactly the rôle which they play in the production of disease. These test-tube studies are undoubtedly of value, but their epidemiological value must be tested by the actual investigation of epidemics."

Dr. Buchanan desired to join in congratulating the Section and the author on the excellence of the paper which had been read. It was a good illustration of the way in which our knowledge of the causation of the particular class of food-poisoning which was under consideration had been advanced by the patient work of bacteriologists. They had enabled us to concentrate attention on a particular group of very similar organisms, the "Gaertner group," as causative agents, to the exclusion of a number of other like organisms, some of them normal intestinal bacteria, with which otherwise they would have been confused. Within the "Gaertner group" they had distinguished certain types, which appeared, so far as could be gathered, to be true types, although the differential diagnosis between them was a matter of considerable difficulty, and the differential tests were extremely minute. Possibly, after all, the difference was one of habitat; it might be that an original ancestor had now assumed one type when it was in human environment, another in the bovine, and another in the pig. The thesis of the paper that when there was an outbreak of food-poisoning from which *Bacillus paratyphoid* B was recovered, that outbreak had been due to infection of human origin, was a very interesting one, and if confirmed, it was of considerable epidemiological importance. Dr. Bainbridge was not alone in the suggestion, and it would be found on looking through the series of reports which Dr. Savage contributed to the medical department of the Local Government Board during the last three or four years, that Dr. Savage came to much the same conclusion. If that could be accepted as, at all events, a provisional working hypothesis, it was a very valuable one. He thought it might be so accepted, provided it were checked and fully tested whenever opportunity offered. With regard to the animal group, Dr. Bainbridge expressed some doubt—the doubt of others—as to the ability of the *Bacillus suipestifer* to cause those outbreaks. He (the speaker) did not quite see how that difficulty could arise if, as he believed to be the fact, the *Bacillus suipestifer* and the Aertryck bacillus were the same; because there were several outbreaks which had been associated with the Aertryck bacillus on evidence which could be scarcely gainsaid. Besides the original outbreak at Aertryck, there was that which Dr. H. E. Durham investigated, which was clearly associated with that type of organism. Another point with regard to the animal strains was that Dr. Bainbridge considered the type of *Bacillus enteritidis* of Gaertner to be specially associated with bovine infection. That, he thought, must be conceded. In several well-known cases the meat was beef. There had been recent outbreaks, however, notably at St. Anne's-on-Sea in 1908, due to pork-pies, which were attributed to the same organism. There was another at Bedford two or three years ago, which was attributed to the *Bacillus enteritidis* type, and not to the *Bacillus suipestifer.* He asked Dr. Bainbridge whether he would say if later evidence had been considered in that connexion.

With regard to the larger outbreaks of food-poisoning, such as had been associated with the making of large batches of pork-pies, potted meats, &c., which had given rise to extensive infection, it was, of course, well known that one could make a differentiation between those in which the animal had obviously been infected, and the meat must have been infectious from the beginning, and those in which the meat was infected in the process of preparation for cooking, or in the after-stages of cooking. In the first class, supposing that the infected meat was prepared and cooked (but not enough to sterilize it) the bacilli originally in the meat would multiply in the succeeding period, and the whole meat become infectious. According to the thesis of the paper, that would be an infection which one would expect from the *Bacillus suipestifer*, or from the *Bacillus enteritidis*, not from *Bacillus paratyphosus* B. In the second case the infection could also be of human origin, and in that case if it were infected by a human paratyphoid case or carrier it would be the paratyphoid B, and that was stated to have occurred recently at Wrexham. But he suggested that if the infection of the meat occurred during the process of its preparation, it did not necessarily follow that the infection must have been by bacteria derived from a human host. One of the characteristic features of several of those outbreaks had been that the people who were engaged in preparing and handling the meat had been those employed in slaughtering; while things used in connexion with the prepared meat were kept in the slaughter-house, and could readily have been contaminated. Therefore, if one might assume that Gaertner infection was present in the slaughter-house—perhaps some animal having the Gaertner infection was slaughtered there, or an animal which was a Gaertner carrier—then there was a possibility of the prepared meat receiving a much more intense kind of infection than one would imagine likely when the person preparing the meat was merely infecting the meat by his own excreta, because the human carrier who infected an article of food could not easily introduce any large dose of infection. Whether the infection in those cases had been of animal or of human origin, it was certainly true, from the history of those outbreaks, that there must have been an enormous multiplication of the bacillus in the meat itself. Dr. Savage showed, in his report appearing in the last annual report of the Medical Officer of the Local Government Board, that the bacilli of the Gaertner group would multiply very freely in meat, whether the meat was sterilized or was prepared meat or sausage-meat in its natural condition, and that the bacilli persisted for many days or weeks. That had been evidenced in other cases. Professor Delépine showed it in the Derby pork-pie case, where the thesis was that the centre of the pie had not been sterilized, and that as the pie cooled the organisms multiplied and extended from the centre outwards. In the case of typhoid infection there was some difficulty in justifying the assumption that when an outbreak was due to a typhoid carrier there had been a multiplication of the organism outside the human body before the outbreak was produced. In these meat-poisoning outbreaks there was no such difficulty.

There was nothing in the paper about toxins, yet he did not think that question could be omitted in considering the epidemiology of this class of food-poisoning. He was convinced that many cases, particularly those due to canned meats, were entirely due to toxins, not to living bacteria. Recently at the Local Government Board attempts had been made to collect cases of canned food-poisoning. That was difficult to do, because such cases were not often reported. Usually one first saw a report of a coroner's inquest, when it was too late to get information or material. But some had been obtained, and he had looked up some recent notes from that point of view. There was one in August at the Astley Sanatorium, where seven servants went to clean a new ward, and took their food with them. They bought a shilling canned tongue on the way, and ate it at their meal. Within an hour of eating it they were all ill with acute abdominal pain, diarrhœa, and collapse. They speedily recovered, and were well about two days afterwards, as soon as the poison had been eliminated. He believed that was a case due to toxins, not living organisms. During the last four months there had been a number of cases in different parts of the country due to poisoning by canned corned beef. In those there was the same sort of story—namely, that the illness had come on almost at once, and, although some of the cases had been severe, the recovery was rapid. In one or two cases material was available, and Professor Delépine and other bacteriologists could not recover any organisms of the "Gaertner group." Of course, in those canned meat cases the toxins might be the endotoxins of dead Gaertner bacilli, and it was known that the endotoxin of the Gaertner bacilli was very resistant to heat. One of the best examples in which that was tested was in a case reported by Dr. Arbuckle Brown at Partick three or four years ago. Some corned beef was taken at a party by twelve persons belonging to five separate families. The bacillus of Gaertner was looked for, but was not found. The poisoning had the same characteristics as those he had mentioned; scarcely any interval between the taking of the food and the onset of the symptoms, with quick recovery after the elimination of the poison. Dr. Brown tested the blood of the persons attacked, by means of the Gaertner strain, and found they reacted strongly. That was a strong indication that the poisoning was due to the endotoxin. He did not wish to say that all canned meat poisonings were due to toxins and not to the living organism, because it was known that there were cases in which the bacillus of Gaertner had been found living in canned meat, and it was known that the temperature to which some canned meats were subjected did not suffice to sterilize them. Major Beveridge had shown that some bacteria were very resistant to even the highest heating which they got in the process of canning. But there was no doubt that the toxin poisoning, as against the living organism, was the principal, or even the only cause of the illness in many cases. It might have occurred to some members that the title "food-poisoning" in the paper was rather too comprehensive when the subject of discussion was limited to the particular group of poisoning cases associated with the Gaertner group of bacilli. Dr. Bainbridge mentioned that there was the *Bacillus botulinus*, and other organisms which produced poisoning of a different kind. Besides that, he (Dr. Buchanan) thought there must be a considerable number of cases, usually mild in degree, associated with decomposition or with dirty conditions of meat and food, which probably had no connexion with poisoning by the Gaertner group. Moreover, all food-poisoning was not bacterial; some of the principal epidemics of food-poisoning had been due to arsenical contamination, and other causes outside the purview of the paper.

Major BEVERIDGE remarked that the subject was of very considerable interest to medical officers in the Army, because they were constantly being called upon to investigate outbreaks of food-poisoning abroad, particularly those which occurred in war-time. He believed Dr. Bainbridge's exhaustive description of those forms of bacilli was the most comprehensive and lucid to which he had ever listened. His own investigations had been confined to the examination of tinned foods, and in that respect, to the *Bacillus cadaveris* of Klein, or *Bacillus putrificus coli* of Bienstock. It was not a pathogenic organism, but it had caused the Government considerable expense. In South Africa, at the time of the war, there were enormous quantities of tins of preserved meat condemned, owing to "blowing." On one occasion 33,000 tins were condemned from this cause in Pretoria. All the tins were piled up in a heap. That night four Kaffirs stole some tins, with the result that they were taken with acute gastro-enteritis in the morning, and two died. That must have been a toxic poisoning. The blowing of the tins was found to be due to the *Bacillus cadaveris*. It was an anaerobe, spore-bearing, and motile. He would not mention its cultural reactions. It could stand enormous temperatures, 115° C. for ten minutes, or 117° C. for less than five minutes. Below 37° its activity was not manifest, but when taken to "hot" climates or kept in the warm incubator at that temperature these bacilli blew the tins in from seven to fourteen days, rendering the supplies unfit for food. It was the custom now to test new supplies by incubating samples as just mentioned for fourteen days. Owing to the enormous supplies required during the war, contracts had to be hurriedly completed, and the blowing of the tins pointed to the want of strict hygienic conditions in preparing the food. Since the war the supplies were inspected at the place of manufacture, and he had not found the *cadaveris* since. The term "food-poisoning" needed a clearer definition. He was not inclined to include *paratyphosus* infection under that heading, because the *enteritidis*, which was a true food-poisoner, existed in the food and formed its toxins, causing gastro-enteritis, and there it ended. But in the case of *paratyphosus*, the infection was conveyed by the food and caused symptoms which were almost indistinguishable from those of enteric fever. He agreed with Dr. Bainbridge that the *paratyphosus* could not be sufficiently differentiated by the agglutination test, and that the absorption test of Castellani was much more desirable.

Professor E. J. MCWEENEY (Dublin) said the paper had put the main issues with reference to the group of food poisoning bacilli in a very clear light. He had had some experience of those cases, both in ordinary hospital practice and in connexion with the Irish Local Government Board. If in every case of supposed typhoid infection the agglutination tests were applied to the serum, he believed paratyphoid infection would be found to be much more frequent in this country than was at present thought. Such cases possessed rather well-marked clinical characteristics—severe initial symptoms, a sharp rise of temperature to 103° F. or 104° F., headache, vomiting, and profuse diarrhœa. The subsequent clinical course was milder than that of ordinary typhoid. The serum of such cases had no greater effect on the genuine typhoid bacilli than mere immobilization. In those cases early examination of the blood or fæces would reveal the paratyphoid B bacillus. He had observed such cases which were diagnosed as typhoid of an unusual or septicæmic type. The fæces, examined in the acute stage, yielded paratyphoid B almost in pure cultivation. The paratyphoid B seemed to monopolize the bowel and drive the ordinary class of colon bacillus out of possession. In two such cases, when the fæces were examined in the acute stage, they were almost pure cultures of *paratyphosus* B. No one who had seen an epidemic of food-poisoning due to the Gaertner bacillus could doubt the ætiological rôle of that bacillus. The outbreak in Limerick, which he investigated, was a striking instance. When he reached the school where the outbreak was, there were nine dead out of seventy-four cases. A certain piece of beef was responsible for the outbreak. Portions of that beef were partaken of with impunity on a Thursday, but when made into a stew and eaten on the Monday it produced the violent outbreak. Meantime the Gaertner bacillus had had time to produce its toxin, some of those bacilli having escaped the roasting. He had no difficulty in isolating the Gaertner bacillus from the internal organs of the patients. The liver showed spots of characteristic appearance, yellow necrotic areas. The intestinal mucosa was swollen, pale, and pitted, but not ulcerated. He had observed post mortem a case of paratyphoid infection. There was moderate swelling of Peyer's patches, the lower 6 in. of the ileum presenting several small ulcers, and the cæcal aspect of the ileo-cæcal valve was ulcerated round. The spleen was enlarged. In the Limerick outbreak, the serum of the recovered cases agglutinated the ordinary typhoid bacillus almost as well as it agglutinated its own bacillus. The community of agglutinins between the Gaertner bacillus and the ordinary bacillus of typhoid, and the non-community of agglutinins between the paratyphoid B bacillus and the typhoid bacillus, constituted one of the leading means of distinction between paratyphoid B and Gaertner. Culturally they were identical, but serologically they were distinct. Most of the cases of Gaertner infection had been shown to be due to beef, yet the beef was not altered by its presence. Beef so infected was found to be swarming with bacilli, yet the appearance and odour were scarcely changed. The infection penetrated easily through large masses of meat. When investigating the Limerick outbreak, he spread a small quantity of a broth culture of the Gaertner bacillus on a slice of cooked cold beef, and superimposed other slices so as to form a heap 4½ in. high. One such heap was incubated, and the other he left at the ordinary temperature under a bell glass. In twenty-four hours the bacilli had spread from the lower slice almost to the top, at the low as well as the high temperature. Inoculation experiments had shown the strain in the Limerick outbreak was very virulent. Mice and rats were fed on the infected meat, and developed a disease which killed in a few days, the rats dying more quickly. In both kinds of animals the microbe was recoverable in enormous numbers from the internal organs and the muscular tissue. Dogs were immune; he fed one hungry dog with 1½ lb. of meat swarming with the Gaertner bacillus, but there were no ill-effects. The question as to the existence of toxin was a very important one. Into the ear vein of a bull calf four months old he injected (at 10 a.m.) 20 c.c. of a liquid containing 10 c.c. of twenty-four-hour broth cultivations of the Gaertner bacillus, and the scrapings of two agar cultivations of the same organism at the same age. Within twenty-fours hours the animal began to suffer from symptoms of dyspnœa, diarrhœa, and restlessness, and a little later appeared to be in pain. About five o'clock in the evening it fell to the ground, and was dead between six and seven. That was clearly a case of intoxication. He easily recovered the bacillus from the organs examined, and from the muscular tissue. The Gaertner bacillus seemed to resist the ordinary influences to which preserved meat was subjected, and it was resistant to pickling. The Germans had found it resisted even such strong brine as 25 per cent. salt solution, so that a piece of infected meat in a pickling tub had the power to spread the infection. In most cases of Gaertner infection it would be found that the source was the carcase of an animal that had been sick before it was slaughtered. That was so in the Limerick epidemic. In that case the meat supplied to the school was not killed by the butcher himself, as had been his practice, but had been bought cheaply from another butcher, who was found to have sold the meat at a loss, showing that he could not have regarded it as of high class. It was important that animals should be slaughtered in an abattoir, not in a private slaughter-house. He regretted that in Ireland private slaughter-houses were still used, and even the Dublin abattoir was not used as it should be—he supposed because there were fees to pay. There was nothing to prevent the carcase of an animal which had died naturally being taken to a private slaughter-house and placed on the market. An interesting point was the resemblance of the Gaertner bacillus to those which were used for the extirpation of rats. The lesson was that some discretion must be used with regard to Ratin in places like meat-safes and slaughter-houses. He had isolated from apparently faultless tinned meat micrococci, which, as they formed no spores, could not be expected to withstand the heating to which the tins had been subjected. Probably the heat did not penetrate right through, and so some parts were not reached. Dr. Bainbridge had put the issues with extreme clearness, but it would have been interesting if he had stated a little more about the absorption tests, and the results which he had thus obtained. Could anything be done in differentiating the members of that group by means of complement-fixation. Anaphylaxis might help further. It gave him great pleasure to appear once more before the Epidemiological Section. Six years ago he read a paper before the old Society, and still had pleasant memories of the way in which his contribution had been received.

Dr. HAMER desired to mention one or two difficulties many members felt with regard to "bacteriological differentiation." The table at the head of p. 52 of Dr. Bainbridge's paper was, of course, conciseness itself: the four organisms had been properly spaced one from another by the printer; but he asked was it not agreed that as a matter of fact there were not four but hundreds or thousands of strains of these bacilli, and there was great difficulty in drawing lines of separation between them. He read in a former paper by Dr. Bainbridge of the difficulty experienced by certain observers in applying the absorption test as a means of differentiating between strains of *Bacillus Aertryck* and strains of *Bacillus gaertner*. Nothing had been said about this difficulty with regard to varieties on the present occasion. Dr. Bainbridge relied upon fermentation tests, but there seemed to have been a good deal of criticism of fermentation tests recently. For example, there was the work of Dr. Blair Martin in connexion with Gram-negative cocci. Dr. Martin spoke of borderline organisms, and he said, "their existence, in the eyes of some, invalidates the claims of the types to specific rank." Again, Dr. Ainley Walker had found that there was a like difficulty with regard to the streptococci; here, too, the fermentation test did not afford a satisfactory means of distinction. Coming nearer home, Dr. Twort dealt two years ago with the group of organisms in question in this paper, and some of his conclusions had been reached quite independently by Dr. Burton Bradley in Australia. Dr. Twort sent up to the Lister Institute a culture of *Bacillus typhosus* in order that it might be shown beyond question that this organism had acquired the ability to ferment lactose. There appeared to be no doubt about the matter, for the organism had fermented lactose at Chelsea, and a paper written by Dr. Penfold had been published in which it was practically admitted that the laws of Nature had been found to operate within the walls of the Lister Institute itself. With regard to the absorption test, Dr. Bainbridge's voyage over that sea of difficulty had been made smooth by destiny; and he even claimed to have established a sort of two-power standard, for he was able to deal single-handed with two such eminent bacteriologists as Conradi and Hübener. A year or two ago, however, Dr. Bainbridge wrote a paper in which he discussed in detail some of the difficulties he had encountered in connexion with employment of the absorption test. Dr. Bainbridge made every effort to obtain pure and original strains of the *Bacillus paratyphosus* B. He applied to various institutions and to well-known bacteriologists, but alas! when he examined the cultures he found that he had to alter the names on the labels of many culture tubes, and some of the cultures were contaminated. Even his own special strain, the bacillus deposited at the Lister Institute as a standard for the guidance of other bacteriologists, had its eccentricities. The growth on agar and gelatine was rather luxuriant and somewhat moister than it should have been. But even granting all Dr. Bainbridge's contentions with regard to the absorption test, there were still difficulties. For example, there was a question as to compliance with one of Koch's postulates, discussed on p. 55. The author said that all true strains of *Bacillus paratyphosus* B were derived from cases of paratyphoid fever; whereas all the strains derived from food or food-poisoning were identical with the *Bacillus suipestifer*. But in the next paragraph he said that *Bacillus paratyphosus* B was "capable of giving rise not merely to paratyphoid fever, but also occasionally to acute gastro-enteritis." What had become, then, of Koch's postulate? He agreed with Dr. Bainbridge when he said "the question of the capacity of paratyphoid carriers to cause infection has so far received very little attention." He confessed to being impressed with Conradi's arguments. The fact that Conradi found *Bacillus paratyphosus* B in the blood of healthy persons, in scarlet fever patients, and in tuberculous patients, and further that it was widely distributed in Nature, and could be recovered from foodstuffs, made it difficult to escape Conradi's conclusion that the alimentary excretion of the paratyphoid bacillus was a physiological phenomenon. If this were granted, all must agree with that authority when he proceeded to say that to institute a campaign against healthy paratyphoid carriers was tantamount to engaging in a battle with windmills.

Dr. BAINBRIDGE, in reply, said he was much gratified by the reception of his paper. It was impossible, in the short time which remained, for him to deal with all the points which had been raised. With regard to the points which Dr. Hamer raised, it was necessary to have some bacteriological standards and some conventions. Those conventions were the ones which bacteriologists at the present day accepted, and they could not yet go beyond them. It must be accepted that when an organism exhibited certain characters it belonged to a certain type, to which a definite name was given, but it was necessary to avoid confusing names and facts. The tests which he had made had led him to the conclusion that there were two sets of bacilli: one which was called the *Bacillus paratyphosus* B, and the other which corresponded to the *Bacillus Aertryck*. With regard to the complement-fixation method, that had been done by Dr. Dean, and it complied with the absorption method for standard strains; also a large number of the German strains were submitted to Dr. Dean by him (the speaker) without his knowing what they were, and Dr. Dean's results corresponded with his own in all respects. He had omitted mention of both the clinical side and the question of toxin on the present occasion, because he wished to deal with the epidemiological point. With regard to the clinical question, he considered that all the bacilli of the group, in which he included typhoid, could cause symptoms of a very varied character. Everybody knew what ordinary enteric fever meant clinically, and so also with gastro-enteritis caused by the Gaertner bacillus. There were clinical stages intermediate between these, and any bacillus of the group might at one time or another cause symptoms like enteric fever or like food-poisoning, or some intermediate conditions. Even the typhoid bacillus could occasionally cause acute disease, and the bacillus of Gaertner could cause symptoms not unlike those of enteric fever. The *Bacillus paratyphosus* came between the two in that, while it more often was like enteric fever, yet now and again it produced symptoms like those of food-poisoning. Professor McWeeney pointed out some interesting facts about the acute onset of paratyphoid fever, and he (the speaker) believed that with some experience one could diagnose paratyphoid fever as a rule, even when apparently it was like enteric fever. First, in paratyphoid fever the onset was sudden, and often there were vomiting and diarrhœa, neither of which were usual in typhoid. Secondly, there was labial herpes in 50 per cent. of cases of paratyphoid fever. Thirdly, the initial temperature rose rapidly in paratyphoid, and gradually fell. The question of the presence of toxins in cooked meat had not yet been settled. In one of the best recorded cases of a food-poisoning outbreak, careful search was made for toxins, and none were found. The question of post-mortem infection of meat must arise in some cases where the processes carried out were not cleanly, but, apparently post-mortem infection was rare, because in England the Gaertner bacillus was but rarely found in the intestines of animals. Finally, he used the term food-poisoning in its clinical sense. It might appear to be an unscientific term, but he thought every member of the Section would understand what he meant by food-poisoning, though perhaps it would have been better to have spoken of acute gastro-enteritis.

Epidemiological Section.

February 24, 1911.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

Diseases of the Cow (excluding Tuberculosis) affecting the Milk in their Relationship to Human Disease.

By WILLIAM G. SAVAGE, M.D.

IT is common knowledge that milk is a frequent vehicle for the conveyance of the contagium of human disease. In the present paper it is only intended to deal with a small part of this wide subject. The whole question of tuberculosis will be omitted as demanding special and separate treatment, while the numerous epidemics in which the milk has been directly infected from a human source will also not be dealt with. Excluding these, we have left for consideration the part which the cow itself plays as a source of disease to man through the milk supply. The abnormal cow conditions may be divided into three groups:—

(I) General systemic disease of the cow without local lesions of the milk organs.

(II) General systemic disease of the cow with local lesions in or on the udder and teats.

(III) Local affections, with little or no general systemic disturbance.

GROUP I.

Excluding tuberculosis, the chief general diseases which have to be considered are the following:—

Anthrax.

Although this disease readily affects cows, it is generally considered that the danger of anthrax being spread by milk is a remote one. The affected animal usually dies within a few hours of infection, and, during its short illness, the milk secretion either is suppressed or, if present, is abnormal in appearance and rapidly decomposes. This is fortunate, since the anthrax bacillus has been recovered from the milk of infected cows. That the danger from anthrax-infected milk is not altogether problematical is shown by the recent outbreak in Chicago (June-August, 1910), when the milk supply of that city was seriously endangered by an outbreak of anthrax. The disease existed for about sixty days, involving twenty farms, and, approximately, 500 cows were exposed to the infection; eighty-seven cows died from the disease. The outbreak was spontaneous, spread rapidly, and required vigorous efforts to prevent its spread. Dairy inspectors from the city were put into the district involved, and by strenuous efforts discovered and quarantined every farm upon which anthrax was found, or was possibly present. All milk on these farms was destroyed, and great care was taken to prevent the sale of milk from infected districts. In consequence of these precautions none of the possibly infected milk reached the Chicago consumer at any time. One of the farmers upon whose place anthrax was discovered subsequently became infected with the disease and died.

Gaertner Infections.

Although cows are not infrequently infected with one or other member of the Gaertner group of bacilli, and widespread outbreaks of food-poisoning have resulted in consequence of the consumption of the infected meat, I have not been able to find any records of outbreaks due to the consumption of the milk of such animals. The probable explanation of this is that, with cows so affected, the milk secretion is either stopped or markedly altered.

Gastro-enteritis.

In cows affected with diarrhœa and other symptoms of gastro-enteritis, the milk is usually abnormal and decomposes rapidly. Such milk is probably liable to produce diarrhœa. The only known instance of which I can find record is that recorded by Follenius and Gaffky,[1] in which three persons at Giessen became very ill with malaise, headache, and diarrhœa after the consumption of raw milk from a cow suffering from hæmorrhagic enteritis.

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[1] *Deutsch. med. Wochenschr.*, Leipz. u. Berl., 1892, xviii, p. 297.

Septic Conditions.

In conditions such as puerperal septicaemia, septic metritis, &c., the milk is usually markedly altered and, no doubt, would be prejudicial. It is also the universal practice to avoid using for human consumption the milk of quite freshly calved cows.

Malta Fever.

Since cows are susceptible to infection with the *Micrococcus melitensis,* and this organism has been found in their milk, this disease is one which may be spread by cow's milk, but Eyre,[1] in a comprehensive account of the disease, does not mention this as an actual known source of infection.

GROUP II.

Of the second group, in which we have general disease of the cow, with local lesions on the teat and udder, the two commonest conditions are foot-and-mouth disease and cow-pox. Bovine scarlet fever may also be included here, but is more conveniently considered later with Group III. That foot-and-mouth disease in cows may be transmitted to man through the milk is attested by a number of cases and outbreaks of disease. One of the most interesting is the outbreak recorded by Robinson at Dover in 1884.

Whether cow-pox affects the milk and causes infection in man is doubtful. The milk of such cows is usually said to be unfit for food, but there appears to be little or no evidence as to its harmfulness, although the milkers' hands are apt to be infected. Stott[2] has described an outbreak of cow-pox in a large herd supplying milk to Brighton. At the time it was discovered by the Medical Officer of Health that most of the cows were, or had been, infectious, and the milk had been sold for more than three weeks while the cows had been ill without any illness being traced to its use. Twelve of the seventeen milkers, however, became infected and suffered from vaccinia. After it was discovered the milk was allowed to be used after pasteurization.

GROUP III.

The most interesting group of conditions are those in which the essential lesions are in connexion with the milk-producing organs. They take the form of inflammation of the udder (garget or mastitis) and the various conditions described under the term "ulcerated teats."

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[1] Article "Maltafieber," Kolle and Wassermann, "Handbuch der pathogenen Mikroorganismen," Jena, 1907, I Ergänzungsband, p. 601.

[2] *Journal of the Royal Sanitary Institute*, 1911, xxxii, p. 20.

Mastitis in Cows.

I propose to lay before you the available evidence as to whether this condition is a source of human disease. Such evidence is derived from two sources—epidemiological inquiry and pathological investigation. As regards direct evidence of disease causation, we are practically confined to *outbreaks* of disease. The difficulty of connecting mastitis in a cow and individual cases of illness is very great, since in the absence of groups of cases with a common milk supply it is almost impossible to have reasonable grounds for associating the two conditions. There are, however, a few instances on record in which the consumption of such milk has caused illness in small groups of persons. It may be accepted as a reasonable supposition that if such mastitis-infected milk is capable, in some cases, of causing extensive outbreaks of disease in man, there must be a number of instances in which it is capable of causing individual illness in susceptible persons, although an epidemic may not result.

Epidemics of illness ascribed to this cause have nearly always been of the nature of outbreaks of septic sore throat, although there are one or two in which gastro-enteritis has been the predominant condition. Holst,[1] for example, records four outbreaks of acute gastro-enteritis from the use of raw milk, and in three of these he considers the cause to have been the milk of cows suffering from mastitis streptococci. In several cases the milk of these diseased cows, after being stopped, was again mixed with the other milk and sold (owing to change in the milkers), and diarrhœa appeared amongst the consumers on the same day. Holst also swallowed 200 c.c. of a milk culture of the streptococci isolated from one of the mastitis cases which had stood for a few hours at 37° C., with resulting body pain and some diarrhœa and vomiting.

All the outbreaks of this nature which I have been able to find are summarized in the accompanying appendix. Mastitis in cows is not an uncommon affection. I have been unable to find any figures giving its actual incidence in the country generally, but the following table for London cowsheds, from Sir Shirley Murphy's Annual Report for 1908, is very instructive:—

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[1] Baumgarten's *Jahresbericht ü. . . pathogenen Mikroorganismen* (1895), 1897, xi, p. 52.

TABLE I.—SUMMARY OF THE REPORTS OF THE VETERINARY INSPECTOR FOR THE YEAR 1908.

Details of examination	Jan. 11 to March 15	March 16 to May 26	May 27 to July 31	August 1 to October 4	October 5 to Dec. 5
Total number of cows examined	3,521	3,283	3,476	3,343	3,565
Affected with disease or defects of the udder ...	230	223	213	213	258
Affected with tuberculous disease of the udder	3	3	2	5	3
Suspected cases of tuberculous disease ...	2	1	—	—	—
Subjects of acute mastitis	23	39	27	29	40
Affected with chronic induration of the udder	7	13	12	11	4
Atrophy of one or more quarters	141	117	123	127	142
Injuries, abscesses, simple eruptions, strictures, and obliterations of milk ducts	16	27	22	18	32
Hypertrophied udders without induration ...	2	2	3	1	1
Giving milk of poor quality (dried-off cows) ...	36	21	24	22	36

This table shows that, taking the mean of the different inspections, 0·9 per cent. of the cows were at the time of inspection suffering from acute mastitis, while 3·7 per cent. showed atrophy of one or more quarters. The latter condition is nearly always due to a previous attack of acute mastitis. These mean results varied somewhat, but not very widely, in other years, as shown in the following table, compiled from the annual reports:—

TABLE II.—PERCENTAGE.

Year	Affected with acute mastitis	Atrophy of one or more quarter
1904	2·2	7·7
1905	0·73	3·9
1906	0·57	3·8
1907	0·27	3·7
1908	0·9	3·7

These figures and the general frequency of mastitis in cows contrast markedly with the recorded prevalence of outbreaks of sore throat alleged or proved to be due to infected milk of a cow suffering from mastitis. Even when a large allowance is made for unrecognized and unrecorded outbreaks the discrepancy is only slightly diminished.

Pathological Investigations upon Mastitis in Cows.

We owe the first definite work upon the bacteriology of this condition to Nocard and Mollereau, who in 1884 isolated a long-chain streptococcus from the udder secretion of ten cows suffering from contagious mastitis. Since that date numerous investigations have been carried out by Hess and Borgeaud, Kitt, Guillebeau, Zschokke, Steiger, Gröning, and others. These investigators showed that this disease might be set up by different organisms. Of these, apart from the chronic varieties due to tuberculosis and actinomycosis, the chief organisms were streptococci, staphylococci, and varieties of *Bacillus coli*. In all these investigations there is but little differentiation of the organisms isolated, and the characters given quite fail to separate them from the saprophytic streptococci and staphylococci not only present in manure, sewage, air, &c., but also in the milk drawn direct from the udders of perfectly healthy cows. They are, further, in no way differentiated from the disease-producing streptococci and staphylococci of man. These investigations unfortunately give no assistance whatever in solving the problem under consideration—the possible pathogenicity of bovine mastitis to man. My researches[1] for the Local Government Board are the only investigations of which I am aware which bear upon this problem, so I trust I may be pardoned for dealing with them in brief detail.

Out of thirty-six cases of mastitis in cows investigated, 68 per cent. were due to streptococci, 16 per cent. to staphylococci, 3 per cent. to *Bacillus coli*, 3 per cent. (one case) to *Bacillus tuberculosis*, and in 10 per cent. (three cases) the results were doubtful. If the three doubtful cases are excluded, the percentage due to streptococci rises to 75. A large series of tests, including the production of acid in certain sugar-alcohol media, were used to differentiate the streptococci. It was found that 80 per cent. of the streptococci isolated could be grouped as forming a common type which I called *Streptococcus mastitidis*. This is a long-chain form growing rapidly in broth, forming a coherent deposit, but leaving the upper part clear. It grows upon gelatine without liquefaction, produces acid in milk, clotting it within three days, gives no neutral red reaction, and produces acid in lactose and saccharose media, never in mannite, and not usually in salicin, raffinose, or inulin. It is non-pathogenic to mice. It is of considerable significance that in several other cases quite different streptococci were isolated.

In view of the possible relationship of mastitis in cows to human disease it was a matter of great importance to consider how far the causal organisms of mastitis were present in quarters which clinically appeared quite unaffected. To investigate this question, the milk of all four quarters was, as far as possible, examined in every case. Excluding cases in which all four quarters were attacked, in 84 per cent. of the cases due to streptococci identical organisms were isolated in one or more of the healthy quarters. In some instances they were present in only one healthy quarter, in others in all of them. It should be noted that not only were these streptococci found in quarters which appeared sound when examined by a veterinary surgeon, but in quarters which frequently showed no evidence of any inflammatory reaction, as demonstrated by the extremely sensitive cytological examination. The milk appeared to be perfectly healthy to the naked eye, although these streptococci, identical with those in the affected quarters, were present. As a rule, the healthy quarters did not become subsequently involved.

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[1] Annual Report, Local Government Board (Report of Medical Officer), 1906-7, xxxvi, p. 205; 1907-8, xxxvii, p. 359; 1908-9, xxxviii, p. 294.

From a public health point of view these results are of considerable significance. In cases of mastitis in cows it is a quite common practice to use the milk from the seemingly unaffected quarters, although the secretion of the diseased quarters is recognized as unfit. The results obtained show that in most cases the farmer is thus adding to the vended milk very large numbers of streptococci indistinguishable from those causing the mastitis.

The primary problem from the human standpoint is whether the streptococci found in cow mastitis are pathogenic to man. To elucidate this, further investigation was required. Careful comparison was therefore instituted between the streptococci found in these two conditions. The streptococci found in sixteen cases of human sore throat were carefully investigated. Two chief types were found, confirming the results previously found by Andrewes and Horder. The most prevalent type was the variety which they called *Streptococcus anginosus*, while the other was identical with *Streptococcus pyogenes*. Comparing the two types most commonly met with in the bovine and human conditions—the *Streptococcus mastitidis* and *Streptococcus anginosus*—it was found that, morphologically and culturally, they were quite indistinguishable even when the different sugar-alcohol tests were employed. In addition, the commonest cultural deviations from the type were the same in both cases. As regards their pathogenicity towards the lower animals, however, they show distinct differences. *Streptococcus mastitidis* possesses low virulence: Occasionally a local abscess is produced, but, as a rule, no pathogenic action is manifested towards mice or guinea-pigs. The *Streptococcus anginosus* type, on the other hand, is possessed of considerable pathogenic action, subcutaneous inoculation into mice frequently causing the death of the animal. The distinction is one of degree rather than kind, and otherwise the close similarity is very interesting. Further comparison is only possible along the lines of their specific properties.

Can the mastitis streptococci, by inoculation upon the human throat, set up inflammatory action? Will the udders of cows when inoculated with streptococci from cases of human disease become infected and mastitis result? Both problems were experimentally investigated. The second question was studied in a series of experiments upon goats. Twenty-two separate experiments were carried out in which the teats were infected with pure cultures of different organisms. The results in the twenty experiments in which pure cultures of recently isolated streptococci were used were as follows: The three streptococci from mastitis cases in cows all set up mastitis in goats. Of two streptococci from sore teats one set up a very severe mastitis, but the other was without effect. A streptococcus very common in, and isolated from, cow-dung also produced no visible effect. Of thirteen streptococci of human origin, one from a healthy throat, six from cases of sore throat (two being scarlet fever cases, and four ordinary tonsillitis cases), and four isolated from other human diseases, all failed to produce mastitis in the goat. In the remaining two cases, both with streptococci not obtained from sore throats, distinct inflammatory reaction and some degree of mastitis resulted. One of these streptococci was derived from a case of Ludwig's angina, and the other from a case of acute epiphysitis.

The results are distinctive. Comparing the human and the bovine groups, it was found that, although often morphologically and culturally they were indistinguishable, they showed a wide divergence when their action towards animals is considered. The bovine mastitis type is non-virulent towards mice and other rodents, but possesses in marked degree the power to cause udder inflammation in goats. The streptococci from human sources possess a considerable virulence towards mice, but as regards the strains isolated from sore throats absolutely, and as regards those from other sources in most cases, they were unable to originate mastitis in goats.

These results suggest that an essential difference of functional power separates the types, and that under ordinary conditions the *Streptococcus mastitidis* is not a cause of human disease.

That the *Streptococcus mastitidis* is not a cause of human disease was also demonstrated by direct experiment. In two separate experiments I directly infected my own throat with massive doses of this streptococcus, isolated only a day or two previously from cases of cow mastitis. On both occasions no ill-effects were experienced generally or locally, and the streptococcus could only be recovered with extreme difficulty from my throat, and this although the isolation was undertaken only two and three days respectively after the throat inoculation, and although that inoculation was massive in amount.

We have here the broad pathological fact that the great majority of cases of bovine mastitis are due to an organism which is not harmful to man. This is in accord with the fact already recorded, that while bovine mastitis is common, sore throat and other septic outbreaks from milk are rare. The fact that certain outbreaks do appear to be due to a cow with an infective mastitis is, I believe, also in line with my pathological results. While I found that *Streptococcus mastitidis* was the usual organism of bovine mastitis, it was not the only type of streptococcus associated with this condition. In one case, for example, a streptococcus of extremely high virulence to rodents was found to be the cause of the mastitis, and this organism in many other ways was quite distinctive from the *Streptococcus mastitidis*, and may well have been potentially virulent to man. If we accept the view that the ordinary type of bovine mastitis is due to organisms non-virulent to man, but that in certain uncommon cases this condition is caused by streptococci highly pathogenic to man, it offers a complete explanation of both my bacteriological investigations and the epidemiological facts with which we are acquainted.

From the practical point of view, it is well to remember that the pathogenic and non-pathogenic types of bovine mastitis are not clinically distinguishable. A hypothesis which I wish to suggest as the origin of these strains virulent to man is more conveniently deferred until the other group of abnormal bovine conditions has been considered.

Ulcerated Teats.

In addition to the diseases of cows already described, in which the teat eruption is part of a general constitutional disturbance such as cow-pox, not infrequently teat ulcerations and eruptions are met with in cows which seemingly are in perfect health. The cowman speaks of them as "sore teats," usually applies some local treatment, and neither notifies the Medical Officer of Health nor calls in a veterinary surgeon to treat them. When a veterinary surgeon is called in or meets with the condition, he frequently speaks of it as spurious cow-pox, and attaches no significance to the condition. In the considerable number of cases which I have come across the cowkeeper has always milked the cows as usual or added the milk to the rest for sale. If ulcerated teats have any relationship to human disease, we can postulate three possible ways in which the effect may be exerted :—

(*a*) The ulceration may be part of a general disease of the cow, and through the teat ulcerations the milk may become infected. This has been shown to be a cause of human disease in foot-and-mouth disease, and bovine scarlet fever has been advocated as another example.

(*b*) The teat and udder conditions may be purely local, but due to streptococci or other organisms pathogenic to man.

(*c*) The teat condition may be due to purely bovine disease or be traumatic in origin, but become secondarily infected by organisms pathogenic to man.

In other words, we may theoretically have a constitutionally infective cow, a local actively infective cow, and a local passively infective cow.

Systematic bacteriological investigations of sore-teat conditions do not seem to have been made. I have investigated a few cases. In most instances long-chain streptococci very similar to the *Streptococcus mastitidis* were found. Of two, used to infect the interior of the teats of goats, one set up a very severe mastitis in the goat, while the other was without effect. In three cases of ulcerated teats a very similar long-chain streptococcus was isolated from the milk, but the ulcerated teats were not bacterially examined.

I have personally only met with one outbreak in which ulcerated teats on the cows were possibly ætiologically connected. In this case a small outbreak of sore throat occurred in one of the Army barracks at Colchester. The Army Sanitary Officer came to the conclusion that it was caused by the milk supplied. It was confined to the barracks supplied from one special milk supply. He found immense numbers of long-chain streptococci in the milk. When he communicated his suspicions to me we at once visited the cowsheds, and found three cows, one markedly affected and two slightly, with ulcerations on the teats. They had crusted over and showed a black scab. We could find no evidence of illness amongst the milkers. The cessation of the cases coincided with the exclusion of the milk of these cows from the general supply.

That teat ulcers may be secondarily infected has been demonstrated in the case of diphtheria, and two outbreaks may be briefly mentioned. Dean and Todd[1] in 1902 described a small outbreak in which undoubted diphtheria bacilli were isolated from the teat lesions and from the milk. In this outbreak certain individuals suffered from diphtheria and others from sore throat, probably diphtheritic. They obtained their milk supply from two cows. The cows were suffering from papules and ulcers on the udders and teats, which commenced about ten days before the outbreak. One of the cows also showed mastitis in one quarter, the fluid secreted being scanty, ropy, and semi-purulent in appearance. True diphtheria bacilli which were virulent were isolated both from the lesions and from the milk. A healthy cow milked immediately after the diseased cows and by the same milker developed vesicles upon the teats, but neither in the vesicles nor in the ulcerative stage of the disease could diphtheria bacilli be demonstrated. The investigators also showed that in calves infected with the eruptive disease no diphtheria bacilli could be demonstrated, while injecting a calf with 10,000 units of diphtheria antitoxin did not prevent the vesicular eruption. The authors came to the conclusion that the lesions in the cow were not due to the diphtheria bacillus, but that the latter was a superadded infection.

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[1] *Journal of Hygiene*, Camb., 1902, ii, p. 194.

Ashby, in 1906,[1] described an extensive outbreak of diphtheria, affecting seventy-five persons living in forty-three houses; forty-two of these houses were supplied by two milkmen. Of the cows of one dairyman (Z.) all the teats of two cows were badly ulcerated and the teats of three other cows were affected to a lesser degree. Six of the cows of the other dairyman (Y.) were affected with an ulcerated condition of the teats. From swabs rubbed into the ulcers upon the teats of the two worst of Z.'s cows diphtheria bacilli were isolated. They were fully virulent for guinea-pigs. Cultures from a sample of milk showed only non-pathogenic diphtheria-like bacilli. Ashby adds: "Whether the eruptive disease of the teat was a specific diphtheritic infection of the cow, or whether there was a specific contagious eruptive condition apart from diphtheritic infection cannot now be told, but certain it is that the diphtheria bacillus was present in the pathological lesions of the cow."

Klein's experimental results in favour of the existence of bovine diphtheria have not been confirmed by either Abbott or Ritter.

Ever since the classical outbreak of scarlet fever at Hendon in 1885 the existence of bovine scarlet fever has been a debatable subject. The subject has again become of practical interest since the outbreak of scarlet fever in 1909 in certain parts of London and Surrey has been ascribed by Hamer and Jones to a possibly bovine source.

Time will not allow me to deal in any detail with the outbreaks which have been ascribed to bovine scarlet fever. After critical consideration of the records I have personally come to the conclusion that this—the existence of a constitutional disease, bovine scarlet fever—cannot be accepted as proven. As an explanation of some of these outbreaks I would suggest the hypothesis that we are dealing, not with a cow suffering from an infective disease, *bovine scarlet fever*, but with a passively infected cow or cows. The theory which I wish to advance is that when the cow is a source of scarlet fever or other septic disease it is because it is acting as a carrier of organisms of human origin, often in a purely passive capacity, but sometimes associated with active but local disease caused by the human infecting organism. It will be seen that this hypothesis, which I believe as a coherent whole has not been advanced before, implies the acceptance of two separate conclusions:—

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[1] *Public Health*, 1906, xix, p. 145.

(*a*) That the cow may be a source of human disease not because it is constitutionally infective, but because it is acting as a carrier of human infective organisms.

(*b*) That disease of the milk-producing organs of cows is only likely to be harmful to man when the causally associated organisms are of human origin or when human organisms are superadded as a secondary infection.

Of course, tuberculosis is not included in this hypothesis. The first conclusion is more capable of proof than the second, and may be accepted without the second being admitted.

Briefly stated, I regard the bovine udder and teat lesions, as commonly met with, as of purely bovine origin and, as such, harmless to man. Occasionally, either as an invasion superadded upon the original bovine lesions or as a primary infection of the milk organs, there is a local infection with organisms of human origin. In such cases the conditions present may be decidedly prejudicial to man. In other words, the cow, in this class of infections, is only potentially pathogenic to man when it acts as an active or passive carrier of organisms of *human* origin.

I would submit that this hypothesis offers the best explanation of the facts with which we are acquainted, and as such I advance it for your consideration and criticism. We may now consider how far the known facts fit in with this hypothesis:—

(1) It is clear that the great majority of bovine udder and teat lesions are not harmful to man. As evidence of this we have in the first place the striking disproportion between the prevalence of udder and teat lesions and the recorded outbreaks ascribed to these conditions. Even allowing for a large number of unrecorded outbreaks and numerous

individual cases of illness, the disproportion is very marked. In the second place, my bacteriological investigations recorded above have proved that the commonest type of mastitis is not harmful to man in a healthy condition, and probably not when debilitated. The evidence of the innocuousness of the ordinary type of bovine mastitis is added to by the demonstration of the causal streptococci in abundance in the milk of the clinically unaffected quarters—milk which must, therefore, have been repeatedly added to the vended milk.

(II) There is reliable epidemiological evidence connecting at least some sore throat and other human disease outbreaks with affections of the bovine milk organs. The descriptions of these outbreaks in the appendix may be referred to for such evidence.

(III) There is clear evidence that organisms of undoubtedly human origin *can* in certain cases infect the milk organs of bovines.

The diphtheria instances already recorded offer the most striking evidence of this, because this organism can be readily identified; if the scarlet fever organism was identified with equal facility, I believe similar evidence for that disease would be forthcoming. The outbreak of Dean and Todd is particularly conclusive, since they proved that there was a separate bovine eruption, with the diphtheria bacilli present as a second super-added infection. Klein and Abbott have both demonstrated that diphtheria bacilli, inoculated into calves and cows, could be recovered at the site of inoculation for some time.

Certain of my goat-inoculation experiments are very interesting in this connexion. The streptococci of human origin, for the most part, died out fairly rapidly when implanted into the teats of goats, but only relatively so; one strain, for example, living for as long as twenty-six days after inoculation. One striking exception was, however, met with. In the case in which the streptococcus isolated from the spleen of a boy who died of acute lymphadenitis was used to infect a goat's teat, it did not set up any inflammation, but survived as a harmless (to the goat) saprophyte for the whole duration of the experiment, extending over seven months. During this period it had not caused any inflammatory changes or alteration in the appearances of the milk, nor had its biological characters become altered except in one particular, and that inconstantly. This streptococcus had originally caused a secondary infection in a boy, with marked elevation of temperature, and, no doubt, largely contributed to his death. We have only to postulate the retention of its virulence for a while, and this goat's milk, if drunk (and it was absolutely normal in appearance), would be a potent disease factor.

It may be suggested that, if we accept this view, the cow would be a much more potent agent for the spread of disease than it now is, seeing the ease with which organisms of human origin can be implanted under present conditions upon cows' teats and udders. This is not likely since, as pointed out, human streptococci for the most part will not grow in or on a bovine habitat. Incidentally I may remark that my results showed that the streptococci of bovine mastitis died out with extreme rapidity in my own throat, although the infection was extremely massive in character.

(IV) The above hypothesis offers the best explanation of a number of facts in connexion with certain milk-borne sore throat and scarlet fever outbreaks, which otherwise it is extremely difficult to explain. For example, a noticeable feature of some of these outbreaks is the prolonged period during which the milk was serving as a vehicle of infection. If we have a purely human source of infection we should rather expect a very sudden outbreak of short duration, since a human source is not likely to be daily infecting the milk over a long period. In the Guildford outbreak, for instance, the milk was infectious for some seven to eight weeks. While the infection probably started from a human source (the farmer), we cannot, as Pierce points out, account for the prolonged infectivity of the milk, unless we postulate a continuous infection of the cows' milk organs from this human source. The farmer was disabled and unable to milk, &c., for at least a month.

The much discussed Hendon outbreak, ascribed to bovine scarlet fever, may be taken as a good illustration of an outbreak in which the conflicting views can only be reconciled by my hypothesis. For example:—

(*a*) The Hendon cows came from Derby. The opponents of the bovine scarlatina conception showed that thirty cows were purchased at the Derby market, but only three went to the Hendon farmer. These other cows showed in many cases sore teats, and spread the disease amongst the herds with which they were mixed. "From the first day to the last, while the milk from these cows was being drunk, no case of scarlatina was heard of among the customers of the dairy."[1] Professor Brown maintained that the Hendon disease was not rare, but comparatively common in milch cows. On my hypothesis we can accept the view that this outbreak of teat and udder ulceration was very widespread. It was due to a bovine disease of an infectious character, having nothing to do with scarlatina. Human cases of scarlatina were, therefore, only traceable from such cows when, as in the three sent to Hendon, superadded scarlatinal organisms were present.

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[1] Report by Professor Brown, Agricultural Department, Privy Council Office, 1888.

(*b*) One of the strongest arguments against a purely human origin was the prolonged infectivity of the milk. The opponents of the bovine origin showed the possibility of human infection. It, however, seems to have been established that none of the milkers or others who came into regular contact with the milk, or the cows, suffered, or had suffered from scarlet fever. The only human infection possible was at the most intermittent, and probably only occurred once or twice. On a well-managed farm, such as this farm was shown to be, to produce prolonged infectivity we must postulate a human factor at work all, or nearly all, the time. There is admittedly not the slightest evidence of prolonged human infection. If, however, we assume that in some now undiscoverable way (possibly in transit to the farm) the already present ulcers and sores of the three cows were infected with the organism of scarlet fever this difficulty vanishes.

(*c*) The most striking fact established by Sir William (then Mr.) Power in favour of the bovine theory was the definite association of scarlet fever cases with the movements of these three cows. This is obviously explained by the hypothesis of their being locally infective.

(*d*) The opponents of the bovine theory are confirmed in their view by the continued rarity of such conditions. On my hypothesis the rarity of such outbreaks is easily understandable. We have three factors which have to be in operation at once: cows suffering from a bovine disease of the teats and udders, which will allow a secondary infection with the organisms of scarlet fever; the existence of cases of scarlet fever; means of bringing the last condition into intimate relationship with the first.

Klein's results are explainable on the supposition that he was working with the original bovine disease material, and with this he was able to reproduce ulcers and vesicles in inoculated calves. The suggested hypothesis offers a complete explanation of all the essential facts of the Hendon disease, and reconciles the conflicting views.

Similarly the Glasgow outbreak[1] in 1892, said to be due to the Hendon disease, can best be explained on this hypothesis, particularly as regards the prolonged infectivity of the milk. In this outbreak Klein's results definitely show that an underlying condition of true vaccinia was present in the cows, as well as what he considers to be the virus causing the outbreak.

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[1] *Brit. Med. Journ.*, 1892, ii, p. 666.

(V) There is also another fact, which may, or may not be germane to this subject, but which is very interesting. If the records of milk-borne outbreaks of septic conditions, and especially scarlet fever, are carefully studied, it will be found in a considerable number of them, when the condition has been looked for, that teat ulceration, mastitis, or other local lesions of the milk organs, have been found to be present. This is inclusive of epidemics in which a definite human source of infection has been traced and accepted as a sufficient cause of the outbreak, and where no question of bovine infection has been raised. I have carefully examined hundreds of cows, and although I am unable to give exact figures, I am convinced that the proportion of cows with ulcerated teats is decidedly higher in the herds of cows from farms implicated in milk epidemics of scarlet fever than for ordinary farms. This higher incidence of lesions may be accidental, but it would seem more probable that a relationship exists, and that the sore teats are not infrequently a nidus for passive infection. That is, given a human milker, actively or passively harbouring scarlet fever organisms, he may, or may not, infect the milk supply, but is more likely to do so if the infection is in the first place conveyed to ulcerated teats, and in this way infects the milk. For one thing, the dose is likely to be larger, and also decidedly less intermittent.

(VI) There is some analogy with tuberculosis. In this disease we have to deal with a bovine and a human tubercle bacillus, both from the same stock, modified by a different animal environment. In children drinking milk infected with bovine tubercle bacilli infection apparently results in only a small proportion of the cases so subject to infection. When infection results, we have bovine bacilli in a human habitat. I believe in the same way human streptococci and other organisms may infect a bovine habitat, and as such serve as a source for human infection.

APPENDIX.

(I) MILK-BORNE SORE THROAT OUTBREAKS.

(1) ABERDEEN [1], 1881.—About 300 cases with three deaths. Symptoms characteristic. Sudden onset, with severe rigor or series of rigors followed by fever. Inflammation of throat and tonsils with swelling of the lymphatic glands in the neck and above the clavicle. Fever usually subsided after two to three days, leaving the patient very weak and prostrate. Outbreak clearly spread by milk. The total number of families supplied with milk from the incriminated supply was 110, and of these at least ninety families were affected. No cases occurred in any families obtaining their milk from other sources. Outbreak ceased when supply of milk stopped. Outbreak ascribed on what would now be considered as quite insufficient grounds to the water supply of the dairy. No mention made as to the condition of the cows or of any examination of, or cases of illness amongst, the milkers.

(2) RUGBY [2], 1880 (reported by Dr. George Wilson).—Over 100 cases in all. Mostly among the boys at Rugby School. Cases of sore throat, but clinical particulars not given. Outbreak clearly spread by milk. Sudden onset in the three (out of eight) boarding-houses connected with the school supplied from the one milkman; in fifteen out of the thirty-seven families which he also supplied in the town there were one or more cases of the disease. All the cases were supplied with milk from the one milkman. Onset, March 16 and March 17. No cases of suspicious illness on the four farms from which the supply was obtained. Wilson came to the conclusion that the probable cause was a cow suffering from garget on one of the farms.

(3) DOVER [3], February, 1884 (reported by Dr. M. K. Robinson).—Within four days 188 persons attacked; in all 205 persons affected. Symptoms, those of local inflammation of the throat, enlargement of the lymph glands of the neck, and in some cases vesicular eruptions. Erysipelas was also met with: four fatal cases. In nineteen out of the forty-two affected streets every house supplied by the implicated milkman was visited, while in the remaining twenty-three affected streets, out of eighty-six houses supplied by this milkman fifty-one were invaded by the disease. The number of persons attacked in each house stricken averaged 2·5 per house. The "nursery," or other special milk, supplied by the same milkman was not a cause of infection. The milk was obtained from four farms. On one of the farms it was found that foot-and-mouth disease had broken out on January 14, and that milk from some of the affected cows was delivered to the Dover dairyman and distributed, after mixing with the other milk, to his customers. Strong confirmatory evidence of the relationship of the conditions found on this farm to the outbreak was obtained from the fact that on two separate occasions the farmer sold his milk and cream to another

milk purveyor, amongst whose customers a second simultaneous outbreak occurred. No information given as to illness amongst the milkers.

(4) CRAIGMORE [4], 1890.—The number of cases which came under medical treatment was about eighty. In March and April, 1890. Symptoms were those of marked inflammatory hyperæmia and swelling of the throat, enlargement of the submaxillary glands and glands of the neck, with high fever and marked constitutional disturbance. Some of the cases were complicated with erysipelas. Three deaths occurred, all children. The evidence showed that the outbreak was spread by the milk from a particular farm. The period of incubation was usually three to four days, but occasionally two days. In some cases the onset of the disease after consuming the milk was very striking. For example, a Glasgow family went to Craigmore from Glasgow on March 28, and started using the milk. On April 1 the whole family were attacked, including the older children who had returned on March 31 to Glasgow to school. Two members of the family who remained in Glasgow and did not drink the milk were not attacked. The milk supply was stopped on April 2 and the cases seem to have ceased almost at once. The outbreak commenced about March 17, but the farm was not visited until April 3. The Medical Officer of Health was told there was no illness among the people or the cows. On further questioning, it was elicited that one of the dairymaids had had a sore throat for a few days, but it was so slight that she had been away from work for only half a day (March 17), this being the same day that the outbreak began in at least three families. On April 4, two days after the milk was stopped, erysipelas appeared on the face of the farmer. A veterinary surgeon examined all the cows and gave a certificate that they were in perfect health and condition. It is not clear if he was engaged by the farmer or on behalf of the local authority. The milkmaid was suggested as the cause of the infection of the milk. Apparently no bacteriological investigations were carried out. A cat and a dog belonging to one of the sufferers, which had been fed a good deal on the implicated milk, became very ill at the same time as their owners, and nearly died from what the veterinary surgeon who attended them called "severe inflammation of the throat."

(5) FINCHLEY [5], November, 1894 (reported by Professor H. Kenwood).—Total number of cases not known. The first twenty-four cases were explosive in onset, all occurring within forty hours. The chief symptom was sore throat, which was not diphtheria, although the throats in many resembled those found in that disease. The suspected milk supplied 94 per cent of the total number of houses which were infected, but only 17 per cent. of the total houses in the district. The disease affected persons in good-class houses with scarcely an exception. On the whole children were not mainly affected. The only source of infection which could be found was that on the farm supplying the incriminated milk were three cows evidently out of health, and in each case suffering from ulcerations of the teats, while one of the cows had a small chronic abscess in the udder. All the throats of those who milked the cows were examined by Dr. Kenwood and found by him to be healthy.

(6) ANGLESEY [6], February, 1897 (reported by Dr. C. Grey-Edwards and Mr. W. D. Severn).—Fifteen cases. No deaths. Symptoms were those of follicular tonsillitis. The evidence implicating the milk rests upon the fact that all the cases had a common milk supply derived from the farm upon which the first three cases (two children and a servant) occurred, and that three children between two and three years of age escaped apparently because they received only boiled milk. The cases occurred between February 12 and March 1. The cows were examined some time after the end of February (date not given) by a veterinary surgeon, who failed to detect anything wrong. When the milk of the individual cows was examined, the milk of one contained staphylococci and streptococci. While the outbreak may reasonably be ascribed to the milk, the evidence supplied does not show the cause of infection, whether human or bovine. We are not told if the servant on the farm had anything to do with the cows or milking utensils.

(7) SURBITON [7], 1897 (reported by Dr. Coleman).—Thirty cases at least known and probably others. Symptoms those of sore throat. Cases supplied from one milkman. One of the milkers found to be suffering from tonsillitis with suppurating whitlows on both hands.

(8) HACKNEY [8], 1900 (reported by Dr. J. King Warry).—The disease was prevalent during nearly the whole of April and the first week in May. Outbreak extensive; 151 known cases in eighty-eight households. The symptoms were those of severe septic sore throat with ulcerated tonsils, enlarged cervical glands, high temperature, and great prostration. In all cases convalescence was protracted. In one case acute septicæmia supervened, followed by acute septic pneumonia and death. There was a marked tendency for multiple cases to occur in families, and while no age or sex was exempt, a very large number of the sufferers were children. The facts ascertained by Dr. Warry (Medical Officer of Health) proved conclusively that the vehicle of spread was the milk of a certain milk vendor (X.). The returns of cases by medical men showed that 138 cases in seventy-five households were supplied with milk by X., and the remaining thirteen cases with milk by seven other dealers; that is, over 85 per cent. of the households in which sore throat illness occurred were supplied with milk by X. It was shown that X. supplied a very much smaller proportion of the houses than 85 per cent. House-to-house inquiries also showed that the persons attacked were those who consumed X.'s milk. There is no evidence in the report as to how the milk became infected. The bacteriological examination threw no light upon the origin of the disease. The vendor X. had a few cows of his own, but also received his milk from seven different country dealers. The cows of the dairyman were examined by a veterinary surgeon, who found them to be in good health. There does not appear to have been any investigation or inquiry as regards the outside farms either as to human illness on the farms or as to the condition of the cows.

(9) BRIGHTON [9], November, 1901 (reported by Dr. A. Newsholme).—A small outbreak of sore throat involving eighteen cases, and followed by a

number of cases of scarlet fever. It is not clear how far the sore throat cases were aberrant scarlet fever cases, and Newsholme considered that some of them were of this nature, occurring among girls unprotected by a previous attack of scarlet fever. The symptoms were those of sore throat with greyish exudation on the tonsils, high temperature, and general constitutional disturbance. By careful inquiry the outbreak was traced to a particular milk supply. The cause of infection appears to have been unrecognized cases of infectious sore throat in several families connected with the farmer who supplied the milk. The relationship was not, however, completely established. When the facts were investigated early in December (the cases were mostly in November) no evidence of udder disease was found, and fairly frequent veterinary inspections of the dairy had been made.

(10) BEDFORD [10], 1902 (reported by Dr. W. G. Nash).—The number of cases was forty-two, occurring in twenty-two families. In addition there were some milder cases not medically attended. The first case occurred on June 27. The symptoms were marked swelling and redness of the tonsils, fauces, palate and uvula, with exudation on the tonsils, marked constitutional disturbance, and considerable temperature. In every instance the milk was obtained from the same dairy, while no cases of sore throat connected with a different milk supply were observed. In some families the children who drank boiled milk escaped. Nash visited the premises, but could obtain no information as to illness amongst the cows or milkmen. No examination by him or by a veterinary surgeon of the milkers or of the cows appears to have been made, and the source of the infection of the milk was not ascertained. The bacteriological examinations made of the milk after the outbreak were negative.

(11) LINCOLN [11], May, 1902 (reported by Dr. L. W. Darra Mair).—An explosive outbreak of 168 cases, sufficiently ill to require medical treatment, all but five of which started between May 9 and May 15 inclusive, and all between May 9 and May 19. Of 156 sufferers, the age of whom was recorded, 133 were adults or children over 12, and only twenty-three were under 12. The epidemic came to a sudden termination in spite of the fact that the distribution of the suspected milk was not prohibited. All the cases were characterized by sore throat, well-marked swelling of the tonsils, uvula and mucous membrane of the pharynx. The cervical glands were enlarged. A rash was present in many cases, but it was unlike the rash of scarlet fever. Mair concluded, after very careful investigation, that the disease was not scarlet fever. There were three deaths. No definite secondary cases could be met with at all. The outbreak was clearly due to a particular milk supply. In the period of five weeks ending May 25, of 199 cases of illness associated with sore throat, 168, or 85 per cent., consumed the suspected milk, while if the cases which started after May 8, when the true epidemic began, only are considered, the percentage of milk consumers is 87. Also the number of houses habitually supplied with the suspected milk at the commencement of the epidemic was 141; and of these no fewer than 85, or 60 per cent., were invaded. The incidence of disease was exceptionally heavy

on households using much milk. The source of infection of the milk could not be traced although the most minute inquiries were made. No sore throat or other illness could be traced amongst those handling the cans or the milk. The cows were examined on May 14 by a veterinary surgeon at the request of the Town Council, and he reported them all healthy, while on May 30 Klein examined the cows and found no symptoms of ill-health in any of them. Klein and Gordon examined swabbings from seventeen of the cases of illness. In no instance was the *Streptococcus scarlatinæ* found, but in three cases a pathogenic yeast was isolated, and it was suggested that this was the cause of the outbreak.

(12) LINCOLN [12], May, 1903 (reported by Dr. L. W. Darra Mair).—There were altogether fifty-six known cases of illness, and the onset of all was between April 27 and May 7. All the illnesses started with sore throat, and thirty-two were reported as having a scarlatiniform rash. The fifty-six sufferers comprised eighteen children under 12 and thirty-eight persons over that age. In one house as many as fourteen cases occurred. The diagnosis of the illness in this second outbreak again led to differences of opinion, although on this occasion the preponderance of opinion was much more in favour of scarlatina. At least eight secondary cases were known. The outbreak was in a quite different part of Lincoln to that affected by the 1902 epidemic, and concerned a different milk supply. Twenty-five per cent. of the houses supplied with the incriminated milk were invaded. The implicated milkman had five cows. Klein examined the cows on May 6 and found that two of them exhibited a vesicular eruption on the udders and teats somewhat similar to the "Hendon disease." No illness was detected among the milkers or their households. The bacteriological examination of swabs showed neither *Streptococcus scarlatinæ* nor the pathogenic yeast found in the first outbreak.

(13) GUILDFORD [13], 1903 (reported by Dr. R. W. C. Pierce).—Extensive outbreak, cases starting early in the last week in September and continuing until the middle of November. The symptoms of the outbreak were sore throats of different grades of severity, but for the most part cases of ulcerated sore throat or follicular tonsillitis. In some cases there was marked glandular enlargement. A number of cases of quinsy. Considerable constitutional disturbance was present, shown by headache, pains in the back and limbs, and fever. In several instances there were joint pains after the acute symptoms had subsided. Five cases developed erysipelas after the sore throat. A few cases were fatal. There was a considerably larger number of adults affected than young children. Information was obtained altogether of ninety-eight infected houses, and seventy-six of these were supplied direct by two milkmen obtaining their milk from a common source. The remaining twenty-two households derived their supply from nine other different dealers. With the stoppage of the milk the outbreak ceased. The incriminated farm was visited on November 13 by Pierce and a veterinary inspector. Of the twenty cows on the farm no less than four were affected with mastitis and yielded

purulent milk. The farmer suffered from a quinsy sore throat about the middle of September, and had pains in his joints subsequently, and was disabled for at least a month. The other members of his family also had sore throats. The first cases, as far as is known, occurred the last week in September, the illness of the farmer therefore probably preceded the outbreak. He occasionally assisted the two men at the milking, and in this way he may have infected the cows and the milk. The bacteriological investigations showed the presence of streptococci in the milk, and Gordon isolated a streptococcus of the *pyogenes* type virulent to mice. It is probable that the farmer was the original cause of the outbreak, but as infection continued over six weeks, and the farmer could only have infected the milk on one or two occasions at the most, some continuously acting source of infection must have been present. If we assume that this continuous source of infection was the diseased cows, and that they were infected from the farmer, as Pierce suggests, the course of the outbreak is easily explained.

(14) FINCHLEY [14], 1904 (reported by Professor H. Kenwood).—An extensive outbreak lasting from January 16 to January 30 and involving some 550 persons. The symptoms were sore throat with enlargement of the submaxillary glands, and marked malaise. Considerable temperature in most cases, with much prostration. In one or two cases quinsy was present, while septic conditions, such as gland abscesses and cellulitis, were met with as complications. Kenwood came to the conclusion that the outbreak was spread by milk. The cases occurred almost exclusively amongst the customers of a particular milk supply, but exact particulars are not furnished. He also satisfied himself that "the employés of the infected supply had not been spreading the complaint by reason of any one of them suffering from a bad sore throat at the period when the outbreak occurred." The precautions taken for the safe storage and delivery of the milk were quite satisfactory. In regard to the cows a veterinary examination of every cow was made, and "the veterinary expert reported that all the animals were in a condition of perfect health, with the exception of two cows which had contracted a chill; the chill had affected their udders, causing their milk to become curdled, and that probably these animals had been in this condition for several days prior to his inspection on January 27. Both these cows were at once removed from the herd." Kenwood gives it as his opinion that "the outbreak was in all probability due to the two cows which were suffering from an obscure condition of ill-health towards the middle of January," but he gives no reasons in the report for coming to this decision other than those recorded above, which are scarcely conclusive. The bacteriological examination of numerous samples of milk were negative. French, one of the medical men in practice at Finchley, gives one very interesting item of information. He found that one of the cases, complicated with severe facial erysipelas, was markedly improved both for the erysipelas and the throat symptoms by the use of antistreptococcus serum. He then tried the serum in other cases, and although they were very intractable to the usual remedies, without exception they all improved markedly, the temperature falling to normal within twelve

hours. Pus examined from the suppurating glands of one of his cases showed streptococci on cultivation in pure growth.[1]

(15) BELVIDERE HOSPITAL, GLASGOW [15], 1904 (reported by Dr. A. K. Chalmers).—In May, 1904, at Belvidere Hospital, thirty-nine persons were affected; chiefly nurses, cleaners, and wardmaids. The symptoms were those of tonsillitis and were mostly mild. No deaths. The illness was traced to milk and ceased when the milk was sterilized. The cases extended over a period of about twenty to twenty-five days, half the cases sickening in the week ending May 14. On April 23 a new cow was added to the herd of seventy-two cows. Towards the end of April the group of cows to which the new cow had first been added began to suffer from a teat eruption, and this spread to the other cows. It spread until about 30 per cent. of the herd were attacked, the outbreak reaching its height about May 6. The hands of four out of eight milkers were affected with sores. Chalmers regarded the teat condition as the cause of the outbreak.

(16) PAISLEY [16], October, 1904 (reported by Dr. A. Robb).—Over 100 cases were known, but there were others which did not come under observation. No deaths. The symptoms were those of acute throat inflammation with constitutional symptoms. Diphtheritic-looking membrane on the tonsils, sometimes also on uvula and soft palate. Malaise, high temperature, and marked prostration. The only factor connecting the cases was a common milk supply. The Burgh veterinary inspector visited the farm late in the epidemic, and reported that many of the cows had evidently recently suffered from cow-pox—the teats showing remains of the eruption—but had now recovered. No information given as to whether there was or had been any illness amongst the milkers. Bacteriological examination of throats showed that the outbreak was not due to the diphtheria bacillus.

(17) COLCHESTER [17], April, 1905 (reported by Dr. W. G. Savage).—Outbreak very extensive, probably at least 600 cases, and of these over 170 cases known to be under medical treatment. The symptoms were those of "septic sore throat." The tonsils and other parts of the throat were red and swollen. In some cases diphtheritic-like membrane was present, even extending on to the soft palate. The submaxillary glands were generally enlarged and painful. In no cases were rashes observed. No deaths were recorded. There were few or no secondary cases. Females suffered about three and a half times as much as males, and adults furnished three times as many cases as children. The incubation period was apparently not longer than two days. The evidence implicating a particular milkman's (M.) milk was complete, being derived from information supplied by medical men and from house-to-house inquiries. The

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[1] French suggests, from these facts, that the cause of the disease was undoubtedly the *Streptococcus mastitidis bovis*. My investigations have shown that the ordinary streptococcus of mastitis is *not* the *Streptococcus pyogenes vel erysipelatosus*, and therefore the fact that there was evidence in favour of the presence of this latter organism is another link in the chain of evidence demonstrating that when cows' udder abnormalities are infectious to man we have present an organism, such as *Streptococcus pyogenes*, which is of human origin.

following table shows the relationship of the cases attended by medical men for which particulars were available and the milk supply:—

TABLE III.

Milk supply	West side of town		Rest of the town	
	Households attacked	Percentage	Households attacked	Percentage
M.'s milk...	58	96·6	6	37·5
From other vendors	2	3·4	10	62·5
	60	—	—	—

The outbreak was nearly confined to the west side, an area not including more than 20 to 25 per cent. of the population. The house-to-house inquiries made in four selected areas showed that 50·9 per cent. of the houses supplied by M.'s milk were invaded, as compared with 6 per cent. for the other milk vendors. The milkman M. obtained his milk from six farms, and from a study of the distribution of the milk to the different milk rounds, and the different incidence of the disease on the different rounds, it was possible to point to farm B as the offending source of supply. All the farms were inspected, but only on farm B were human cases of illness or disease amongst the cows met with. Upon this farm a cow was found in a shed with the others suffering from mastitis of one quarter, and which had been ill and noticed since about April 19. There can be no doubt but that the milk of this cow was being added to the milk up to the time of visit, April 27. The cessation of the outbreak corresponded with the exclusion of the milk of this particular cow. Several cases of illness were found on the farm, but the cases were coincident with cases in the town. The first illness on the farm was that of the farmer himself; it began on April 17, was a severe sore throat, and confined him to bed for three or four days. He never milked the cows, and said he had very little to do with them. Five or six other cases occurred amongst the farmer's family on the farm, but all began after April 20, and all drank the milk. The outbreak started about April 17 and ceased about April 29, the maximal incidence being on April 21 and April 22. The bacteriological investigations showed that the sore throats were presumably due to streptococci, while no diphtheria bacilli were present. The fluid from the mastitis quarter of the cow showed abundant streptococci. I had not then differentiated these streptococci, and was therefore unable to prove that the throat streptococci and mastitis streptococci were or were not identical.

(18) LEITH [18], June to October, 1904 (reported by Dr. W. Robertson).—A long-drawn-out mixed outbreak of diphtheria and septic sore throat. The actual diphtheria notifications in connexion with this outbreak were two at the end of June, two in July, five in August, nine in September, and the last on

October 16. The connexion of the cases with the incriminated milk supply is not clearly set out in the report, but apparently all the diphtheria cases had a common milk supply. The relationship of the sore throat cases to the milk supply is not established in the report, but apparently there was a close relationship. An examination at the farms failed to find any cases of sore throat or suspected sore throat. Of the forty-five cows five were found by the veterinary inspector to be suffering from ulcers on their teats. Drying pustules were found on some of the ulcers, and the veterinary surgeon diagnosed the condition as one of cow-pox. A further detailed examination a week later showed fifteen cows suffering from ulcerated teats. A dairymaid was affected with pustules on her hands, and the farmer's son was also affected. Although swabs were taken repeatedly from the ulcers and examined the Klebs-Löffler bacillus was never isolated, although "now and again an organism possessing the microscopical and cultural characteristics of the Klebs-Löffler was detected." They were, however, non-pathogenic to animals.

The above recorded outbreaks are all that the writer has been able to find in the British Isles. Foreign literature has not been searched, but very few such outbreaks appear to have been recorded. The following outbreak at Christiania is of interest :—

(19) CHRISTIANIA [19], 1908 (recorded by Dr. Sömme). — An extensive outbreak which began in almost every case with a sore throat. In addition to sore throat with red tonsils and pharynx, the usual symptoms were swelling of the glands of the neck and high temperature. Complications, chiefly of a pyæmic character, were numerous, but gastro-enteritis and diarrhœa were apparently not common. A number of the cases died. Adults were affected about three times as commonly as children. The number of cases of the epidemic notified was 548, but as notification was voluntary, it is probable that the total number was considerably higher. The means of spread of the outbreak was quickly traced to the milk supplied by one dairy, and thence to a cow with a diseased udder. The cow was slaughtered, and a bacteriological examination of the abscess found in the udder led to the isolation of streptococci in every way identical with the streptococci obtained from patients attacked by the epidemic. The streptococci were long-chain forms, pathogenic to mice after intraperitoneal injection.

(II) OUTBREAKS OF DIARRHŒA OR GASTRO-ENTERITIS SPREAD BY MILK.

Records of such are even rarer in the literature than those of sore throat outbreaks. The following one has been recorded in this country and several abroad :—

(1) ABERDEEN [20], November, 1908 (recorded by Dr. Matthew Hay).—An extensive but circumscribed outbreak. The illness usually began with severe headache, shivering and backache. More or less severe vomiting or belching, followed with abdominal pain in severe cases. In most cases diarrhœa was present and continued for several days. Usually some fever, and in a few cases

cramps in the legs were complained of. All the cases recovered. Inquiry soon showed that it was due to a particular milk supply. For example, Hay found within a short street in which the dairy was situated nearly seventy cases, all of which, except two, were supplied from the incriminated dairy. In the other houses with a different supply, which constituted more than half the total number, there were only two cases of ill-defined nature. The milk was obtained from three farms. On one of these there had been a number of cases of illness. The first persons sickened at the farm on November 5, while, with one exception, all the illnesses of the group investigated in the city commenced on November 6 to November 8. Hay attributes the outbreak to the occurrence of cases of disease at the farm, which infected the milk. No mention is made of the cows, or indeed whether they were examined at all.

Jensen ("Milk Hygiene") mentions the two following outbreaks:—

(2) STOCKHOLM.—In Stockholm the members of nine families became ill with nausea, vomiting and diarrhœa, fever and weakness, together with cramps in the legs. The disease was traced to the use of milk from a certain herd. The herd comprised fourteen cows, one of which was proved to be affected with mastitis. Two persons working in the stable were sick with the same symptoms.

(3) Lameris and Van Harrevelt mention an outbreak of diarrhœa among a number of persons in a hospital. The disease was traced with comparative certainty to the milk of a herd in which several cases of mastitis due to streptococci were found. The infection probably came from the use of milk from a cow that appeared to have recovered, but whose milk still contained streptococci. The milk was used only after being boiled, but the boiling was probably imperfectly done.

REFERENCES TO APPENDIX.

[1] *Brit. Med. Journ.*, 1881, i, p. 657.
[2] *Ibid.*, 1881, ii, p. 415.
[3] *Practitioner*, 1884, xxxii, p. 467.
[4] *Glasgow Med. Journ.*, 1894, xxxiv, p. 241.
[5] Annual Report of Medical Officer of Health, Finchley, 1894.
[6] *Brit. Med. Journ.*, 1897, ii, p. 339.
[7] Annual Report of Medical Officer of Health, Surbiton, 1897.
[8] *Ibid.*, Hackney, 1900, pp. 60-69.
[9] *Journ. of Hyg.*, Camb., 1902, ii, p. 150.
[10] *Lancet*, 1902, ii, p. 1050.
[11] Annual Report, Local Government Board (Report of Medical Officer), 1902-03, xxxii, p. 190.
[12] *Ibid.*, p. 213.
[13] *Brit. Med. Journ.*, 1903, ii, p. 1492; *Journ. of State Medicine*, 1904. p. 595.
[14] *Brit. Med. Journ.*, 1904, i, pp. 602, 831.
[15] *Public Health*, 1903-04, xvi, p. 769.
[16] *Ibid.*, 1904-05, xvii, p. 773.
[17] *Ibid.*, 1905-06, xviii, p. 1.
[18] *Ibid.*, 1904-05, xvii, p. 246.
[19] *Lancet*, 1908, i, p. 1707.
[20] *Public Health*, 1910, xxiii, p. 181.

DISCUSSION.

The PRESIDENT (Dr. Theodore Thomson), in thanking Dr. Savage for his paper, said that his work on the subject in the past rendered him a recognized authority. The main hypothesis advanced was a very tempting one, introducing their increasingly familiar friend the carrier—this time the domestic cow.

Dr. BUCHANAN said he very much appreciated the clear way in which Dr. Savage had set forth the considerations in the paper, while the speculations advanced were very interesting. He had not read the paper by Dr. Stott, which Dr. Savage mentioned in referring to the absence of evidence that milk from cows which had cow-pox produced human illness. But his impression with regard to the outbreak of cow-pox referred to was that the inquiry which was made, and which led to the idea of the absence of human illness, was not of an extensive kind. The outbreak among the cows was extensive, and the dairy was a large one, supplying hundreds of families; and in order to prove that the milk from those cows had caused no harmful effects it would have been necessary to make a house-to-house inquiry over a considerable period, and in a wide area: inquiry, for example, as to the occurrence of what was believed to be chicken-pox, or of boils, or skin affections, or to ascertain whether children who had been having that milk and had been subsequently vaccinated had proved insusceptible. If such complete inquiries had not been made he did not think great importance should be attached to the reported absence of illness following the taking of the milk in question. With regard to the fact that milk was allowed to be used after pasteurization, the dairy company concerned were perhaps rather fortunate, as objection might be taken to milk which was likely to contain inflammatory products. The same question arose with regard to some of the cases of cow mastitis referred to in Dr. Savage's paper. Dr. Savage said: "From the practical point of view, it is well to remember that the pathogenic and non-pathogenic types of bovine mastitis are not clinically distinguishable." He (Dr. Buchanan) thought that when dealing with milk from herds which were affected by mastitis it would be undesirable, in the present state of our knowledge, to take too many risks. He would not give too much weight to the result of the heroic experiment which Dr. Savage made upon himself. If Dr. Savage had given himself an acute abscess or septicæmia it would, no doubt, be different; but as the result was happily negative, it should be remembered that it was that of a single experiment, on an adult, who was healthy, and presumably had no abrasions in his throat. He did not altogether follow the hard-and-fast distinction which the paper suggested between the purely local and the constitutional disease in the cow. It was possible that the same illness, caused by the same organism, might sometimes be associated with constitutional symptoms, as well as by purely local symptoms. He believed that quinsy in the human was not unassociated with milk from time to time, and the disease had a sort of incubation period. It was very definite in its course; but one could look upon it as either a local or a constitutional affection, according as one inquired whether the main seat of the inflammation was localized, or whether the patient was constitutionally ill. It seemed difficult to draw the line of demarcation; and he thought the same sort of consideration might be applied to cow disease, not only in the case of mastitis, but also in that of cow-scarlatina. With regard to the Hendon outbreak, as the author rightly pointed out, one must assume that there was continuing infectiousness of the milk in circumstances under which, there, the infection by the milker, if it occurred at all, was certainly not continuous. Sir William Power proved that there was no evidence of infection by the milker in that case: and though at the time the new idea that the cow could have anything to do with scarlatina was strongly combated, nothing appeared to have been brought forward to disprove it, and there had been subsequent confirmation in other outbreaks. If that was so, and one had to assume in these outbreaks that the cow was continuously infectious for some period, it seemed difficult to suppose that the source of infection was solely a mere accidental human infection at some moment, which persisted locally. It was equally possible to believe that there was a more general infection (from human or bovine sources), in which the bacteria of the scarlatina were excreted by the cow, or in which they remained in the udder and multiplied in the milk sinuses. This matter was, however, rather speculative, and all those who had to do with scarlatina outbreaks would feel much indebted to the author for the valuable suggestions he had made.

Dr. BUTLER remarked that Dr. Savage had chosen the psychological moment for contributing a paper on the relationship of human to bovine disease, as members were now turning their thoughts to the observations of Jenner, for the non-observance of which serious results were being threatened. It was encouraging to learn that the cow was not to be debited with a serious rôle in the distribution of disease. One was indebted to the cow for contributing a degree of immunity from another serious disease which it would be difficult for it to wipe out, however virulent it became as a factor in the distribution of other diseases. The author referred to the fact that there was little epidemiological evidence as to the effect of the cow infection upon the spread of disease among human beings. The observations of Drs. Dean and Todd in an outbreak of diphtheria aptly illustrated that fact. He was much struck in that contribution by certain dates and initials, and on turning up his records in the isolation hospital with which he was connected, he found that they were cases some of which had been under his charge, but he had not known they were the subjects of an investigation. He had had to perform tracheotomy on one of the patients. The remarkable feature of that outbreak was that there was a perfect chain of evidence that in a particular couple of families the spread of diphtheria could be accounted for entirely by the affection of the udder and the teats of the cows, and infection of the milk, and diphtheria bacilli had been isolated from them. The cows were in the possession of a gentleman who kept them for his own family and servants, and the milk was not distributed in the district, where there was at that time a very extensive outbreak of diphtheria. If those facts were borne in mind, it was necessary to consider whether the cows had not been themselves infected from the human sources rather than conversely. With that outbreak going on for weeks or months before those families were affected, one would have to account for the relationship between the affection of the udders of cows and the many persons who did not partake of the milk and yet suffered from diphtheria. A more reasonable hypothesis would have been that the disease was contracted by children attending the public elementary schools in the district known to be affected, and had then spread by contact infection, while the udders of the cows already infected with other lesions had the superadded infection as a result of the people themselves having contracted it. But that did not entitle one to arrive at the other conclusion—namely, that as a result of the infection of the udders of the cows the further spread was accounted for. It had not been established that milk had played any considerable part in the spread of diphtheria, and in Dean and Todd's paper there was only evidence of resultant distribution in the milk of the diphtheria bacillus from an affection of the udders of the cows, coincident with the distribution of diphtheria associated with a limited number of persons who partook of the milk of those cows, but not necessarily resulting from it. Probably the cart had been put before the horse in that case, in supposing that the distribution in those families was due to the consumption of the milk. The other view suggested itself—namely, that the people themselves were infected by contact, and that the milk became infected possibly through the udders of the cows as the result of infection conveyed from them. At all events, it seemed to support Dr. Savage's hypothesis that, given a lesion of the udder, there might be added another infection; but that did not entitle one to conclude that such superaddition of the infection was the cause of the further spread of the disease.

Dr. PARSONS said the paper had the merit that it advanced a hypothesis which he did not think had been previously put forward to account for the cases in which the milk had appeared to have an infective property derived from the cow itself. Dr. Savage would probably not claim that that hypothesis was definitely proved, but it suggested experiments to follow up the clue. If it were proved to be true, it afforded a means of conciliation between those who approached the question from the human point of view and those who viewed it from the veterinary side. The latter were positive that the disease of the cow was a purely local matter, and had no association with scarlet fever, whereas the evidence seemed very strong, from the epidemiological side, that the disease of the cow did convey human scarlet fever. One point of interest was the great suddenness with which the outbreaks in question occurred, which was against the idea of cows being chronic carriers, and often the cow implicated was one which had only recently calved. In an outbreak which he investigated, one near Macclesfield in 1889, the cases in the different households were connected by a common milk supply and a nearly simultaneous occurrence, and the proportion of cases in the households bore an exact correspondence with the amount of the milk consumed by them. There were, however, three types of disease connected with that milk. There were cases of scarlet fever, other cases of what seemed to be septic sore throat, and others of what was clinically diphtheria. The milk in a scarlatina outbreak had but a small degree of infectivity. There might be hundreds of primary cases, but very few in which there was a second outbreak in a house from the first one, and that had to be explained by the hypothesis, in order that it might hold good.

Dr. BOND agreed with Dr. Buchanan that it was not necessary to look upon infection of the cow as only a local infection ; he thought these might be also constitutional infections. At a Brighton Congress on a similar subject he said that, as small-pox produced in the cow a modified constitutional disease, vaccinia, so scarlet fever in the human subject might produce in the cow a modified form of that disease. It was the general opinion that outbreaks due to milk generally produced a modified scarlet fever and a smaller percentage of secondary cases. The fact that the apparently healthy quarters were also infected with the identical streptococci in ordinary mastitis also showed that it might be a constitutional affection in the cow.

Professor WOOLDRIDGE said he approached the question from the veterinary standpoint. The hypothesis advanced by Dr. Savage was a very plausible and attractive one at first, and was worthy of further investigation. There was not much danger of anthrax being conveyed by the milk of affected cows, because of the rapidity of the symptoms and the early death. When the anthrax bacilli were plentiful the animal was near death, and milk was very unlikely to be taken from it for human consumption. More likely the case referred to by the author which died was affected by inoculation, and not from drinking the milk. He knew of no case of cattle affected with Gaertner bacillus during life to which food poisoning could be traced—i.e., by human beings eating such meat. Where meat had been found infected with the Gaertner bacillus, the meat had been exposed since death ; or else the infection occurred where the animal had been moribund. With regard to the septic conditions, such as puerperal septicæmia and septic metritis, Dr. Savage said the milk was usually markedly altered. That he regarded as a misstatement. He had examined very large numbers of cows affected with those conditions, but he did not know of one case in which the milk was markedly altered, though he did not suggest such milk should be consumed, as possibly it might contain harmful toxins, and should therefore be avoided. In Group II bovine scarlet fever was mentioned. That had long been a bone of contention between the medical and veterinary professions. The latter recognized no disease in cattle which could be compared with scarlet fever in man, or which deserved that name. With regard to the summaries of the reports, the figures referring to acute mastitis conformed with his own experience of the prevalence of acute mastitis in cows. He had been pleased to see the differences pointed out between the two forms of streptococci met with in those infections, the *Streptococcus mastitidis*, and that found in sore throat in the human subject, particularly with regard to their pathogenicity. Only pathogenically could they be distinguished. In making cultures one could not say whether there was a mixed culture, taking it for granted that the assumption in the paper was correct—viz., that the organism in the human sore throat might be implanted on an udder which was already infected with mastitis. If so, why was not the pathogenicity of the human streptococcus evidenced when the culture was injected into the mouse? The fact that it was not constituted a strong argument against the suggestion that the udder of the cow was likely to be the seat of a dual infection. Dr. Savage's experience with regard to implanting cultures in his own throat was supported by an experience which he himself underwent involuntarily. An apparently healthy cow, in full milk, was intended for experimental purposes, and the milk was consumed for several days by six persons—two adults (a male and a female) and four children (three male and one female). After several days two more persons, he and a colleague, also partook of that milk as it came fresh from the cow. He thought it rather thin, but did not pay much attention to that. Next day he had some more and thought it seemed rather salty, which made him give more attention to it. On putting a little into the palm of his hand he noticed small points about the size of a pin's head. Those he transferred to a cover-slide and stained, and demonstrated myriads of streptococci. Similar smears from the centrifugalized milk gave the same result. Yet no person of the whole eight who partook of it experienced the slightest inconvenience in the throat or any other part of the intestinal canal. Therefore he agreed that, taken as a whole, mammitis in the cow was not dangerous to the human subject. Still he did not advise that milk from such cases should be taken. The appendix to the paper was supposed to suggest that the cow was responsible for the spread of those infections. He thought there was no doubt that milk was the means of conveying the infection round the particular towns. The point was whether the milk was subsequently infected after leaving the cow, and he believed it was. Of the eighteen epidemics referred to, in nine there was no evidence to connect the cows with the outbreak. In some instances there was no mention if the cows were healthy, but in others it was definitely stated that no disease was discoverable. In three of the cases there was evidence against attendants at the farms being affected with sore throat. In nine instances some diseased condition was attributed to the cow. In one case (Case 3) the evidence against the cows was that a month before they had suffered from foot-and-mouth disease. But that was not sufficient to condemn the cow. Four epidemics had associated with them ulcerated teats, but there were no organisms discovered in connexion with the ulcers or with the milk which corresponded with the organisms causing the sore throat. In the Colchester case the farmer was attacked with sore throat on the 17th of the month, and the first appearance of the affection of the cow was on the 19th of the month. There, he thought, a human origin must be credited for the spread. In conclusion, he thanked Dr. Savage for his paper, which he hoped would emphasize the necessity for a more complete and more thorough system of dairy inspection.

Dr. SAVAGE, in reply, said, in answer to Dr. Buchanan concerning cow-pox, that while Dr. Stott mentioned that there were no cases, he did not think particulars of the exact inquiry made were given. He agreed that, administratively, all cases of mastitis should be looked upon as potentially infective, because clinically it was impossible to distinguish between the dangerous and the harmless varieties. While he was of opinion that the great majority of cases were harmless to man, he believed that a small minority were harmful, and until they could be clinically differentiated all the cases must come under the ban of suspicion. With regard to the experiments on himself, he thought the massive dose used might fairly compensate for any possible lack of sensibility. He admitted that perhaps he had drawn a rather hard-and-fast line between the local and general condition, but he believed his classification was a fairly practical one. He could not agree that the hypothesis of an occasional rare constitutional affection in the cow as a cause of scarlet fever was comparable to his hypothesis, or could equally explain the facts. It must be admitted that all efforts by veterinary surgeons to produce bovine scarlet fever by inoculation had failed. In answer to Dr. Parsons, he would point out that of course he only suggested that quite a small proportion of scarlet fever outbreaks were spread by any infection conveyed by or through the cow. He could not agree with Professor Wooldridge that Gaertner bacilli had never been found in the milk during life, as he had come across a record of one such occurrence. With regard to septic conditions, he was willing to accept his correction ; he had no personal knowledge of the fact as to alteration in the appearance of milk in this class of cases. On one important point Professor Wooldridge had evidently misunderstood him. That speaker evidently thought that in the paper he (Dr. Savage) suggested that the cases of cow mastitis which were infectious to man were cases in which a human streptococcus was superadded to the mastitis caused by the ordinary bovine streptococcus, a mixed growth resulting. If he would re-read this part of his paper he would find that this was not his view at all, and he was glad to make that clear. Mastitis in cows was usually caused by the *Streptococcus mastitidis*, a streptococcus, in his opinion, of purely bovine origin, and as such harmless to man. In a small proportion of cases the cow mastitis was caused not by this streptococcus at all, but by other streptococci of quite different type, and part of his hypothesis was that some of these were possibly of human origin and in that case probably pathogenic to man. There was no question of a double infection. Professor Wooldridge suggested that the cases brought forward in the appendix were mentioned in order to support the thesis of the paper. That was not necessarily so ; they were brought forward impartially, and he thought it would be admitted that he had stated them without prejudice. It was clear that in the majority of the outbreaks the infection was spread by milk. It was equally clear that in a number of them the suggested view that the outbreak was connected with diseased cows was not proved, sometimes not even warranted, as a hypothesis, by the facts disclosed. For others, however, there was evidence of a valuable character connecting the cows with the outbreak. He would refer in particular to the outbreak described by him at Colchester, in which at the time he most carefully inquired into the cause of the outbreak and came to the conclusion, taking all the facts into consideration, that the cow suffering from mastitis was undoubtedly responsible for the outbreak. He was very glad to receive Professor Wooldridge's views and criticisms from the veterinary aspect of the subject.

Epidemiological Section.

March 24, 1911.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

Typhoid Carriers and Contact Infection. Some Difficulties suggested by Study of Recent Investigations carried out on "Living Lines."

By W. H. HAMER, M.D.

THE task of the inquirer who seeks to trace the origin of, say, one hundred cases of typhoid fever, may not inaptly be compared to the problem confronting a man who has to piece together evidence based upon what people tell him concerning the colour of a hundred balls hidden in a bag. It is known that balls of certain colours, blue (corresponding to infected water), yellow (infected milk), brown (infected shellfish), and so on, may, any or all of them, be there. Some authorities have been led to infer that red balls (corresponding to infection by healthy bacillus carriers) may also be present; others question this possibility. The outcome of the inquiries in any particular outbreak is that the investigator satisfies himself that, in all probability, so many of the hundred balls in the bag are blue, so many brown, &c., &c. There is, of course, no absolute certainty in the matter, and, indeed, differing degrees of favour are accorded at one time and another to the various hypothetical causes, there being an undoubted tendency to lean, say, to blue or to yellow at one period, and then perhaps to favour brown or red at another. This will be clear to those who reflect that the President of the Society of Engineers, forty years ago, traced all the cases of typhoid fever in London coming under his notice to defective drains, or, again, to those who compare the vogue of the water hypothesis twenty years ago with that of the carrier hypothesis at the present time.

It is obvious, when a new ætiological factor is under discussion, or has recently assumed prominence, that in many accounts relative to outbreaks of disease there is likely to be a sort of unconscious bias causing undue emphasis to be laid upon the new factor and leading to relative neglect of older hypotheses.

But in recent years there has been quite a fresh development. The evidence collected by the epidemiologist is now frequently supplemented, or even, some would perhaps say, superseded by a fresh kind of evidence. A new expert (the bacteriologist) has arisen; some have unquestioning faith in his powers, others adopt, at least on certain points, an attitude of reserve. The water suspected by the epidemiologist may not really be infected water; the supposed blue balls, with respect to which the investigator has been pursuing his inquiries, may not be blue at all. If not, there is no use in speculating as to whether any of them are in the bag, for it contains no spurious blue balls. One of the balls is submitted to the new expert, and he pronounces it to be a blue ball. In the case of water and milk, it is true, bacteriological inquiry has not accomplished much in this particular connexion, but in dealing with bacillus carriers (the red balls) the opinion of the bacteriologist is oftentimes regarded as an absolutely final pronouncement, and a kind of mental paralysis overtakes the epidemiologist. It is important, therefore, to endeavour accurately to assess the value of this opinion. It is held by some epidemiologists that the bacteriologist labours under the initial disadvantage of being unable to affirm, when he finds the *Bacillus typhosus*, that the organism he has isolated is capable of producing typhoid fever. In other words, it is not possible to make the organism conform to all the postulates laid down by Koch. As Klinger observes, the bacteriologist is "unfortunately compelled to fall back upon epidemiological observations." Again, we find Conradi, when speaking of the healthy carrier, making the somewhat significant remark: "It is difficult to prove that he is infectious, as we discover him only because of the fact that he lives in infected surroundings. *Post hoc ergo propter hoc.*" It might, in fact, be contended that the new expert is colour-blind, and question arises as to the degree of his defect in this sense. To what extent can his pronouncements in this particular connexion be trusted? If he be "dangerously colour-blind for red" it is no use asking him to express an opinion upon the colour of balls supposed to resemble the hypothetical red balls in the typhoid bag.

It is, in fact, necessary, in summing up on the evidence as to the causation of an outbreak by a bacillus carrier, that it should be decided, how far, on the one hand, is importance to be attached to the facts observed by the ætiologist or statistician, and how far, on the other hand, is importance to be attached to the bacteriological finding? If there is any single instance in which a causal connexion has been established between a carrier and cases of typhoid fever, this mode of origin has to be reckoned with. Even although no such clear evidence be forthcoming, should it, nevertheless, be shown that persons, suspected on purely statistical evidence, are more often actually found to be carriers, than would be expected to be the case as the result of mere chance, then again presumption is raised that the mode of transmission in question is one which must be taken into account.

It is necessary, therefore, first to inquire whether, without assuming that because a man is a carrier he is *ipso facto* infective, we can show that he is so; and, second, to inquire whether the percentage of the *general population*, which can be proved to be bacillus carriers, is notably smaller than the percentage of *suspected persons* who have been actually demonstrated to be bacillus carriers—equally diligent inquiry being, of course, assumed for both the suspected and the control populations.

With regard to the first question, independent epidemiological or statistical evidence is needed. There has been an undoubted tendency in the last few years to accept the demonstration of the fact that a suspected individual is a bacillus carrier as conclusive proof that the said individual has infected others. Thus Rommeler's series is a case in point. It is referred to on p. 271 of Dr. Ledingham's Report. (For convenience the references to cases mentioned in Dr. Ledingham's recent Report,[1] the great value of which is enhanced by its containing a comprehensive bibliography, are given throughout the present paper). A foster-mother, her six-year-old daughter, her three-year-old niece, her own suckling child, and her fifteen-year-old daughter, are attacked by typhoid fever. "There appeared to be little doubt that the series of cases arose from the suckling child, temporarily adopted by the family, *but the suspicion was raised to certainty* by the result of the bacteriological examination carried out." Again, Kayser (loc. cit.,

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[1] *See* Supplement to the Thirty-ninth Annual Report of the Local Government Board.

p. 289) found that "on 23rd, 29th, and 30th March, 1905, five people living in different streets in Strassburg fell ill with typhoid fever. All drank raw milk procured from a milk-seller, H. At one of the farms supplying H. there was a boy who had an indefinite illness about six months before, and whose stools contained large numbers of typhoid bacilli." Here, again, apart from the bacteriological evidence, no conclusion is possible, but the positive result is held to clinch the proof. Or take Mayer's Case 3 (p. 276). On a large estate eighteen cases of typhoid occur in the course of eleven years. There is some suspicion as to "a rather unsatisfactory pump-well . . being polluted from a water-closet employed by the manager of the farm, and, in fact, fourteen days before the outbreak (four cases in 1906) the manager had been known to use this closet. His fæces were now examined, and he was discovered to be a carrier." Thereupon follows the conclusion—"It was practically certain that for years he had been spreading typhoid in his vicinity."

Then, too, there is Dr. Meredith Richards's case of the milker (p. 295), who was not known to have suffered from typhoid fever, but whose wife had been attacked in 1894. We are all familiar with the facts, the somewhat unpromising start in 1903, when out of forty-seven possible consumers of the suspected milk, only one, a butler, developed typhoid fever. Dr. Meredith Richards explains the escape of those who did not suffer by saying that the amount of milk consumed per head was small. The butler may, of course, though it has not been suggested that such was the case, have been upon an exclusive milk diet; he may even have been consuming as much milk as all the rest put together; but, unfortunately, concerning his habits as to milk-drinking no particulars are now ascertainable. Again, there is the fact that only six persons in all were attacked, during five years (including the butler and a female who was "believed to have had cream in coffee and probably with fruit or tart" at lunch on one occasion, the particulars being recalled after an interval of nearly two years); and this although, as already stated, forty-seven persons "consumed the milk at various times." Moreover, there is the "absence of infections during the milker's sojourn in a large dairy during 1906-08." All these shortcomings are, however, overlooked upon the finding of the typhoid bacillus. Any criticism directed against the completeness of the case, from a purely statistical point of view, is now silenced by referring the objector to the fact that the bacteriological examination gave a positive result.

No satisfaction, however, can be obtained by the statistician from study of the circumstances of outbreaks in which only a few persons are supposed to have been infected by a typhoid carrier. The figures must be large enough to admit of application of ordinary control methods of examination; but if the literature of typhoid carriers be carefully studied with this question of control methods in mind, the conclusion must inevitably be reached that there is no real statistical or epidemiological evidence to show that there ever are, or have been, red balls in the bag. Take, for example, the notorious case of "Typhoid Mary"; putting aside the bacteriological evidence, no jury of statisticians could convict. The absence of particulars as to the sizes of the several populations at risk and as to the extent of contemporaneous prevalence, if any, of typhoid in the localities affected; the repeated mention of places of summer residence (with presumable special risk of shellfish infection); the low case mortality (one fatal case in twenty-six); these are points for criticism; but the main thing is the uncertainty as to the value to be attached to the particulars which are available. Dr. Ledingham writes concerning the case: "Much difficulty was experienced in tracing out the past history of this cook, and possibly the achievements mentioned here have no claim to be exhaustive." There is another possibility, that the account of achievements instead of failing by defect may fail by excess. As the patient herself refused all information, and was only put under detention "after a severe struggle in which police aid had to be requisitioned," one is not surprised to learn that "considerable gaps in her history could not be filled up in spite of careful inquiry." But the statistical evidence in this case, and in cases such as those of Kayser, Mayer, Hutchinson, Baumann and Johnstone (Jennet Hill), is not presumably presented as being more than suggestive. What the investigators especially emphasize is their discovery that certain persons were found to be typhoid carriers.

In fact, if it be desired to come really to close quarters with the statistical evidence in favour of the carrier hypothesis, the issue becomes narrowed down to consideration of a few milk outbreaks. Limitations of space necessitate restriction of reference to four of the chief of these.

(1) There is an Irish case (Kilworth Camp). All the sufferers *except one* belonged to a particular mess. The milk consumed at this mess came from a farm where two cases of "pneumonia" had occurred. Three members of the civil population who drank the same milk were also attacked. There is no information as to how many civilian

consumers of the milk escaped, nor as to how many civilians who did not drink the milk were attacked. "There had been a good deal of enteric fever around the district for some years." As to the incrimination of a particular suspected dairymaid, two residents at a farm, at which she worked in 1904, developed typhoid fever; the baby at another of her places of work was "delicate": and the farmer who employed her, in 1906, attributed some cases of typhoid fever to her, and "got rid of her." This farmer appears, therefore, to have independently discovered for himself the bacillus carrier hypothesis. All this is interesting, when considered in the light of the subsequent discovery that the girl actually was a bacillus carrier; but, from an a priori point of view, it is far from being convincing.

(2) Dr. Watt, Medical Officer of Health, Aberdeenshire, has described several carrier outbreaks; to one of them great importance is at the present time being attached. "Miss S.," from 1887 to 1908, was found to have been associated with a long series of typhoid cases at certain farms, and also in Aberdeen, "where milk-borne outbreaks traceable to these farms periodically occurred." I am greatly indebted to Dr. Watt and to Dr. Matthew Hay for access to original reports dealing with these outbreaks. It is clear from the study of these reports that, speaking generally, the suspected milk was not supplied to all the sufferers in the prevalences, and, as this is a point of vital importance, the circumstances relating to the several milk supplies are deserving of being examined at some length.

In September, 1898, typhoid fever in Aberdeen "showed a very large increase, thirty cases with two deaths having been reported, as against six cases with one death in the preceding month." . . . "The excess of cases was traced . . . to an infected milk supply from two associated dairies." . . . An arrangement was made "with the dairy-keepers, on payment of £50, that they should permanently cease to carry on the business of dairymen in their present premises." Then follows the important statement: "It is of interest to note that, even after deducting the cases of typhoid, traceable to the infected milk supply just referred to, there remain thirteen cases for the month, a number which is about twice the average weekly number in recent years. There would appear, therefore," writes Dr. Hay, "*to have been some general influence at work, atmospheric or climatic possibly, which favoured the development of typhoid in the city.*" In October, 1898, "typhoid fever showed a decided decrease . . . but the number is still unusually high." Two cases were "infected outside the city from residence at a farm in Aberdeenshire, which has been repeatedly associated with outbreaks of typhoid." This is, therefore, the second farm coming under suspicion; it, however, only supplied two cases.

In November, 1898, thirty cases were reported or discovered. "It will be remembered that a large increase, in September, was traced to an infected milk supply from two adjacent dairies . . . which were subsequently closed. The sharp increase during the past month has also been traced to a milk supply in the R. district . . . in all, twelve cases of typhoid have been found to be comprised in this outbreak." So that here, too, the dairy (the third dairy suspected) fails to account for anything like the total excessive number of cases. "Just as the foregoing epidemic was closing another of a similar nature, but on a more extensive scale, began to emerge."

This, the fourth milk supply to fall under suspicion in Aberdeen, in 1908, is one of the supplies with which a typhoid carrier was later associated, and it is a supply which therefore especially concerns us. It is the supply referred to, on p. 294 of Dr. Ledingham's Report, as "farm G." Dr. Hay writes: "The almost simultaneous occurrence of three cases of apparent typhoid among the customers of the dairy, which happened in the medical practice of the Convener of the Public Health Committee, and were immediately brought under my notice by him, led to inquiries being made, through the County Medical Officer, as to the presence of illness at the dairy. He visited the farm with the utmost promptitude, found three cases of suspicious illness, one of which had existed for over a fortnight, and immediately arranged for the stoppage of the milk supply. Further investigation of these cases, and the application of the Widal test, has shown them to be cases of typhoid fever, and I understand they were subsequently so reported by the medical attendant, who, it is right to mention, had been summoned for the first time only a day or two before the visit of the County Medical Officer."

Dr. Hay refers to the distribution of the milk from this farm, in "several streets in the east end of the town," . . . and he says, "so many cases of typhoid have already arisen among the consumers that it would be possible to map out the daily route of the cart, from the locality and distribution of the cases." . . . "Up to the present time twenty-five cases from this source have been reported or discovered within the city." Then he goes on to say, "At the moment of writing a third (or counting the farm supplying the two October cases, a fourth) milk epidemic of typhoid appears to be commencing. This was traced to another farm, from which a case of illness, which was found to be typhoid, was removed. The supply to the city from this farm was stopped. . . . Such a series of milk epidemics of typhoid . . . points to an unusual prevalence of typhoid at present among the families of dairy keepers and farmers."

In December, 1898, Dr. Hay writes: "Typhoid fever has largely increased during the month, sixty-one cases with three deaths having been reported. . . . During the past four months there have been 141 cases of this fever. The vast majority of the cases, occurring during the past month, have arisen from the two last of the infected milk supplies to which I made reference in my preceding report." . . . "Several cases have recently arisen, not from consumption of the infected milks, but from contact with the cases infected by the milks." . . . In January, 1899, Dr. Hay was able to report that typhoid was rapidly declining.

The careful contemporary reports of Dr. Matthew Hay have been quoted at length, inasmuch as they strikingly illustrate the statistical difficulties of the problem as to the origin of the successive prevalences; difficulties which cannot be appreciated from study of a brief summary of these reports. One or more of the five outbreaks occurring between November, 1898, and January, 1899, may have been due to milk; but, as Dr. Hay had already noted in September, 1898, there was "some general influence at work." What that general influence may have been it would, probably, be impossible now to say, but clearly there is a considerable element of doubt as to the causal relation between the milk from farm G. and the Aberdeen cases. The demonstration of the association of a bacillus carrier with this farm has been appealed to in support of the case against the milk; but the evidence against the milk cannot be relied upon as materially strengthening the case for the infectivity of the bacillus carrier.

There is still another milk outbreak in Aberdeen, which has been mentioned in connexion with Miss S. (the carrier associated with farm G.). The farm in question is referred to (on p. 294) as farm M.; the evidence is far from being conclusive: it amounts to this, that fifteen cases of typhoid fever occurred in 1901 among the customers supplied from an Aberdeen dairy, the milk being obtained from farm M.; as against fourteen cases (among the customers) for nine other years, when the dairy was not so supplied. The fact is, however, that from 1902-08 typhoid fever was at quite a low ebb in Aberdeen; and while it is true that the dairy had eight cases in 1901, when it was supplied from farm M., as against six during the period 1902-05 (an annual average of 1·5), when it was not so supplied, it transpires that the relative incidences upon the customers of this dairy, during these two contrasted periods, were much the same as those on the Aberdeen population as a whole; for in the latter there were fifteen deaths in 1901 and sixteen (an annual average of four) in 1902-05.

The Aberdeen carrier, Miss S., was discovered nearly ten years subsequently to the supposed (farm G. and farm M.) milk outbreaks. She was then living at a farm T. Three cases of typhoid fever occurred at this farm. No milk, Dr. Watt informs me, was distributed from the farm, and no typhoid fever had ever occurred there before. He adds that it was admitted that, later, other cases besides the three mentioned occurred at this farm. Dr. Ledingham (p. 295) says other outbreaks "appear to have been associated with farm G., but the actual facts could not be ascertained. In any case it was suggested that this farm had been visited in September, 1878, 1882, and also in 1885, by the City Medical Officer of Health, in connexion with milk-spread outbreaks in Aberdeen."

Dr. Ledingham, in summing up for the thirty-one years, adds twenty-five cases of typhoid fever occurring at three[1] farms, to twenty-six cases and fifteen cases, occurring in connexion with two dairies deriving milk, at various times, from one or another of these farms (the total population consuming these supplies is not stated; it must, in some of the instances in question, have been considerable, so that the case-rates were not even, necessarily, in those instances excessive), and then he concludes by saying, "Probably, were all the facts known, it would be found that the number of cases, directly or indirectly attributable to infection from (this carrier), during the period 1877-1908, would not fall short of 100." "This woman," he adds, "is eclipsed only by the Folkestone carrier." A posteriori, the possible association of 100 cases with a carrier is important; but, to the cold and calculating a priori observer the details are unsatisfying. Indeed it may be remarked, generally, that typhoid-carrier literature strikingly illustrates this difference in point of view. Given absolute reliance upon the positive bacteriological result, all is regarded as grist that comes to the mill. In the absence of this confidence, the investigator, who finds case added to case with, as he thinks, insufficient exercise of critical reserve, is forcibly reminded of the well-known account of a celebrated contest in which eleven buckram men grew out of two; the case so far as the two men were concerned being, moreover, one of mistaken identity.

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[1] Four farms are referred to, but at one no cases occurred, although the carrier worked there.

(3) Dr. Matthew Hay has very kindly drawn my attention to a remarkable outbreak reported upon by him in 1907. The burgh of Peterhead, with a population of about 13,000 (increased during the herring fishing season—May to August—to 16,000-17,000), had, during the ten years 1897-1906, an average of about eleven or twelve cases of typhoid fever per annum, and then in the months June to August, 1907, 357 cases were notified. Dr. Hay discusses in detail the question of water supply, but is "distinctly of opinion that the case against the water cannot be proved." The facts appear to him "to adapt themselves far more readily to the conclusion that the epidemic originated in milk infection, and that it was subsequently continued wholly or mainly by contact infection." If the case for milk infection could be regarded as proved, the outbreak would assume interest in connexion with the typhoid carrier hypothesis, for a servant at a suspected farm had nursed her mother through an illness, some weeks before the Peterhead outbreak, and the mother was found later to be excreting typhoid bacilli in her urine. The servant herself, however, *remained well, and her blood and urine were examined with negative results.*

The following are the main difficulties as regards acceptance of the view that the outbreak was milk-borne. First, the suspected milk was only supplied to six out of the seven persons sickening in the first week; to seventeen (or possibly twenty-one) out of twenty-four of those sickening in the second week; and to seventeen (or possibly twenty) out of twenty-three of those sickening in the third week. It was suggested, in partial explanation of this difficulty, that a second milk supply became implicated, about a week after involvement of the first; and that, if the two supplies be taken together, thirty out of thirty-one of the cases developing in the first two weeks would be accounted for.

In the second place, as Dr. Hay says, "It is not usual for a milk-produced epidemic of typhoid to be followed by so many secondary cases as in the epidemic under discussion." As a matter of fact, the epidemic continued for three months; but, after the first four weeks, no special incidence upon any particular milk supply was observed. This continuance of the outbreak is strongly suggestive of some other influence than that of the suspected milk supply (or supplies) having been in question.

Thirdly, the distribution of the cases in the town, and of the cases in affected households, discussed on pp. 21 and 22 of Dr. Hay's Report, is unusual. In particular, the occurrence of the remarkable groups of cases, shown on the map (attached to the Report) and discussed on p. 22, should be mentioned. "In nearly seventy families there were two or more cases in each family, yielding a total of about 187 cases—or not far short of half of all the cases."

The Report is an exceedingly interesting one, but the case against milk cannot be regarded as proven, and hence the outbreak cannot be held to furnish strong independent support of the bacillus carrier hypothesis. These Scotch reports constitute a most valuable contribution to bacillus carrier literature, but it is clearly desirable that they should be studied in all their details, before any definite opinion is formed with regard to the epidemiological section of the inquiry in each instance. Granting the bacillus carrier thesis, these outbreaks may perhaps be thought to furnish confirmatory evidence, but as a secure foundation for that thesis they cannot be relied upon. It is a curious circumstance that all the outbreaks of typhoid fever, in which a carrier is believed to have infected milk, should be outbreaks in which the statistical evidence against the milk is not clear and unmistakeable. A further instance of this remains to be considered.

(4) By a singular chance the Folkestone case has been studied by two epidemiologists; by the first from an a priori, and then by the second, ten years later, from an a posteriori point of view. The epidemiological evidence in the case was originally examined by Dr. Thomson (Local Government Board Report, 1901-02), before the days of the carrier. Then suspicion as to a carrier was entertained, his existence was demonstrated bacteriologically, and we have Dr. Johnstone's Report of 1910. A comparison of the two reports is most instructive. In the Report of 1901-02, the relation of a milker, N., to outbreaks of typhoid fever was most carefully studied. There had been then two considerable prevalences, one in 1899 and one in 1900. With the latter (*see* p. 205, Report of 1901-02) N., it was argued, can have had nothing to do, and with regard to the former there was an element of doubt (*see* p. 201-2). But the same man, N., "had worked on two farms which supplied milk to six sufferers from enteric fever in Folkestone in 1896, and to fourteen sufferers in 1897; no case of a sufferer being supplied with such milk during 1898 is referred to. From the particulars given it appears that, at the time of the main prevalence, the milk with which the outbreak was associated was consumed, as the sole supply, or mixed with other supplies, in some 1,000 houses (or, say by 15 per cent. to 20 per cent. of the population of Folkestone). The

facts regarding N. were noted as constituting a "somewhat singular coincidence," but it was pointed out that, in any event, milk "*cannot be accepted as a full explanation of the local increase*" of fever, during the years in question. It may be added that it is now apparent that the years, 1899 and 1900, of excessive incidence of fever in Folkestone, were also years of exceptional incidence in many other parts of the country, and notably was this the case in London; the exceptional incidence in the two towns strikingly corresponds, moreover, in relation to season of the year. So far the 1901-02 Report; but cases, as was naturally to be expected, occurred from time to time (on an average about three cases a year) among consumers of milk with which N. was associated. This is just about the number that might have been predicted if such milk was still being supplied to from 15 per cent. to 20 per cent. of the population of Folkestone.

Then, in 1909, a group of twelve cases occurred; N. was working at the farm supplying the milk, but averred that he had not been milking cows at the critical period. This farm is, however, added to the three farms of the 1901-02 Report; and investigation into the past further revealed the fact that, fifteen years or more previously, N. had worked at a farm where cases of typhoid fever were stated to have occurred. There is nothing, it seems, to show that milk distributed from this farm caused mischief at the time.

Dr. Johnstone, in 1910, mentions that during 1901 and 1905 there was evidence suggesting that N. milk was not infectious; but on the whole he holds that the milk of all five farms must be regarded as suspected; and, having now in view the demonstration of the fact that N. is a bacillus carrier, he concludes that during the past fourteen years typhoid fever has been spread in Folkestone mainly by milk infected by the man N.

If the fact that N. has been found to be a bacillus carrier is to be regarded as settling the Folkestone case, one cannot demur to the inclusion (a posteriori) of any of the more or less doubtful cases of typhoid fever, mentioned in the Report of 1901-02, as cases of that disease, e.g., the case at first regarded as influenza at No. 2 farm;[1] the two cases at No. 4 farm (*see* footnote on p. 201 of 1901-02 Report); and the cases assigned to N. milk in May, 1896 and in 1898 (there is no evidence in the 1901-02 Report justifying such assignment). But if we are to approach the question still from an a priori, i.e., from a purely statistical point of view, the Report of 1910 materially strengthens the case against the supposition that milk infected by N. was at fault. Thus there is the apparent non-involvement of consumers of the milk supplied away from Elham, from 1893 to 1895; the apparent absence of excessive incidence, beyond what chance would account for, in Folkestone in the six years, 1902-08; the exceptions referred to on p. 14 (1910 Report). Finally, there is the consideration that the Elham Rural District, in which N. resided for fifteen out of the seventeen years, 1893 to 1909, shows, even when the cases with regard to which some doubt arises are included, an annual case-rate of only about 1 per 6,000 persons. This very low case-rate in Elham, which was harbouring a man who is supposed to have possessed quite a unique capacity for spreading infection, may be commended to the notice of those German and American authors who have recorded such dire results as occurring from association of persons with bacillus carriers. The Folkestone case cannot be regarded as decisive, and the importance of this fact cannot be insisted upon too strongly, for it is really the only case which has been exhaustively investigated on what may be termed statistical lines. It is, indeed, a very dêbatable point whether there is, in fact, any conclusive epidemiological evidence to show that typhoid bacillus carriers (or paratyphoid bacillus carriers) are a source of danger.

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[1] Cf. p. 197 of 1901-02 Report and p. 6 of 1910 Report.

In the light of this last consideration, it may even be urged that the question as to whether paratyphoid and typhoid bacilli are the causal organisms of paratyphoid and typhoid fevers should be still regarded as an open one. The frequent association of these bacilli with these diseases is readily explained on the hypothesis that the bacilli are common "secondary invaders," just as closely allied bacilli are in yellow fever and hog-cholera. In the case of paratyphoid bacilli their common occurrence in Nature, and, in the case of both typhoid and paratyphoid bacilli, their frequent discovery in healthy persons, removes difficulty which might be felt in explaining their appearance as secondary invaders. On the other hand, the fact that outbreaks, closely resembling typhoid fever, have been found in association with presence of bacilli other than the supposed causal bacilli, is easily explained if the secondary nature of the rôle played by the "causal organisms" be admitted. Such outbreaks have been described in India, associated with *Bacillus paratyphosus* A;[1] in Sumatra, associated with the same organism;[2] and in Manchuria, associated with an organism which resembled *Bacillus*

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[1] Grattan and Harvey, *Journ. Roy. Army Med. Corps*, 1911, xvi, p. 9.

[2] *Centralbl. f. Bakt.*, Jena, I Abt., Referate, 1910, xlvi, p. 234.

paratyphosus B, but gave a positive indol reaction.[1] Furthermore, on the hypothesis that the bacilli are really secondary invaders, it is easy to explain mixed infections of typhoid and paratyphoid, of typhoid and Malta fever,[2] and of paratyphoid and Malta fever.[3] It is also easy to understand, on this hypothesis, such recently published results as those of Vallet and Rimbaud[4] with regard to the common occurrence of intermediate forms between *Bacillus coli* and *Bacillus typhosus* in the intestine of the dog.

To return, however, to the problem of the balls in the bag. Even granting, the objector may urge, that there is no conclusive laboratory evidence as to the presence of red balls in the bag, there still remains the fact that numbers of persons who have been suspected, on a priori grounds, of being carriers, have been actually proved to be carriers on bacteriological examination. The following considerations are deserving of careful study in this connexion.

In the first place, the bacteriological examination when suspects are in question is, at any rate in some instances, a very arduous undertaking. In the case of control populations, on the other hand, such examination is, as a rule, a much more simple affair. Dr. Theodore Thomson states that, when an ordinary population is examined, "The results show considerable variations, partly dependent upon the differing degree of frequency with which bacteriological examinations were made and on the varying degree of efficiency of the methods employed." Moreover, he adds: "It cannot be said that investigation of this kind has yet been made on such a scale, or with such allowance for the varying circumstances of different populations, as to justify definite conclusion" (loc. cit., p. 247). Dr. Ledingham says: "It must be remembered that a single negative examination of the excreta can lead to no definite conclusion" (p. 315).[5] Again, Schumacher suggests that "The proportion of explained cases depends on the time that can be spent on inquiry, on the persistence with which the bacteriological work can be carried out, and, finally, on the willingness of the people concerned to supply samples for examination."

The proportion of carriers in an ordinary population, we are told, is about 3 or 4 per 1000. This result is arrived at, be it noted, upon single examinations, which "can lead to no definite conclusion." So much for the control population, now for the suspected one.

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[1] Horiuchi, *Centralbl. f. Bakt.*, 1908, I Abt., Orig., xlvi, p. 586.

[2] *Lancet*, 1910, ii, p. 330; also *Centralbl. f. Bakt.*, Jena, I Abt., 1910, Referate, xlvii, p. 14.

[3] *Centralbl. f. Bakt.*, I Abt., Referate, 1909, xliii, p. 185.

[4] *Centralbl. f. Bakt.*, I Abt., Referate, 1910, xlviii, p. 188.

[5] Conradi, it is interesting to note, has recently stated that a single *positive* result, as regards the paratyphoid bacillus, can lead to no definite conclusion.

Dr. Ledingham (p. 255) says, the proportion of instances in which the typhoid bacillus can be detected in the fæces of typhoid convalescents "depends almost entirely on the degree of efficiency of the bacteriological methods in current use"; again (p. 256), "with further improvements in technique a still higher percentage will be obtained"; he speaks, too (p. 259), of "enormously prolonged intermittency in convalescent carriers"; further, in considering the importance to be attached to absence of bacilli in supposed bacteriological cures, he says (p. 256): "such cures must be regarded with great caution"; and (p. 317), "in order to be certain that the patient is bacteriologically typhoid-free a much longer period of observation (than the sixteenth day after operation) is necessary." Once more (p. 332), "Evidence derived from such a short period of observation (end of eleventh week) proves nothing"; and, yet again (p. 336), "No definite verdict . . . can be pronounced until the cases so treated have been under bacteriological supervision for at least one or two years thereafter."

In contrast with Minelli's 0·4 per cent. (1 in 250), in a control population of prisoners, there may be cited the percentage of 11·6 reached by Semple and Greig in their work with convalescents (daily examination). In the infantry regiment of the St. Brieuc garrison (p. 263), a careful clinical examination was made of those who "showed the slightest symptoms of indisposition," and the fact was demonstrated that "a surprising proportion of them was found to give either positive Widal tests or typhoid bacilli in the fæces." In sixty-four atypical cases the stools of thirteen, or 20·3 per cent., gave positive results. Scheller (p. 292) found among forty healthy persons, who had consumed a suspected milk, no fewer than eighteen bacillus carriers. Schumacher was able to trace 26·6 per cent. of the 1,750 cases occurring in an outbreak at Cröv to contact with carriers; but his work (p. 362) "was limited only by the reluctance exhibited by a few families to provide material for examination, and even this appears to have been successfully overcome. . . ." And again (p. 362), "As the result of the extensive bacteriological work carried out . . . all the forty-five cases (1903-08) received adequate explanation."

In asylums large numbers of carriers have been found. Dr. Ledingham (p. 302) suggests that this is due to the fact that "the facilities for thoroughly carrying out bacteriological examination on a large scale are greater than is usually the case elsewhere, and correspondingly increase the probability of the detection of carriers." Nieter and Liefmann obtained a percentage of 2·8. Dr. Ledingham (pp. 306 and 307) records instances of 3 in 80, 3 in 110, and 7 in 110. But he says (p. 345): "In institutions a positive (Widal) reaction is occasionally obtained in persons whose fæces, though repeatedly examined, fail to show the presence of the typhoid bacillus. Certainly in such cases we have to reckon with intermittent periods, imperfect bacteriological methods, and possibly other factors not yet fully understood." Kamm had "an experience which was unfortunate and perhaps exceptional," 8 positive reactions in 136 samples, but no typhoid bacilli in the stools. "It would be interesting," says Dr. Ledingham, "to know the further history of these cases, and whether definite carriers were ultimately discovered." It would be interesting, also, to know something more about the further history (after their submission to bacteriological supervision, with daily examinations, for one or two years), of control populations, dismissed as free from typhoid bacilli on the evidence afforded by a single examination, "which can lead to no definite conclusion."

As a *dernier ressort*, there is Weber's "method of demonstrating the presence of *Bacillus typhosus* in a carrier, who may not at the time be actually shedding the bacillus in the fæces" (p. 348). "Weber passed a dose of oil (about 200 c.cm.) into the stomach, thereby causing regurgitation of typhoid-containing bile into that organ. After half an hour the stomach was emptied and the fluid obtained, which presented an upper oily layer and a lower watery bile-stained layer. In two chronic carriers, whose stools contained only a very few typhoid bacilli, he was able to recover the typhoid bacillus from the upper oily layer. It is not always possible, however, to obtain this back-flow of bile into the stomach."

What would Minelli's 4 per 1,000 have been raised to had he adopted Semple and Greig's daily examinations; increased his percentage by tracing the further history of any cases, at first giving negative results, but later showing a positive Widal reaction in 1 in 50 dilution; and finally dealt with all the outstanding instances by Weber's method? It cannot be argued that typhoid bacilli would have been found in a very large proportion of the prisoners, for, as Dr. Ledingham warns us (p. 330), "It must be remembered that our bacteriological methods are not yet sufficiently delicate to detect minimal quantities of typhoid bacilli in excreta."

Another point must not be forgotten. A particular suspected person now and again proves to be a carrier. Other suspected persons turn out not to be carriers. Rosenthal (p. 277) examined the fæces of thirteen suspects and found one carrier. At Lailey's Row (p. 286) fifteen persons were examined; Kossel (p. 289) examined fifteen persons; Lumsden and Kastle (p. 292) thirteen persons; Scheller (p. 292) conducted an "extensive investigation from a carrier point of view." Friedel (p. 304) carried out a like comprehensive investigation with negative results, and then later, a more extensive one, with the outcome that a female, aged 65, was found to be a carrier. Kayser (p. 290) had to have recourse to "much touring among the farmers who supplied the milk." Semple and Greig examined all the "cooks and contacts" in a particular regimental unit, and again, "a large number of healthy soldiers," before obtaining a positive result.

A remarkable military instance presenting peculiar difficulties is that described by Niepratschk (p. 311). The cooks and assistants failed him, so he began "a systematic examination of the 'Unteroffizieren' of the individual batteries." At length Sergeant B. was found to be a carrier, excreting $2\frac{1}{2}$ millions of typhoid bacilli per cubic centimetre of urine. The battery he belonged to was not specially affected; all three batteries were equally involved. It was suggested, in explanation of this anomalous circumstance, and as no hitherto recognized mode of spread could be appealed to, that the men who removed the stable litter had especially suffered, and hence that Sergeant B. must have urinated in the stables. Sergeant B., however, denied this, alleging that he had always used a particular urinal, so the alternative hypothesis is advanced that, here, by splashing, the trousers and boots of men standing near him were contaminated, and the significant fact is noted that those who cleaned these boots suffered; moreover, the shoemaker who mended Sergeant B.'s boots contracted typhoid fever. Curiously enough all but two of the fellow "Unteroffiziere" of Sergeant B. escaped, but they did not brush their own boots. Again, there was a handrail covered with wood, on the surface of which Sergeant B.'s hand must frequently have rested, as he passed up the steps, leading from the urinal to the canteen room, to obtain his glass of beer; and it is suggested that the hands of the sufferers also rested on this handrail, for the entire strength of all three batteries ascended this particular flight of stairs. But, if this handrail caused so much mischief, and if Sergeant B. infected with such unerring certainty the boots and trousers of the man who stood next him in the urinal, it is quite impossible to understand why the "Unteroffiziere" with whom Sergeant B. was day by day intimately associated in multitudinous ways escaped as lightly as they did.

As far as I can learn the most exhaustive series of examinations of a typhoid carrier is that recorded in the *Journal of Pathology and Bacteriology*, July, 1910, by Walker Hall and Roberts.[1] The woman was under first one and then another form of treatment, and her urine, fæces, and blood were examined *daily continuously for fifteen months;* for nine months the urine gave negative results. The writers say: "These daily examinations made such demands on our time that it was impossible to investigate the matter from other standpoints." Examinations of so exhaustive a kind on a control population, say on Minelli's 250 prisoners, would be an exceedingly serious undertaking. The prospect opened up is, from some points of view, a visionary one. With such extension of the activities of the working bacteriologist as would be likely to result, there should be great increase, in the near future, of opportunity for acquirement of special knowledge; indeed, the expert might hope, at no distant date, to attain to the degree of proficiency and precision of the workers, described by Swift in his account of Laputa, who, on a strict view of suspected excrements, were able to form a judgment of men's thoughts and designs, and who had satisfied themselves that the "ordure of the man who was plotting regicide presented characters clearly distinguishing it from that of one who thought only of raising an insurrection or burning the metropolis."

The report of Niepratschk prompts the reflection that bacillus carriers are nothing if not specialists. For example, there is Kayser's baker's wife, whose victims were "apprentices"; Mayer's manager (p. 277) infected a water-closet; the mischief attributed to Rosenthal's female carrier (p. 277) was wrought by means of vegetables; Kayser's Frau H. (p. 280) contaminated well water; and his Frau B. fruit and pastry; Friedel's (p. 304) carrier's medium was potato salad. There are several infections of milk; it should, however, be noted that in one of them[2] the milker at fault suffered from facial paralysis, and "had no control over his flow of saliva" . . . and this "might have become mixed with the milk at milking time." Finally, there is the happily unique case of Niepratschk, already discussed, in which straw, boots, trousers, and the wooden handrail on the stairs leading to the canteen room, may have all played a part. It has been pointed out that the Folkestone carrier, despite his milk record, did not cause a high attack-rate in Elham; but this circumstance is far less extraordinary than the fact that Sergeant B., who infected so many men in two batteries to which he did not belong, did so little mischief among individuals with whom he was especially associated. Again, the account given of Sergeant B. makes one wonder whether the man who brushed Mayer's "manager's" boots and trousers escaped; how it was that his shoemaker was not attacked; and whether any bacteriological examination was made of the handrails of any flights of stairs which, at one time or another, this manager may have ascended.

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[1] *Journ. Path. and Bact.*, Camb., 1910, xv (*Proc. Path. Soc. Gt. Brit. and Irel.*, July 8-9, 1910, p. 120).

[2] *Lancet*, 1910, ii, p. 791.

It is noteworthy that the attempt to obtain a previous history of typhoid in persons who are found to be carriers is fraught with extreme difficulty. Dr. Ledingham refers (p. 355) to "accumulated knowledge regarding atypical and apparently symptomless enteric fever on the one hand, and typhoid carriers with no history on the other." Then there is the need for going back so many years. For example, in Klinger's case (p. 258) the attack dates back thirty years; an American case (p. 265) dates back thirty-eight years; Kayser's case (p. 275), thirty years; Mayer's (p. 276), forty-nine years (i.e., to 1857), one knows not with what precision typhoid fever was diagnosed in the Bavarian Pfalz sixty-four years ago; Rosenthal's very dubious case dates from 1878 (p. 277); Greig's case goes back to 1856; and the illness of Miss S. occurred in 1877. Dr. Ledingham says (p. 267): "The difficulty of obtaining accurate past histories is . . . very considerable." He thinks some carriers may have had typhoid fever in a slight or atypical form; in other words, that accurate knowledge would be likely to increase the percentage of cases with a previous history of typhoid. There is always, however, the possibility that accurate knowledge would diminish the recorded percentages.

Again, there is the uncertainty as to the nature of the illness in many instances. Compare, for example, the histories cited by Baumann (pp. 281 and 282), Watt (p. 283), by Johnstone at Jennet Hill (p. 285), the Kasauli case (p. 310); and Mrs. H.'s febrile attack of May, 1908 (p. 329), is another instance in point. It is, moreover, worth while remembering that "Typhoid Mary" is not known to have ever had typhoid fever, and that the attack of the Folkestone carrier himself is based upon slender evidence. "The only severe illness he was able to recall occurred about thirty years ago. At that time he had suffered from lassitude, lack of appetite, and general malaise; and, though he was not bad enough to take to his bed, the illness had left a lively impression on him and his wife."

Even when, after pressing inquiry as to history of typhoid fever far back into the past, success is achieved or within sight, there may still be the difficulty Seige[1] has referred to. There is a suspected person with a possible history of typhoid, but a positive bacteriological result is wanting. It may not be easy to remedy this defect. Thus quite recently a Neolithic skeleton was discovered in Essex, within which were seeds of the dog-rose and blackberry. The man must therefore have died in the autumn, and he was apparently a young adult. A positive result here, in a possible case of typhoid fever of 4,000 years ago, would on ordinary contact infection principles have once and for all accounted for every case of the disease occurring since Neolithic times.

Further, there are the instances of non-effective carriers, such as those of Dean (loc. cit., p. 321), Conradi, &c.; and, in particular, those kept under close daily observation in America and in this country. In addition to all this there is the need for further explanation, in relation to the carrier hypothesis, of the peculiarities of distribution of the lesions met with in carriers; and there are striking anomalies, to which the President of this Section has called attention, as regards incidence of the carrying propensity upon age and sex, and the failure of ascertainable effect after a considerable body of presumably infective material has been implanted in a community as the result of a water outbreak.

The most important point of all, however, in connexion with study of the class of difficulties under consideration, is the fact that it has now become clear that account has to be taken of the possibility of "mutation." Certain ordinary intestinal organisms may apparently acquire the capacity of invading the blood and tissues, and at the same time may assume characters closely simulating those of pathogenic forms. This is the case in hog-cholera, for example, or in the epizootic of guinea-pigs, described by Petrie and O'Brien. The idea that such changes may occur has again and again been mooted, but to the demand of the precisians for ocular demonstration there has been, in the last two or three years, actual response. Few had ventured to anticipate that the operations of Nature could be mimicked, even under the limitations of laboratory conditions, but in the light of the work of Reiner Müller, Massini, Twort, Ainley Walker and Burton Bradley, it is clear that there is need for reconsideration of the position as regards supposed specific distinctions.

Koch remarked at the Tuberculosis Congress at Washington, that the possibility of the transformation of the typus humanus into the typus bovinus was of great theoretical but no practical interest. A similar point of view is that which has been presented, in a detailed statement, by McConkey, in a paper dealing with those members of the *Bacillus coli* group which ferment lactose. He says: "From a practical standpoint it matters not whether these organisms are different bacilli or one bacillus in various guises. If organisms isolated from a certain material (A) give, in the majority of cases, a certain series of reactions (a_1, a_2, a_3), and if organisms isolated from some other material (B) give, in the majority of instances, a different series of reactions (b_1, b_2, b_3), then we would be justified in associating the series (a_1, a_2, a_3) with the material (A), and the series (b_1, b_2, b_3) with the material (B); and if we found both the (a_1, a_2, a_3) series and the (b_1, b_2, b_3) series given by organisms isolated from (B), we could presume that some of (A) had become mixed with (B). It might be that both series of reactions were given by the same organism, and that the difference was simply the effect of environment. This would not affect the conclusion. It would only show that the organism had not been long enough in (B) to have its (a_1, a_2, a_3) series altered by the changed environment."

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[1] *Klin. Jahrb.*, Jena, 1905, xiv, p. 507.

There are probably few who would be prepared to accept this statement without modification now. The work done in connexion with hog-cholera or with the guinea-pig epizootic, just referred to, makes it clear that there is need for having in mind the possibility that in a given material (A) there may lurk unrecognized organisms capable of giving reactions (b_1, b_2, b_3). But, then, it has been objected, consideration must be limited, in such a connexion as this, to study of pure cultivations; what occurs in Nature, so the argument may conceivably run, is of theoretical interest, but there is the practical—what happens in the test-tube. The demand made is an extremely exacting one; at first sight it seemed a hopeless task to attempt to meet it; but, now, on Reiner Müller's plates and in Twort's fermentation tubes the unexpected has happened. Mutation has been actually encountered in pure cultivations; it can no longer be alleged, therefore, that it does not occur in Nature.

Those, on the other hand, who have always disputed the claim that it is incumbent upon the believer in mutation to demonstrate its occurrence under laboratory conditions, will now, more than ever, insist that the environmental influences in the test-tube can no more be presumed to teach all that happens in the living body than, to quote the famous simile, the jelly on the strand can be expected to tell dry sea-beach gazers how it fared "when there was mid-sea and the mighty things."

But, in further objection, it may still be urged that the verdict concerning the limitations of the fermentation test (pronounced in the case of Gram-negative cocci, for example, by Blair Martin, and in that of the streptococci, by Ainley Walker) does not affect the position as regards the typhoid bacillus; for here, we are told, agglutination can be appealed to as well as fermentation. Dr. Ledingham, however (p. 347), observes that there are "many members of the non-lactose group whose appearance on plates may simulate strongly that of the typhoid colony, which may indeed give a slight reaction with a potent antityphoid serum, but which can be absolutely excluded by their fermentation and other properties." So that, once again there is a circle in the proof after all. Dr. Twort told this Section that "no one was justified in asserting that a certain organism did ferment a particular sugar and another did not; it was purely a matter of degree." What becomes, if his view be accepted, of the distinctions drawn[1] between *Bacillus typhosus* and some other non-lactose fermenters? We have been recently assured that Morgan failed to find *Bacillus typhosus* in diarrhœa stools; but in his paper he showed that bacilli of "Type Flexner" are of quite common occurrence in such stools; and the criteria differentiating such bacilli from typhoid bacilli must now apparently be admitted to be of an evanescent character. It is deserving of note that the use of media of selection has demonstrated the fact that some typhoid strains are able to grow on media upon which others are completely inhibited. Moreover, Dr. Ledingham (p. 353) insists that, in testing the virulence of strains of typhoid bacilli, "as small an interval as possible should elapse between isolation of the strain and the determination of its virulence"; again, Scheller (p. 354) believes that "an avirulent bacillus may suddenly regain its virulence."

Whether, however, mutation of bacilli ordinarily present in the human intestine, resulting in their becoming typhoid bacilli, occurs or not, there is abundant scope for the transformation at any time of any Londoner into a healthy typhoid carrier. Dr. Houston, in a recent report, has given the result of a search for typhoid bacilli in London drinking water (raw Thames water), estimating at the same time the delicacy of the method employed. He found one typhoid bacillus in 12,000 c.c. of water (twenty-four samples), and his test was sufficiently delicate to enable him to detect typhoid bacilli, provided there were more than seventy or eighty in each 500 c.c. of water examined. On this basis of reckoning (and, imperfect as it is, it is the only one at present available), each Londoner consumes in filtered water[1] not more than one typhoid bacillus a day, but more than one bacillus about every three weeks.

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[1] *See* Morgan and Ledingham's Table (*Proceedings*, 1909, ii, pp. 148-149).

When bacteriological methods are sufficiently delicate to permit of the detection of the typhoid bacilli we all harbour, it will no doubt be admitted that we are all healthy bacillus carriers. What will then be the significance of the discovery of the presence of typhoid bacilli in the excretions of a person suspected of having infected others with typhoid fever?

CONTACT INFECTION.

An examination of the evidence upon which outbreaks of typhoid fever have been attributed to the influence of bacillus carriers, illustrating strikingly as it does the need for checking the results obtained in a suffering population against those obtainable in a control population, suggests application of a similar procedure in making investigations with respect to contact infection generally.

In dealing with carriers, as with drain effluvia, water, shellfish, and the rest, regard must always be paid to the likelihood, a priori, of being able to associate, as a mere chance coincidence, one or another possible cause with particular cases of disease. This principle must also be borne in mind in connexion with supposed cases of contact infection. There has of late years been a marked tendency to assume that, if a case *may have been* infected directly from a previous case, as a matter of fact, it *has been* so infected. There is no doubt a sort of excuse, in dealing with an individual case, for unhesitatingly accepting, in the absence of something better, any probable or even likely explanation. But, as soon as a number of cases can be brought under review, the correctness of a generally accepted doctrine can be tested by applying control methods.

The popular belief with regard to contact infection can be subjected to scrutiny in this way by examining the facts as to the attack-rate of nurses in institutions. Judging by the Census Return, the resident female staff in the large hospitals (including fever hospitals) and infirmaries of London approximates, roughly speaking, to about one per cent. of the female population over 10 years of age. Again, examination of the London notification returns for 1899 and 1900 shows that the incidence of typhoid fever upon the said staff was twenty-two cases in 1899 and thirty-four cases in 1900. This case-rate does not very much exceed that upon other women of corresponding ages in London in those years. This is a rough test at best; we are on surer ground, however, if consideration be limited to the Metropolitan Asylums Board Hospitals. The official reports of the last twenty years show some 200 cases of typhoid fever (allowance must, of course, be made for instances in which the same case appears in the returns for two consecutive years, and is thus liable to be counted twice over), an average of about ten cases per annum. If the mean population at risk be taken at 4,000, and the average case-rate for women of the ages in question at 8 per 10,000, only some three or four cases per annum could be expected to occur as a mere chance result.

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[1] The reduction in the number of total bacteria effected before water is delivered to the consumer averages about 99 per cent.

But two considerations need to be borne in mind. Typhoid fever more particularly affects certain sections of the London population, and hence question rises as to whether the female staff at the fever hospitals have circumstances in common with the sections of the population which escape or with those more especially attacked. Again, the likelihood of detection of atypical and obscure cases of typhoid fever in a fever hospital is certainly much greater than it is outside such institutions. In correspondence with this the low case-mortality of typhoid fever occurring in the Metropolitan Asylums Board staff is deserving of note. In the light of these two considerations an annual average of ten cases cannot, it may be submitted, be regarded as being greatly in excess of the number which might have been expected to occur, apart altogether from any supposed special risk from contact infection.

A very remarkable point about the typhoid fever of fever hospitals is the tendency of the cases to present themselves in groups. In some instances, moreover, the occurrence of a group of cases inside the hospital coincides with development of localized prevalence of the disease in the neighbourhood of the hospital. This was notably the case, for example, at Homerton, in 1893, 1899, 1900, and again in 1910. In the last-named year there was reason for thinking the cases inside and outside the hospital were due to a common cause.

In the well-known discussion on the direct infectivity of typhoid fever[1] but scant attention was given to the question as to whether the case-rate in nurses, &c., is higher than might be anticipated on a mere chance distribution of the cases. Both Dr. Caiger and Dr. Goodall, however, stated that the nurses, &c., attacked in the fever hospitals under their control were, in a very large proportion of instances, ascertained to have been "working in enteric fever wards." For the last ten years great significance has been attached to this observation; but it is desirable that there should be a clear understanding as to what is meant by "working in enteric fever wards." In connexion with the development of five cases of typhoid fever among members of the female staff at Homerton last autumn, Dr. Goodall and I carefully considered this question. Three, perhaps four, of the sufferers had nursed typhoid patients. We found that, on a cursory view, roughly, less than 15 per cent. of the control population were "typhoid nurses"; but, on full investigation, with help of carefully kept records as to employment of the nurses, and after questioning a number of the nurses themselves, it transpired that, taking account of changes of duty, of "days off," of temporary substitution of one nurse for another, and so on, instead of 15 per cent., about 50 per cent. of the nurses had, in one way or another, been associated with typhoid fever cases in the preceding two months. When typhoid fever is diagnosed in a nurse working in a fever hospital, any association with typhoid fever patients counts. The "sufferer population" is very closely questioned as to such contact. The "control population" must obviously be just as closely questioned. The point which should be determined is whether the nurses attacked by typhoid fever are, or are not, in overwhelming proportion, singled out from among those engaged in continuous attendance upon typhoid fever cases. There has, in the past, undoubtedly been room for much misapprehension here. Another difficulty is that in atypical cases, when judgment as to the diagnosis hangs in the balance, there may be a disposition to lean to typhoid fever, if there is a history of contact with typhoid patients, and there is not the same disposition in the case of a nurse with no history of exposure to "contact risks." Yet again, it cannot be doubted that a nurse who has been working among typhoid cases is more likely to report malaise (with headache, &c.), and to suspect that she is suffering from typhoid fever, than is a nurse who does not believe herself to have been exposed to risk. I have been told by a nurse that she ascribed her attack to having had charge of a particular patient, when, as a matter of fact, having regard to the question of dates, infection from the supposed source could almost certainly be excluded.

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[1] *Trans. Epid. Soc. Lond.*, 1899-1900, N.S., xix, pp. 164-198.

In the light of these considerations one goes back to the carefully weighed judgments on direct infection of men like Murchison and Fagge,[1] and when we are told that typhoid fever has changed its type, and that it now shows ability to spread by contact infection, whereas it did not do so in Murchison's day, we may well remember the warning of Murchison himself, in a somewhat similar connexion, and consider whether the diversity may not reside, in some degree, at any rate, "in the mental revolutions of practitioners rather than in the actual revolutions of disease." Kelsch[2] has observed that, with our predecessors, contagion was nothing, with our contemporaries it is everything. Both cannot be right, and, with regard to a question which turns on careful examination of actual facts, our predecessors are not likely to be in error, for in this field of inquiry they are past masters.

If we turn from the study of hospital staffs to that of contact infections in the ordinary household, we are at once involved in the gravest difficulty. Such sequences of cases as those discussed in Dr. Goodall's paper, or those referred to in many annual reports of medical officers of health in recent years, may of course have been due to contact; but, generally speaking, careful consideration will show that they may at least equally well be traceable to repeated re-introduction of infection into the household. Each localized prevalence must be taken on its own merits, but there is good reason for thinking that, speaking generally, a greatly exaggerated view of the importance of contact infection prevails at the present time.[3]

LENGTH OF INCUBATION PERIOD.

These considerations make it desirable to say a word, in conclusion, on the subject of length of incubation period. It is generally assumed that the incubation period in typhoid fever does not exceed one month. The study of several outbreaks of typhoid occurring in London prompts the suggestion that the period may be longer than this, and may extend to at least six or seven weeks. Thus, in an outbreak, in Southwark in 1900, believed to be due to food infection, those cases, which developed more than four weeks after the presumed date of distribution of the infected food, were, in accordance with the accepted view at the time, classed as "secondary," though with some hesitancy in particular instances. It is now, however, by no means clear that these cases were not really primary cases. Again, in Shoreditch in 1908, the dates of occurrence of onset of symptoms in a few instances appeared to cast doubt upon the ordinarily accepted view as to length of incubation period.

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[1] Vide *Trans. Epid. Soc. Lond.*, 1899-1900, N.S., xix, p. 164 *et seq.*

[2] *Rév. d'Hyg.*, Par., Jan., 1911.

[3] I have discussed this question at some length in reports on particular typhoid fever outbreaks in 1908 and 1910.

Last autumn opportunity was afforded for further study of this question. The dates of onset of symptoms of the cases constituting two special prevalences, made the subject of study, arranged themselves in two main groups, one having a time-range from August 10 to late September, the other a time range from September 1 to the middle of October; in each instance a period of some six or seven weeks. The most natural explanation of the occurrences was found to be a food infection hypothesis, and a detailed study of all the circumstances in the individual cases distinctly favoured the supposition that single infections were responsible for the prevalences. Accepting this view, it would be necessary to assume that, in extreme instances, the incubation period of typhoid fever may extend over as long a period as six or seven weeks.

In support of such a contention appeal may be made to the Winchester outbreak, reported upon to the Local Government Board by Dr. Bulstrode, in 1901. Some of those who partook of the implicated oysters did not, it will be remembered, develop symptoms until after the lapse of over a month—nearly five weeks in two instances. Moreover, Klinger[1] states that he has found the incubation period extend to fifty-four days.

It is noteworthy that a few of the late attacks, in London in 1910, were second cases reported from households already invaded, and some of these late cases might therefore quite reasonably be regarded as "secondary" cases. But one of these late cases was not secondary to a previous attack in the same house, and the man was almost certainly infected at least five weeks before the recorded onset of symptoms, as he partook of the suspected food only on three or four specified days at the end of the week ending Saturday, August 13. There were, moreover, other instances in which the incubation period was in all probability a long one.

The view held by an investigator with regard to "length of incubation period" materially influences his attitude towards "direct infectivity." Are the second and third cases in houses which occur two, three, or more weeks after the date of a food or water infection to be regarded as "primary" or "secondary"? The usual practice is to refer them to contact infection, but there are reasons for not too unquestioningly adopting this course. Thus it will be noticed that these supposed secondary infections come absolutely to an end within a limited number of weeks after the date of infection of the primary cases. In the great water outbreaks (Lincoln, Maidstone, Worthing, &c.) this is borne out. Again, in the London prevalence of last autumn, all the supposed "secondary cases," in a particular locality, were those of persons who had eaten some suspected food obtained from particular suspected shops, and it should be further noted that they were in all instances very mild cases. In slight attacks of typhoid fever it is difficult to fix with precision the date of commencement of illness, and some of the patients may have developed symptoms earlier than the date specified. Beyond this it is known that approximately 12 per cent. of the recognized cases of typhoid fever suffer from "relapse." This fact suggests the possibility that some of the outlying mild cases, occurring five or six weeks after distribution of an infected food or water supply, are really instances of relapse following upon unrecognized or "ambulatory" typhoid fever. Finally, it may be remarked that, following upon a single case of typhoid fever in a house, there is a tendency to diagnose any obscure illness in the household as typhoid fever also. This tendency was apparent in London last autumn in certain instances, and the history of recorded outbreaks of typhoid fever suggests that the same influence has been at work elsewhere.

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[1] *Arbeit. aus dem Kaiserl. Gesund.*, Berl., 1909, xxx, p. 584.

In endeavouring to bring before the Section reasons against thinking mainly or entirely on "living lines," in dealing with typhoid fever, reference has been confined absolutely to discussion of the shortcomings which those who do not accept the bacillus carrier hypothesis regard as constituting its sins of commission. There are, of course, also sins of omission; the failure of the hypothesis to throw light upon the general epidemiology of typhoid fever, seasonal prevalence, age and sex incidence, distribution in time and place, association with spread by ingesta, and so on. The discussion of all this is beyond the scope of the present paper. There are, however, it may be observed, many who think that the sins of omission of the bacillus carrier hypothesis constitute a more serious indictment against it than do its sins of commission.

DISCUSSION.

The PRESIDENT (Dr. Theodore Thomson) expressed the thanks of the Section to Dr. Hamer for his paper, in which he had marshalled all his facts with his usual skill, and discussed them with great lucidity. Many of the points raised were of extreme interest. Some of them perhaps would not command universal agreement, particularly among bacteriologists.

Dr. LEDINGHAM, in opening the discussion, said he hoped he might be excused if he refrained from complimentary remarks, as he intended to traverse Dr. Hamer's paper entirely in a controversial spirit. It was now nearly ten years since the importance of the typhoid carrier in spreading typhoid fever came to be recognized. The typhoid carrier had come as a boon and a blessing to the bacteriologist and the epidemiologist, and he failed to understand Dr. Hamer's statement that a state of mental paralysis overtook the epidemiologist when he was informed of the result of the examinations of excreta of a suspected typhoid carrier. So far as his own experience went he had been more struck with a state of mental exultation. Dr. Hamer remarked in the paper that a priori reasoning had been lacking in the investigation of particular epidemics associated with typhoid carriers. He (Dr. Ledingham) would point out that in the discovery of the typhoid carrier a priori reasoning only was employed, and it was only after all other possible causes of infection had been exhausted that recourse was had by certain acute minds to the possibility that the typhoid bacillus might sojourn for long periods in the animal body. He thought the main position of the typhoid carrier as an infecting agent in the spread of typhoid fever remained at present unchallenged, although he admitted that numerous difficulties still remained to be explained, from both the epidemiological and bacteriological standpoints. With regard to the question of carrier infectivity to which Dr. Hamer referred, and the instances given in his (Dr. Ledingham's) recent report, he wished to state that in compiling those instances for publication he had had the great advantage of Dr. Theodore Thomson's advice. It frequently happened that the evidence which suited him (the speaker) was not enough for Dr. Thomson, and accordingly they generally made a compromise. In some cases, however, where for the sake of completeness of proof he proposed tendering elaborate evidence of a negative character from examinations of water supply, &c., Dr. Thomson had advised its omission as the evidence already given was sufficient. It was quite inaccurate to state that these cases ultimately traced to carriers had not been carefully investigated from other points of view. With regard to the general objections brought by Dr. Hamer, he had cited Conradi's view, that it was difficult to prove that the carrier was infectious, because he was generally discovered in infectious surroundings. Dr. Hamer had forgotten the other facts in the history of the typhoid carrier question; he forgot that though it might be difficult to prove that the carrier was prima facie infective, if that carrier were isolated, no further cases arose. Also he omitted to state that it had been shown by the German statistics that after the carriers had been isolated or advised with regard to hygienic precautions, use of disinfectants, &c., the percentage of cases due to those carriers had markedly diminished. The evidence was given in that report, and could be relied upon. There were also cases due to declared carriers. Carriers had been found, but they had neglected to take the proper hygienic precautions, and further cases had been found to arise from them. He might also instance a recent case which had come to his notice. An asylum in Germany which had been typhoid-ridden for many years was enjoying a period of immunity from typhoid fever owing to the discovery and isolation of carriers, when another case arose. The person affected was the nurse in attendance in the carrier isolation ward. That, in his opinion, was a beautiful instance of the infectivity of the carrier. There was also good evidence of carriers having infected themselves. A recent instance which he noted had occurred in a case of a surgical operation on the gall-bladder for gall-stones. There was no question at the time of the patient having had typhoid, or of typhoid bacilli having taken any part at all in the causation of the gall-bladder lesion. Sixteen days after the operation, however, the patient developed typhoid fever.

Passing to the instances of infectivity which Dr. Hamer had quoted from his (Dr. Ledingham's) report, he (the speaker) was not prepared to admit that in the investigation of small country endemics or domestic outbreaks it was possible to apply statistical methods. The population involved was too small, the matter was complicated by the occurrence of secondary cases, by the infrequency in the excretion of the typhoid bacillus, and by other factors. In institutions statistical evidence was impossible. Nowadays investigators had admittedly preconceived notions as to the presence of carriers, and accordingly did not now sit in an armchair and work out the probabilities of contact of one patient with another; they looked upon all persons in a typhoid-ridden institution as suspected persons; not suspected in the sense meant by Dr. Hamer. Statistics were inapplicable also, owing to intermittency in the excretion of the bacillus by the carrier. Perhaps this circumstance more than any other rendered explicable the occasional prolonged non-infective periods. He knew that Dr. Hamer did not believe that the typhoid bacillus was the cause of typhoid fever, and therefore they (Dr. Hamer and he) were starting on different planes. It would have been better if Dr. Hamer, in his paper, had confined his attention to one aspect of the subject—e.g., the question whether the typhoid bacillus was the cause of the disease—because his views on the carrier question were largely coloured by his conception of the essential ætiology of typhoid fever.

With regard to the individual epidemics referred to by Dr. Hamer, these would perhaps be dealt with by those present who happened to be responsible for the investigation of those cases. He (the speaker) apologized for any bias he might have had in reporting cases, but Dr. Hamer himself had not been quite free from bias, though his was in the direction of shellfish, and especially fried fish. Recently he had read a report by Dr. Hamer on the association of fried fish with cases of typhoid fever, and he seemed to have traced most of the cases to two or three fishmongers' shops. From Sir Shirley Murphy's report for 1905 it appeared that those fish in the course of frying were subjected to a temperature of 172·5° C. Two years ago Dr. Hamer put forward a view which, so far as one could understand, was that typhoid fever, or in fact any infection, was due to some subtle reaction between a specific "ferment" or "activator" and an associated bacillus. The latter might even be, in the case of typhoid fever, a transmuted *Bacillus coli*. Dr. Ledingham took it that if fried fish was subjected to a temperature of 172° C. the "activator" could not be a ferment or a living virus. The virus of anterior poliomyelitis was destroyed at a temperature of 70° for an hour, and other viruses could be killed by similar temperatures. The virus in fried fish must be of the nature of a ptomaine; when the ptomaine got into the intestine it found some bacilli there, possibly the ordinary colon bacilli, on which it acted as a stimulus. Perhaps Dr. Hamer's "activator" met the "ubiquitous" typhoid bacillus forthwith, endowed it with pathogenicity, and so produced typhoid fever. Dr. Hamer's "activator" was his substitute for the varying susceptibility of man to disease, but that factor Dr. Hamer did not recognize throughout his paper. Dr. Hamer said he had always kept the carrier hypothesis in mind throughout his investigation. He (the speaker) looked through the report to find if any investigation had been made on that point, and whether typhoid bacilli had been found in the excreta of the people in the fishmongers' shops. He found no evidence of that, and therefore he asked whether such investigation was made, and if not, why it was omitted. After all, a living typhoid bacillus was better than a dead fish. Dr. Hamer, in referring to certain instances of carrier infectivity, had remarked that typhoid carriers were nothing if not specialists. They were bound to be specialists. If a milkman handled milk, milk was most likely to be the medium of conveyance: if an old woman sold vegetables, vegetables were most likely to be the means of conveyance. They were the articles through which the carriers came into contact with other people. Dr. Hamer considered it was preposterous to make a point of the presence of typhoid bacilli on the handrails of stairs up which a regiment of soldiers marched. He (the speaker) did not think it at all unreasonable. Lieut.-Col. Sir D. Semple had recently informed him that on several occasions he had invited his army carriers into the laboratory without their knowing what he wanted, and got them to place their hands on Conradi plates. He detected typhoid bacilli on those plates afterwards. Neither was it unreasonable to associate the cleaning of the boots with the occurrence of the fever. Urinary carriers were particularly infective, as the urine contained thousands of bacilli in every drop. He would leave the criticism of the Aberdeen cases to be dealt with by the Aberdonian observers, who were not hampered by heterodox views with regard to the typhoid bacillus. Dr. Hamer laid great stress on the postulates of Koch. But bacteriologists had now outgrown Koch's postulates, as Koch himself outgrew them. The chief postulate on which Dr. Hamer insisted was that typhoid fever must be transmissible to animals. But the evidence was clear that man, who was an animal, could be infected by the typhoid bacillus alone, without any hypothetical virus in the case at all. There was the unfortunate series of typhoid fever cases which occurred at the Lister Institute some years ago, and he asked whether those cases could be explained on any other basis than that the infection was due to a stock typhoid bacillus with which those persons were working. There was there no question of an additional hypothetical virus. The "activator" in these cases was probably the lowered vitality of the persons infected, probably owing to the strenuous life in the laboratory. Dr. Hamer said that the typhoid bacillus was ubiquitous, and that if sufficiently frequent examinations were made all persons would probably be found to harbour it. He (the speaker) admitted it would be well if larger populations were examined from that point of view, but he thought it extremely unlikely that all persons would be found to be bacillus carriers. Dr. Hamer assumed that the typhoid bacillus might be a purely secondary invader of typhoid fever, analogous to the secondary position assumed by the *Bacillus suipestifer* in hog-cholera. The typhoid bacillus occurred, however, in every case of typhoid fever; but that was not so in hog-cholera concerning the *Bacillus suipestifer*. It had also been noticed that there was little or no agglutination of the *Bacillus suipestifer* by the serum of the affected swine. Then there was the question of atypical cases of typhoid fever. Apparently Dr. Hamer would not admit the varying susceptibility of man to disease as a factor in the production of infection. If there was one fact which had emerged from all recent bacteriological work it was that cases of infection with the typhoid bacillus could occur with very varying clinical symptoms. The same was true of dysentery and cholera. Bacteriologists recognized that fact as much as did epidemiologists. Dr. Hamer referred to the guinea-pig epizootic recorded by Petrie and O'Brien, but in that outbreak the *Bacillus suipestifer* was not found invariably in the cases which succumbed to the virus. All bacteriologists admitted that the recent work on variation and mutation in bacteria was most important. The typhoid bacillus was everywhere regarded as the most constant organism in respect of its sugar fermentations, and he could see no evidence that the work on mutation and variation had influenced current views on the specificity of these reactions. In fact, probably mutation in the sense of De Vries did not occur in bacilli. Evidently those organisms had the power of varying only in particular directions; there was no evidence that it would ultimately be possible to transmute the *Bacillus coli* into the *Bacillus typhosus*, though it might be possible to trace the purely vegetative evolution of those bacilli. But even though the *Bacillus coli* was so transmuted that it was indistinguishable from the *Bacillus typhosus*, it would still be a masked *Bacillus coli*. He objected to the term "intermediate types." In respect of sugar fermentations there was a smaller gulf between the *Bacillus pestis* and the *Bacillus typhosus* than between the latter and *Bacillus coli*. Dr. Hamer was not professedly a bacteriologist, therefore those points might not appeal to him. Bacteriologists were well aware of what was called heterologous agglutination; heterologous agglutinins could generally be separated off by the absorption method of Castellani. Finally, with respect to Dr. Hamer's statement that dysentery bacilli of Flexner type had been frequently found by Morgan in cases of summer diarrhœa, he wished to point out that such was not the case.

Dr. BAINBRIDGE said that on the question of the ubiquity of the paratyphoid bacillus, he thought Dr. Hamer had read the paper by Conradi, which suited his views, and had not paid attention to anything else which had been written on the subject. Conradi's statement that paratyphoid bacilli were very common in Nature had not been confirmed; in fact, it had been repeatedly denied, and in this country, at any rate, those bacilli had not been found, with one exception, in any but paratyphoid fever patients, or in persistent paratyphoid carriers. There was no reason to suppose that that bacillus was a secondary invader. With regard to the typhoid bacillus being a secondary invader, the fact that the bacillus could be demonstrated in the blood, even during the latent period before the disease had started, was against that view of a secondary hypothetical virus, which indeed had been looked for in typhoid fever, but without success; it had also been looked for without success in infections by the *Bacillus enteritidis*. With regard to mutation, he did not understand the remark: "Certain ordinary intestinal organisms may apparently acquire the capacity of invading the blood and tissues, and at the same time may assume characters closely simulating those of pathogenic forms." The bacillus was the same, whether it was in the intestinal canal or in the blood. There was no question of change in the characters of the bacillus. There seemed to be no good evidence that the transformation of one bacillus into another had ever been completely produced in the laboratory; whether it could occur in Nature would be very difficult to prove. True, a bacillus could be induced to take on certain new characteristics and lose others by careful training; but he believed that spontaneous changes of that kind were very rare, and even so they were not complete.

Mr. M. GREENWOOD, jun., said that, as a statistician, he was in general sympathy with Dr. Hamer's method, because he thought one function of a bacteriological laboratory was to provide material for the statistical investigation of disease, and that such an investigation was of fundamental importance. He wanted to ask a question about the Folkestone outbreak, because there seemed to be some discrepancy between Dr. Hamer's view of the statistical facts and the statements made in Dr. Johnstone's report. The problem for the statistician was to determine whether the incidence of typhoid on the consumers of the suspected milk was greater than one could attribute to chance, in view of the number of persons supplied with milk from the different sources. According to Dr. Johnstone's report there were four farms, milk from which might have been contaminated by the carrier—farms 2, 3, 4, and 5. Dr. Johnstone wrote[1] that, as far as could be learned, the average number of houses supplied daily with milk from No. 2 farm was 100; No. 3, 170; No. 4, 120; No. 5, 100; 170 houses daily was the largest average supply. The population of Folkestone was about 36,000, and it was necessary to suppose that something like 180 persons lived in each house to bring up the number of the population obtaining their milk supply from these farms to a number which would give the actual incidence of cases as described in the table, excluding imported cases. Dr. Hamer said: "From the particulars given it appears that, at the time of the main prevalence, the milk with which the outbreak was associated was consumed, as the sole supply, or mixed with other supplies, in some 1,000 houses (say by 15 per cent. to 20 per cent. of the population of Folkestone)." He was puzzled to see how one could get 1,000 families supplied, when the average daily supply to houses was 170 at the most. It was necessary to have milk every day, and so it was difficult to reconcile those two figures, even allowing for casual supplies. To him the only reconciliation possible was by supposing the incidence of typhoid in Folkestone to be limited to some special district in which 25 per cent. of milk was obtained from that particular farm. But he could not see any evidence in the Report that such was the case. Another explanation was, that all the cases were omitted which occurred up to 1901-02. Such an admission would not be a priori justifiable. There was a series of persons, some of whom obtained milk from a particular supply, and others not; could those figures be reconciled with a pure chance distribution of typhoid? If one took the supply as stated, and allowed, e.g., ten persons per house as a rough number, then in the series of years the expected incidence would be about 12, and the actual incidence among persons supplied with N milk was 207. He did not see how one could reconcile the facts with a chance hypothesis unless one supposed that at every period of N's activity the milk supply of more than half of the population of Folkestone passed through his hands, and there seemed to be no evidence in the Report that such was the case. It would not be fair to attach great importance to the possibly large number of persons who obtained casual supplies from N without making allowance for the dilution of N milk with unsuspected milk. Dr. Hamer had not sufficiently explained the basis of his conclusions that the distribution was a chance one. It seemed to him that the question of the general frequency of carriers, as Dr. Hamer stated the problem, was a matter of definition. The argument appeared to be that every person in whom the bacillus had been detected was a carrier, and that every other person would be found to be so if enough examinations were made. The intensity of the condition must be considered; on such lines as those of Dr. Hamer one could never use statistical evidence. If it were to be a question of the relative prevalence of insanity in two towns, for instance, one might say the prevalence in town A was ten times that in B on the basis of the number of certified lunatics; but it was open to anyone to retort that everyone was more or less mad, and that if the test were fine enough the insanity rate would be found to be 100 per cent. in both towns. Such an objection would not be of much practical importance, however, and the same might be said of certain difficulties raised by Dr. Hamer.

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[1] Dr. R. W. Johnstone's "Report to the Local Government Board on Occurrences of Enteric Fever in the Folkestone Urban District," 1910, p. 11.

Dr. E. W. GOODALL remarked that the paper dealt with many important subjects—the question of contact, length of period of incubation, mutation of one kind of bacillus into another, and the further question of whether the typhoid bacillus was or was not the cause of typhoid fever. Any one of those subjects was sufficient for an evening's discussion. The special point he wished to deal with was that regarding infection by contact, more especially as applied to persons who were attending upon typhoid patients. When he introduced a discussion on that subject at the Epidemiological Society ten years ago he particularly pointed out that the infection in typhoid fever was not conveyed from the patient to the healthy person in the same way as the infection of small-pox or that of measles was supposed to be conveyed. It was conveyed through particular channels out of the body of the patient, chiefly by the excreta, and subsequently gained an entrance through the alimentary canal of the person attacked. The person attending on the patient probably became infected either by food contaminated by his own hands or directly from his hands. With regard to the attack-rate, the incidence of typhoid fever on the nursing staff in the hospitals of the Metropolitan Asylums Board was, according to Dr. Hamer, much about the same as it was upon women of that age throughout London generally. But he (the speaker) did not agree with that. [Dr. HAMER: I said ten nurses were attacked where only three could be expected to be attacked.] Dr. Goodall remarked that that was a very large difference, and was equal to admitting that there was something very special in their surroundings. When typhoid fever arose in the neighbourhood of a hospital those suffering from the disease were sent into the hospital, and from them the nurses caught the disease. Hence when typhoid was prevalent in the neighbourhood it was also prevalent amongst the nurses, and their infection was not due to the same cause as was the general infection of the neighbourhood. He believed that the nurses did get the disease from their patients. In an investigation of the tables of illness of the staffs published by the Asylums Board he found that those hospitals to which there were no typhoid cases admitted were practically free from typhoid amongst the staff. On this question of diagnosis, he could say that the cases of typhoid amongst the staff were average cases: there was no question of putting any doubtful cases into the records. He thought the incidence of typhoid fever outside hospitals was somewhat exaggerated owing to errors in diagnosis. Seldom was the proportion of errors of diagnosis less than 25 per cent. in cases admitted to hospital. He thought that the explanation of the lower mortality among the nurses at Homerton than among the general patients was that the nurses were brought under treatment much earlier. With regard to Murchison's views, that observer was obsessed with certain curious ideas of the origin of typhoid fever, and those ideas could be read in all his writings. Murchison denied that typhoid occurred amongst the nursing staff of hospitals at all. We knew now, and even Dr. Hamer admitted it, that on that point Murchison was wrong. At

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[1] *Trans. Epid. Soc. Lond.*, 1899-1900, N.S., xix, p. 164.

the very time Murchison was enunciating his views, a correspondence was in progress in the medical journals showing that typhoid fever occurred amongst hospital staffs, especially if their members were nursing typhoid fever patients. Much as he would like to be able to say that the staff did not catch the disease from patients, he was convinced in his own mind that they did. Some years ago he looked up the question of the length of the incubation period of typhoid fever. One wanted to know in that respect when the exposure to infection ceased, and taking reported cases in which exposure to infection was known to have existed for a definite period, he was unable to find an incubation period of longer than twenty-three days, and that was in a person who had been discharged from a gaol where typhoid fever had been prevalent during his incarceration. He was, however, disposed to admit that the incubation period of typhoid fever might be longer than was usually admitted. He supposed it was possible for the typhoid organism to be absorbed into some of the glands of the intestine, and remain there for a considerable time latent, just as the tubercle bacillus could, without producing symptoms. In regard to some diseases, it would be as well to avoid speaking of an incubation period at all; it was doubtful whether it should be spoken of in reference to scarlet fever or diphtheria. While congratulating Dr. Hamer on the ability with which he had put forward his views, he could not help thinking that they were one-sided, and that he had been as guilty of important omissions as the writers whose opinions he had been attacking with so much vigour.

Dr. BUCHAN said that it was important for epidemiologists to observe, in investigating outbreaks of typhoid fever, that the finding of an associated bacillus carrier by the bacteriologist was not necessarily a full explanation of all the epidemiological facts, and therefore the termination of the inquiry. Dr. Hamer had shown by an analysis of several outbreaks attributed to carriers that there were difficulties in the way of accepting without restriction the bacillus carrier hypothesis. In some instances, for example, the carrier had appeared on the scene after the outbreak began. In other cases the epidemiological facts indicated a particular person as a carrier, but bacteriology was unable to confirm the suspicion. This had been explained by Dr. Ledingham by saying that present bacteriological methods were not sufficiently delicate to detect minimal quantities of the *Bacillus typhosus*. While this statement by Dr. Ledingham would naturally be accepted, it did not transform suspicion into fact. Another point to be noted in connexion with carriers as vehicles of infection was that it was often many years after the initial attacks of typhoid, and without further association with any case of typhoid fever, that they suddenly gave rise to outbreaks. Then there was the difficulty of obtaining a previous history of typhoid fever in many cases of carriers, and again, there were carriers who, up to date, had not given rise to a case of typhoid fever. These facts were bound to make us careful, and ought to make us try to assess as accurately as possible the epidemiological value of the new bacteriological facts with regard to typhoid fever which had recently been discovered. What were these facts?

It was now established that there were bacillus carriers who had had typhoid fever. This fact naturally raised the question of the length of infectiousness in cases of this disease, and it was possible that the prevalence of typhoid fever could be explained by its being carried over from year to year and place to place by persons who were continuously or intermittently infectious. Similarly it had been established that there were persons who were carriers who had not had typhoid fever, and the possibility of these people being in the incubation stage of the disease, with that stage much longer than usual, had to be borne in mind. The case of typhoid fever following upon an operation on the gall-bladder mentioned by Dr. Ledingham lent support to this view. Dr. Hamer had stated that some recent investigations by him had made him extend the incubation period to six or seven weeks, and Dr. Buchan believed that there were occasions when the incubation period of the disease was much longer than that. In any case, long infectious periods and long incubation periods as corollaries to the bacillus carrier theory helped us to explain some of the features of typhoid fever.

Dr. Hamer raised a point when he stated that possibly the importance of contact infection was much exaggerated at the present time. In investigating 946 cases in Birmingham, Dr. Buchan had been able to ascertain that there was contact infection in only 124, or 13 per cent. Of the other cases, some were due to shellfish, some even to fried fish, and some to watercress, and 661 cases, or 70 per cent., were unaccounted for, and were certainly not due to contact. Dr. Ledingham, in opening the discussion, had made a point that certain fried fish said by Dr. Hamer to have caused typhoid fever had been heated to a temperature of 172·5° C., and could not, therefore, have caused the disease. Dr. Ledingham did not say how long they had been so heated, but Dr. Buchan would like him to know that in experiments which he (Dr. Buchan) had conducted mussels had been heated in a steam sterilizer at 100° C. for as long a period as four and a half hours without producing sterility.

In dealing with contact infection, the methods of infection also required consideration. Dr. Hamer had shown that in cases of bacillus carriers it was necessary to assume that the infecting person infected everything with which he came in contact, and it was to be noted that the path of infection in all cases was assumed to be by the mouth. Dr. Buchan did not wish to quarrel with either of these assumptions, but it had to be fully realized that they were assumptions if it was desired to progress and to establish facts.

Perhaps the most important point in the paper was that dealing with mutation. Under certain circumstances it was known that the *Bacillus coli communis* entered the body and caused disease—appendicitis, cystitis, pyonephrosis, &c. Possibly the same or some other organism more or less constantly present in the human body might under certain conditions in a similar way cause typhoid fever, but he did not think any definite pronouncement could be made in the present state of our knowledge.

To sum up, then, and to explain the occurrence of typhoid fever on the bacillus carrier theory it was necessary to assume that the bacillus carrier could be continuously infectious, intermittently infectious, and auto-infectious, that prolonged periods of infectiousness and incubation might occur, that the carrier might infect anything with which he came in contact, and that the path of infection was by the mouth. If, on the other hand, one tried to explain the occurrence of typhoid fever on the mutation theory, no assumption was necessary, except so far as mutation itself was an assumption. Neither theory explained, however, the seasonal prevalence of typhoid fever or its sex or age distribution. Obviously there was still much to be explained and accomplished by workers in this field of research, and he felt sure that Dr. Hamer's paper would stimulate thought and work, and that the criticism to which Dr. Hamer had subjected certain findings was in the best interests of epidemiological science.

Dr. W. J. PENFOLD said: It had to be admitted that variations of the fermentation properties of *Bacillus typhosus* did take place. The variations that had been described were the following:—

(1) *Bacillus typhosus* had been trained to ferment lactose by Dr. Twort. This required two years' constant growth on lactose media. He (Dr. Penfold) had examined the strain so produced, and on plating on lactose he found it always a mixture of fermenting and non-fermenting elements. If subculture was made from a fermenting colony, again a similar mixture of colonies resulted. He had trained between twenty to thirty strains of the *Bacillus typhosus* for nearly two years on lactose media, and so far had got none to produce fermenting colonies. It appeared, therefore, that the negative character of *Bacillus typhosus* in respect of lactose was very stable indeed.

(2) Only one mutation in the strict sense had been described in the case of *Bacillus typhosus*, and that was in respect of isodulcite. It was a curious thing that R. Müller examined seventy strains of the organism isolated without reference to this character, and found they all mutated in this way, and in about the same time. He had up to date examined over thirty strains, and obtained the same result. This character tended to show the essential similarity of all typhoid strains, and R. Müller declared it to be the most specific test for *Bacillus typhosus*.

(3) All typhoid strains fermented dulcite broth slowly. Occasional tubes did not show full acidity as indicated by litmus, but if two or three tubes were put on from any strain full acidity occurred. So here again the variation in dulcite media was general for the whole species.

Dr. Horrocks had recently revived the idea that *Bacillus typhosus* could be converted into an organism of the *Bacillus alkaligenes* type.[1] This was formerly alleged in Germany, where further work appeared to discredit it. Dr. Horrocks's paper gave an account of thirty-seven experiments with five successful conversions of the *Bacillus typhosus* into an organism of a non-fermenting type, and one experiment where partial success in this direction resulted. Out of the five, three were inoculated from the same strain of *Bacillus typhosus*, and it seemed not unlikely that this strain might have been contaminated. The partial conversion was also of the same strain. Dr. Horrocks's methods were by

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[1] *Journ. Roy. Army Med. Corps*, 1911, xvi, p. 225.

(1) symbiosis with *Bacillus coli*, (2) growth in extract of fæces, and (3) growth in mixtures of urine and tap-water. These results called for thorough examination.

It was clear, therefore, leaving aside Dr. Horrocks's work which required further examination, that variations of *Bacillus typhosus* in respect of carbohydrates either (1) were very slow in taking place—e.g., lactose—or (2) occurred in a definite manner—e.g., isodulcite—where all the strains mutated in the same direction in about the same time. These latter facts tended consequently to reassure us in the use of the sugar tests for *Bacillus typhosus*, and also in believing the organism to be a very well defined and definite entity.

The PRESIDENT said he wished to offer a few comments, because Dr. Hamer had made a strong point of the value of what he called a priori investigation. He agreed with the author that that was very important. In many of the numerous investigations which he (Dr. Thomson) had read where outbreaks of typhoid fever had been referred to a carrier on insufficient evidence, there had been too much a posteriori reasoning. It had been said: "Here is a carrier, and therefore that is the cause," without taking care to exclude everything else. It was the duty of the epidemiological investigator, when he had set up a theory, to demolish that theory if he could. Many investigators, if they found a carrier, thought that fact settled the matter.

He had a few reproaches to address to Dr. Hamer, because he did not think that all the epidemics had been investigated in the incomplete way Dr. Hamer indicated. He (the speaker) had known some which had been done very well indeed. He made that remark particularly because of the Folkestone outbreak, on which Dr. Hamer's comments did not seem quite sufficient, nor could he say they were strictly accurate. It was possible to be misled by the way in which Dr. Hamer put the matter. For instance, he said: "It may be added that it is now apparent that the years 1899 and 1900 of excessive incidence of fever in Folkestone were also years of excessive incidence in many other parts of the country, and notably was this the case in London." That was true, but that statement had no value of itself unless the two sets of facts were closely compared. He got out the facts in London, and found that for 1891-95 the mean annual average of enteric fever cases was 3,284, and in 1899 4,467—i.e., one-third more. In Folkestone during the period 1891-95 the mean was 7 per annum, while in 1899 the number of cases was 87—i.e., twelve times as great as the previous mean. So the comparison which Dr. Hamer made did not help one in the matter. Dr. Hamer also said: "With regard to the outbreak in 1900, it was argued that the carrier could have had nothing to do with it, and with regard to the former (the outbreak in 1899) there was an element of doubt." That was a reference to his (Dr. Thomson's) Report, and he did not think it expressed what he found. The truth was that the vast majority of the cases of enteric fever were associated with a milk supply which was derived from two farms. He was beaten; he did not know what was the cause of the infection of the milk, and he suggested as much in his report. He had to fall back upon the hypothesis that the series of milk outbreaks up to then had been associated with the water supplies from different farms in different years. When he had to consider which was the milk probably infected on the vaguest of evidence, he had to be guided by the sanitary conditions of the places, and he chose the farm in which the water supply was the worst. He had a further argument in one of the years—namely, that on the farm on which the carrier was working the cows were shifted after the epidemic had begun to another farm three miles away, and the fever still continued. The cows were brought back again to the original farm, and still the fever persisted. He therefore concluded that the cause could not be associated with that particular lot of cows, so that the other farm must be at fault. But he had really missed the point—namely, that the carrier was working at that farm and when the cows were transferred he went with them. He had had the chance of being the first person to discover the intestinal carrier, but he missed the opportunity. Dr. Johnstone went down later and detected the situation.

However the truth might stand about the *Bacillus typhosus* being the pathogenic agent in enteric fever, he was satisfied that workers had made a distinct advance from the epidemiological standpoint in the discovery of the carrier. Whether that bacillus was the causative agent of enteric fever or not, the fact remained that in some epidemics, such as that at Folkestone, it had been clearly proved that a milk supply had caused fever, and that the milk had been associated with a particular person who was a carrier. That was valuable knowledge which could and ought to be utilized in the epidemiological investigation of enteric fever.

Dr. HAMER, in reply, said Dr. Ledingham and he could not see the same side of the shield on the subject under discussion. Dr. Ledingham relied upon particularly suggestive instances. He said, "Here is a beautiful instance of a nurse who was infected when she was working in a carrier ward." But he (Dr. Hamer) did not regard this individual case as a beautiful instance, but asked, "How many such instances have been recorded?" Again, Dr. Ledingham referred to the infection of laboratory workers; but he (the speaker) did not regard the reported cases as conclusive. The question was whether, having in view the number of nurses at risk, and the number of laboratory workers at risk, there were more cases among nurses and laboratory workers than might be expected as the result of mere coincidence. Carriers were deemed to be infective because they came from infected surroundings, as Conradi had said. In the *Lancet* of March 25, 1911, there was "A Record of Ninety Diphtheria Carriers." When he (Dr. Hamer) read the title of this paper he thought here was proof at last; but so far as he could see, there was no suggestion that any of these carriers (with one doubtful exception) had done any mischief at all. With regard to fish infection, to which Dr. Ledingham had referred, temperature was not a serious difficulty. It was a common experience to find, on cutting open a fish which had been cooked, uncoagulated red fluid at the sides of the vertebral column. This showed that the heat had not penetrated. Again, living nematoid worms were sometimes seen in cod, served at table, after the fish had been subjected to the ordinary process of frying. With regard to Dr. Ledingham's point as to the need of searching for bacillus carriers, he would have endeavoured to have had examinations made of the excreta of several of those associated with the outbreaks in London last autumn, but it transpired, in numerous instances, that material had already been sent to the Lister Institute; possibly the results of the investigations would be reported at a later date. The main point, however, was this: in the original fish outbreak of 1900, three distinct fish-shops in widely separated parts of London were responsible for spreading the disease; it was difficult, on the carrier hypothesis, to account for the involvement of these three shops at one and the same moment. Moreover, in 1910, thirty-three shops and stalls were implicated. So that clearly the fish was infected before it left Billingsgate. As to Dr. Ledingham's final observations, the expression used in the paper (p. 126) was not Flexner type but "Type Flexner," in inverted commas. The reference was, as was indeed stated (p. 126), to Morgan and Ledingham's paper.[1] Not being professedly a bacteriologist he had, in fact, taken the precaution of sheltering himself under authority. It was perhaps well he had done so, for it appeared now that the two writers quoted were being taken to task by the one last named. The views of experts as to nomenclature were seemingly as liable to undergo mutation as the very germs themselves. The fact, mentioned by Dr. Bainbridge, that he had looked in vain for the hypothetical virus did not finally dispose of the possibility of its existence: it might be found at some future date. Dealing with Mr. Greenwood's remarks as to the number of houses supplied with the infected milk in Folkestone: in the later report this number was given as from 100 to 170; but, it was added, "It sometimes happened that the retailer obtained milk from other sources and mixed it with that obtained from one of the above farms, thus conveying infection in smaller doses over a wider area." The earlier report,[2] however, gave much more detail. More than 1,000 houses were supplied in 1899 (p. 200) and nearly 1,000 houses in 1900 (p. 203); and the possibility is mentioned (p. 207) that other persons may have consumed one or other of the infected milks, "without this fact having been detected, owing to the practice among milk vendors of occasionally supplying on their rounds small quantities of milk to other vendors whose stock may be running short." In the 1910 Report the chance supplies are over and above those to at most 170, houses; in the 1901-02 Report these chance supplies are additional to those furnished to some 1,000 houses. He (Dr. Hamer) considered he was justified in stating that at least 15 per cent. of the population of Folkestone drank the suspected milk. With regard to the suggestion that we all harbour typhoid bacilli, Mr. Greenwood said everyone had some mental peculiarity, and yet segregation of the insane was practised. So, it would be remembered, Hamlet was sent to England because his madness would not be noticed in a country where all the men were as mad as he. It was interesting to pursue the analogy between typhoid fever and insanity further. Dr. Ledingham spoke (loc. cit., p. 329) of "those bacillary invasions which have been found to occur in mentally enfeebled persons." Why should it not be assumed—thinking on "living lines"—that insanity was due to these invading bacilli; that they were causal, not secondary? A case of mental disease would then be explained on discovery of the fact that the sufferer had, say, placed his hand on a contaminated handrail, or brushed the boots of a previous sufferer. If no such record of contact with actual sufferers were reported, it would still be open to the inquirer to obtain material from a normal person, if such an one could be found; in this material the offending causal organism would of course be demonstrable and proof of causal relationship would then be complete. With regard to the nurses in the hospitals of the Metropolitan Asylums Board, he agreed with Dr. Goodall that there were circumstances "very special in their surroundings." For one thing these nurses were attended by experts in the art of diagnosing typhoid fever, and it was no extravagant assumption to make that this in itself played a part in determining the exceptional incidence observed. Dr. Penfold's investigations were very interesting, but it was surprising to find so much importance attached to mere negative results. It did not follow, because a man went on a number of successive days to his ordinary place of work, that he had never been abroad. He agreed with Dr. Penfold that Dr. Horrocks's results called for thorough examination. Reiner Müller's contention, referred to by Dr. Penfold, that the ability of the typhoid bacillus to undergo a certain mutation was a specific characteristic, was interesting. We were formerly assured that the typhoid bacillus did not undergo mutation; now a bacillus that did not undergo a particular mutation could not be the typhoid bacillus. The stone which Dr. Ledingham had rejected, when building up his argument on the subject of "The Enteric Fever Carrier," had become, in the hands of Reiner Müller, the head of the corner. With reference to the President's remarks, it was difficult to adequately summarize a report in a few paragraphs. He gathered from Dr. Thomson's report that there were reasons for thinking that, at any rate in some of the years in question, the milk supply with which the particular milker was concerned was not a supply to which any suspicion could be held to be attached. There was, moreover, the difficulty, as stated, that reliance had to be sometimes placed upon the "vaguest of evidence." But the task of Dr. Johnstone was a fortiori more difficult still (in so far as he relied upon his restatement of the facts), for the later inquiries relative to the earlier outbreaks were necessarily instituted after the lapse of years. In studying the differences in the proportionate incidences of typhoid year by year in London and in Folkestone, one had to have regard to the fact that the former was a large town with multitudinous sources of infection, the latter a small town, with which longer intervals of comparative freedom from excessive prevalence of disease were naturally associated. It was exceedingly fortunate that the Folkestone case had been made the subject of inquiry by the President of the Section, and that at a time prior to the promulgation of the bacillus carrier hypothesis.

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[1] *Proceedings.*, 1909, ii, p. 148.

[2] *See* Supplement to Thirty-first Annual Report of Local Government Board (1901 2), p. 190.

Epidemiological Section.

April 28, 1911.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

The Problem of Pulmonary Tuberculosis considered from the Standpoint of Disposition.

By WILLIAM BULLOCH, M.D., and MAJOR GREENWOOD, Jun.

IT has long been a commonplace of medical writers that the development of most diseases requires the co-operation of two sets of circumstances. On the one hand, the organism within which the morbid process is to unfold itself, must conform to certain conditions of structure and function. This is the so-called "internal" cause of the disease. On the other hand, some agent, the "external" cause, actually or functionally outside the organism, must exert an effect, peculiar to itself and a property of its own structure, upon the organism which is in process of becoming the seat of the disease. In a large number of instances we now know that the second of the above-mentioned factors belongs to the groups of simply organized plants or animals. Hence the metaphor of "soil and seed" has been conventionally employed to express the belief outlined in the above sentences, and for the present we shall conform to this usage. It is evident that soil and seed, being biologically distinct, may be supposed capable of independent variation. Fixing our attention upon any one kind of seed, we may imagine that in the whole "population" of such seeds all grades of aptitude to germinate may be present. Similarly in the various soils, all grades of fertility for the growth of particular seeds may exist. Since the two variables are independent, the co-existence of any grade of the one with any grade of the other is a function of the relative frequencies in each "population." Evidently if the proportion of very readily germinating seeds in the seed "population" be extremely high, and the proportion of very infertile soils in the corresponding soil "population" very low, then, for that particular seed the choice of soil is relatively unimportant. We see instances of this in such diseases as measles. On the other hand, we have reason to believe that in the case of a disease like leprosy, fertile soils are uncommon. In the case of tuberculous disease, especially in its pulmonary manifestations, medical opinion has undergone fundamental changes. There was a time when the "soil" was regarded as of overwhelming importance, the existence of the "seed" being unknown or misjudged. With the discovery of the tubercle bacillus the trend of popular and, in many places, medical opinion has been to minimize the importance of the soil, if not to disregard it altogether. For reasons too obvious to require statement, it is desirable to know accurately what light has been shed upon this point by the large number of investigators who have studied it. We propose, therefore, to submit the results of a critical inquiry as to the value of work dealing with the respective shares of soil and seed in the genesis of pulmonary tuberculosis. Before, however, entering upon the details of this work it is desirable to refer to certain general aspects of the matter which are of great importance, even if frequently overlooked.

It is to be carefully noticed in the first place that no direct evidence on a matter of this kind can be obtained—so far, at least, as human beings are concerned. Were it possible to choose at random a large number of persons, to divide them at birth into two sets, to bring up one class in contact with exceptional opportunities for infection, and the other in circumstances presenting what one may call usual, normal, or wonted facilities, an examination of the differential incidence of phthisis would settle the question once and for all. No true experiment of this kind has been, or is likely to be, done. Cases often quoted as fulfilling the necessary conditions will not, we believe, stand critical examination. It has, for instance, been suggested that a comparison between the incidence of tuberculosis upon persons in attendance on tuberculous patients and the general rate will lead to results of value. When, however, the subjects of such a comparison are relatives of the diseased persons the question of inherited disposition to tuberculosis is begged. When the subjects of such a comparison are nurses or sick attendants, the difficulties of class incidence are not allowed for. Illogical conclusions of the following nature are frequently met with in popular medical writings. It is pointed out, perhaps, that the incidence of phthisis upon the attendants in some chest hospital or sanatorium for consumptives is much lower than upon the members of the households of tuberculous patients of the working classes. This difference is, then, attributed to the greater care exercised by the attendants in such institutions to avoid infection, as contrasted with the carelessness of hygienic measures and overcrowding which characterize families below the poverty line. Such reasoning is based on the assumption that all persons are equally liable to develop phthisis, given an equality of chances to become infected. In other words, there is a circle in the proof. Precisely the same criticism applies to the inferences often drawn from the higher prevalence of tuberculosis among the poorer classes in general, and in overcrowded dwellings in particular. It will, therefore, be apparent that all arguments of this kind require to be carefully weighed, and that in no circumstance can they be held to be of more than secondary importance. It is equally clear that the increased prevalence of phthisis in the families of diseased persons cannot of itself demonstrate the importance of a disposition to the malady in particular stocks, since such an inference would in turn beg the question of increased opportunities of exposure to infection.

A second preliminary observation relates to the direct transference of laboratory or veterinary experience to the realms of human pathology. Such a transference frequently savours of errors similar to those pointed out in the jests relating to the mechanics of the examination room, where wheels are frictionless and the weights of (very small) elephants may be neglected.

We shall now briefly review some of the more important work dealing with the problem under discussion, outlining first of all the state of medical knowledge prior to the discovery of the infectivity of tuberculosis. Secondly, we shall examine briefly the more recent literature dealing with the immediate problem, and finally some reference will be made to the bearing of these results.

Before, however, dealing with these details, it may be well to consider the concepts, which have been variously described as disposition, "anlage," diathesis, dyscrasia, constitution, habitus, temperament. Unfortunately these terms are used differently by different authors, although at one time they were considered as almost synonymous, and they were the subject of speculation too theoretical to be otherwise than mentioned here. The importance of this aspect of disease was largely developed by the Montpellier school, who separated "diathesis" from "disposition." This is clearly brought out in Sarda's learned study, and even more distinctly in the following definitions from a thesis by Pagnon (1838), from which it appears that a diathesis is an "état général et acquis de l'économie, modification insaissable dans son essence, pouvant rester indéfinitivement latent et se traduisant à l'extérieur sous l'influence d'une cause quelconque par des phénomènes identiques dans plusieurs points. Il ne faut pas confondre la diathèse avec l'aptitude à contracter les maladies. Cette dernière résulte de conditions particulières de sensibilité qui peuvent se rencontrer sans aucune modification morbifique."

There has also been a growing tendency among the German pathologists to restrict the meanings of such words as "disposition" and "disease-anlage." The recognition of the *causa essentialis* and the *causa occasionalis* of a disease is of importance. Thus, in his excellent introduction in the Krehl-Marchand "Handbuch der allgemeinen Pathologie" (1908), Marchand says: "Ein Krankheitsanlage im eigentlichen Sinne ist eine innere Causa essentialis, die ohne notwendiges Hinzutreten einer anderen Ursache die krankhaften Veränderungen zur Folge hat oder zur Folge haben kann wenn auch in vielen Fällen eine äussere Einwirkung als 'Gelegenheitsursache' (causa occasionalis) die Krankheit selbst hervorruft." After referring to conditions like hæmophilia, colour-blindness, and developmental irregularities, he goes on to say: "In allen diesen Fällen ist die angeborne Anlage eine ihrem Wesen nach uns unverständliche Eigenthümlichkeit der Organisation oder der 'Konstitution' die eine gewisse Minderwertigkeit in dieser oder jener Sphäre bedingt und selbst schon den Keim der Krankheit darstellt.

"Eine andere Reihe von sogenannten Dispositionen oder Anlagen ist *nicht* essentieller Natur. Man versteht darunter die grössere Empfänglichkeit oder geringere Widerstandsfähigkeit des Organismus gegenüber gewissen äusseren Einwirkungen die die eigentliche Krankheitsursache darstellen. Niemals kan eine solche 'Disposition' die Krankheit selbst zur Folge haben."

Birch-Hirschfeld uses the terms "Anlage" and "Disposition" as interchangeable. "Als Constitution bezeichnet man die Gesammtanlage des Korpers, sowohl hinsichtlich der Menge und des Verhältnisses seiner einzelnen Bestandtheile zu einander, als auch nach dem Maass seiner activen Leistung in der einen oder anderen Richtung mit Einschluss seiner Reizbarkeit sowie seiner passiven Widerstandsfähigkeit. Der Ausdruck der Constitution in der äusseren Erscheinung wird als Habitus bezeichnet; während die Art der Reaktion die namentlich auch in den psychischen Bewegungen hervortritt als Temperament benannt wird."

Like Marchand, Baumgarten also distinguishes the "disease-anlage," from the disposition. "Unter Anlagen," he says, "verstehen wir eine derartige in der Zellensubstanz bereits vorhandene Abweichung ihrer

Lebensbewegung von der Norm, welche sich wandelnd durch die in ihr liegende Richtung aus sich selbst heraus früher oder später zu den Erscheinungen führt, oder führen kann, welche zu einem einheitlichen Bilde zusammengefasst wir die Krankheit nennen . . . Unter Disposition verstehen wir den einen der für das Zustandekommen einer bestimmten Krankheit notwendigen Faktoren."

Ribbert refers to the Disposition as "diejenige Eigenschaft eines Organismus welche krankheitserregende Ursachen eine erfolgreiche Einwirkung gestattet."

These quotations will suffice to show the advisability of separating the "disease-anlage" (diathesis) from that condition of susceptibility to disease which is commonly called "disposition." With special reference to tuberculosis this is found running through the whole of medical literature. At first described as the phthisical "constitution" or "habitus," we find, later, such terms as "procatarxis," "predisposition," or "diathesis" as essential elements in the conception of this disease. In point of time we find in the classical writers, mainly the idea of constitution or habitus. Later, writers like Richard Morton, Desault, and others, described accurately what we should call the "predisposing" factors; while, at a still later period, Bayle founded the doctrine of the tuberculous "diathesis" in the strict sense of the word. Subsequent to the period of Laennec and Louis there was a gradual return to the doctrine of disposition which, in combination with the *causa essentialis,* determined the occurrence of a tuberculous infection. The relative importance of the disposing as contrasted with the infective element is the subject under discussion.

The description of the consumptive type created by Hippocrates runs like a thread through the whole fabric of medical literature. In the third book of the "Epidemics" he says: "The form of the body peculiarly subject to phthisical complaints was the smooth, the whitish, that resembling the lentil, the reddish, the blue-eyed, the leucophlegmatic, and that with scapulæ having the appearance of wings."

In the pseudo-Hippocratic treatise "Prorrhetics" it is stated that phthisis is difficult to remove if hereditary.[1] In the sixth book of the "Epidemics," which is probably spurious, there is a passage which suggests a belief in the constitutional factor. In Van der Linden's Latin translation it is given: "Morbus tabidam speciem inducit si talis ex natura aeger fuerit ex tali morbo superstes evadet; et alia eodem modo."[2]

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[1] Adams's translation, i, p. 259.

[2] Leiden, 1665, i, p. 817.

Galen, in his account of the phthisical type makes the same remark as Hippocrates with respect to the scapulæ, and he notices the narrow, shallow chest which frequently figures in later accounts.[1] Celsus, while reproducing Hippocrates's clinical description, does not refer to the existence of a disposition, but he recommends taking "a long voyage, change of climate, taking care to remove to a grosser air" (Book III, chapter xxii).

Pliny (23—79 A.D.) also makes no reference to disposition, but incidentally we may remark that he refers to a treatment popularly supposed to be quite modern: "Silvas eas dumtaxat quae picis resinaeque gratia radantur utilissimas esse, phthisicis, aut qui longa aegritudine non recolligant vires satis constat; et illum coeli aera, plus ita quam navigationem Aegyptiam proficere plus quam lactis herbidos per montium aestiva potus."[2]

The post-Galenical writers, Aretaios, Rhuphus of Ephesus, Oreibasios Aëtios, Alexandros of Tralles, and Paulus of Aegina were collected in a single volume by Henry Stephans in 1564. Unless the phrase of Aëtios (de febribus hecticis ac tabidis). "Eas vero quae in tabem procedunt, sanare impossibile est. Natura enim ipsarum calida est et sicca,"[3] may be considered to have reference to a constitutional condition, we have found no new facts pointing to this belief in their writings. Avicenna (eleventh century), in the third book of the Canon, deals with the point at some length. His account is transcribed in a footnote,[4] and appears to be little more than an amplification of the Hippocratic and Galenical tradition.

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[1] *See* Adams's translation of Hippocrates, p. 353, note 3.

[2] Book xxiv, cap. 6.

[3] "Tetrabiblon," II, i, cap. 92.

[4] Avicenna, Lib. III, Tract 4, cap. 20 (Alpagus's translation, edition 1557, p. 271): "Isti sunt alati habentes pectus strictum, habentes spatulas nudas carne et proprie posterius habentes spatulas declives ad anteriora eminentes et quasi unicuique eorum sunt duae alae et quasi spatulae ejus sunt abscissae ab adjutorio et ante et retro; et habentes colla longa et inclinantes et ad anteriora. Et quandoque praecedunt guttura ipsorum et eminent. Et in pectoribus quidem ipsorum, quae sequntur ea, multiplicantur ventositates et inflatio est in eis propter parvitatem pectorum eorum. Si autem cum hoc est debilitas cerebrorum recipientum superfluitates et cum hoc non digeruntur cibi, tunc jam complentur conditiones: et proprie si fuerint humores eorum acuti cholerici. Et formae quidem susceptibiles phthisis velociter cum alatione praedicta, sunt lenes absque pilis albae declinantes ad russedinem; et etiam corpora dura spissa propter illud quod accidit eis de ruptione venarum. Et complexio susceptibilis illius, est ejus cujus complexio est frigidior. Et aetas in qua multiplicatur phthisis est illa quae est inter octavum et decimum annum usque ad adventum (XXX) annorum. [N.B., the Hippocratic limits are 18—35 years.—W. B., M. G.] Et ipsa in regione frigida, est plus propter illud, quod accidit in ea de ruptione venarum et sputo sanguinis plurimo. Et tempus in quo multiplicatur illud, est autumnus."

From these varied quotations it will be seen that while a belief in the existence of a physical type or habitus specially prone to phthisis is to be found in the great medical classics it is certainly not clear that very great stress was laid on the matter. As we shall show, it is clear, however, that the idea that phthisis is an infective disease was not so entirely neglected by the ancients as has often been thought.

As a considerable proportion of the medical writings of the fifteenth and sixteenth centuries are purely derivative, we need not stay to analyse them. Coming to the seventeenth century, we may point out that Christopher Bennet, the first English writer on phthisis (1654), refers to the hereditary nature of the disease, which by the time we reach the eighteenth century had become a mere commonplace of the literature. This had also been emphasized by Lommius (1560) more than a hundred years before. While expatiating on the fact that phthisis is chronic and infallibly fatal, and incident to slender persons with narrow chests, long necks, prominent scapulæ, distorted legs, and to those persons subject to frequent defluxions, he adds, "idem malum magna ex parte a parentibus ad liberos transit; sic ut integras etiam familias veluti jure quodam hereditario oppugnet."

With the appearance of Richard Morton's "Phthisiologia" (1694) we have emphasis laid on what we should call the disposing causes, in addition to the actual constitutional factor in the disease. He says (p. 64): "The cause of a consumption of the lungs in general is a vitiated disposition of the mass of blood of the spirits in the nerves, contracted gradually from several procatartick or predisposing causes in which the sharp or malignant serum or water of the blood being separated by the soft or glandulous substance of the lungs does stuff, inflame, and at length also exulcerate the lungs themselves, which is the immediate cause of this distemper." The "procatartick" causes specially referred to are: (1) Stopping of usual and necessary evacuations, monthly courses, child-bed purgations, old sores, fistulas, issues, sweating in the soles of the feet; (2) passions of the mind—fear, grief, anger; (3) intemperance in eating and drinking; (4) want of exercise; (5) night studies and long watchings; (6) sleeping in the day, and sleeping much; (7) an ill air; (8) "an hereditary disposition from the parents does very often bring a consumption of the lungs when everybody knows very well that those who come of consumptive parents are apt to fall into the same distemper"; (9) an ill-formation of the breast, whether it be natural or accidental. "I call that natural when the breast is narrow, the neck long, the shoulder-blades stand out like wings, and I call accidental when there is crookedness or distortion of the breast, whereby not only the parts of the breast, and especially the lungs, being once weakened in their tone, are rendered subject to the flux of rheumes." (10) Infection; (11) chalky stones and such like in the lungs; (12) some preceding distemper. "The body being thus predisposed to a consumption by these and other such like procatartick causes, the more immediate cause of this distemper is for the most part the taking of *cold*, from whence it comes to pass that in a body disposed in such a manner by a load of humours or water continually derived from the distempered habit of the body into the lungs a cough is caused that is not easily shaken off."

These views of Morton were much in advance of his time, and slightly paraphrased, may be looked upon as the opinions largely held at the present day. They were accepted in the main as current teaching shortly after his time. The popular medical view is seen in the following passage from a not very profound observer, Salmon. In his "Practice of Physick" (1716) he says: "To these previous symptoms you may add an hereditary disposition which gives more certainty to all the former signs. For as usually one troubled with gout or stone, asthma, king's evil, mania, frenzy or epilepsy usually beget children affected with the same diseases, so likewise one troubled with a phthisis or a consumption may beget a child apt to have the same disease." Referring to the symptoms of phthisis, he adds that, "though authors and even daily experience have approved all these signs to be the characteristics of a confirmed phthisis, yet it is as true that they do not all happen to every patient, but some to one patient, and others of them to another, and that according as their strength, nature, constitution, and habit of the body will admit them."

It was during the first third of the eighteenth century that Pierre Desault (1675-1737), a distinguished physician of Bordeaux, published his important views on the relationship of tubercles to the production of phthisis. Basing his views on long-continued pathological anatomical observations, he was the first to assert that the causes of phthisis were "conjoint" and "antecedent," the conjoint cause being that "dont la présence fait la maladie et la destruction la guérit. Cette cause conjointe de la phthisie nous l'établissons dans des tubercules et concrétions qui se sont formez dans le substance du poulmon et qui sont répandus dans ses lobes. Ces concrétions ressemblent à des grains de grêle et sont de différentes grandeurs." Desault distinguished three types of persons becoming tuberculous from antecedent causes, viz.:

"(1) Ceux qui ont la poitrine serrée, le col long, les épaules relevées comme des aïles d'oiseau y sont les plus sujets; (2) ceux qui ont le malheur d'être nez de parens phthisiques y sont très-enclins, car cette maladie à cela de commun avec bien d'autres, qu'elle se traduit par héritage; (3) ceux qui sont engagez à vivre avec les phthisiques à les servir, peuvent prendre le mal, pour peu qu'ils y ayent de disposition; car il est contagieux, comme nous le serons voir dans la suite; ainsi les femmes qui servent leurs maris phthisiques le prennent souvent d'eux et les maris de leurs femmes."

The famous van Swieten, in his commentary on Boerhaave, apropos of the latter's aphorism regarding the gravity of hereditary phthisis gives the details of a family observed by himself. The mother came of a phthisical stock, and died of phthisis, together with the rest of her brothers and sisters, before the age of 30. The husband, a healthy man, lived to be over 80. Of the four children of the marriage which survived infancy three died of phthisis, while the fourth, "reliquorum sorte territus, tempestivis venae sectionibus funestam haemoptien cavit," a plan of operations which he seems to have carried out with such vigour as eventually to die not of phthisis but of its prophylaxis. The children of this man were healthy at the time of record.

These excerpts will show the current teaching on the nature and causes of phthisis towards the last third of the eighteenth century. But a new era was setting in. The foundations of the morbid anatomy of phthisis laid by Desault were sunk deeper by William Stark and Matthew Baillie in this country, and the recognition of the macroscopic "tubercle" as a constant factor in consumption was almost bound to alter the current of thought on the ætiology of the disease. This alteration came about particularly through the observations of G. L. Bayle, to whom we largely owe the idea of a tuberculous *diathesis*, an idea destined to play a dominant rôle in all discussions on this subject, down to the researches of Villemin and Koch. The evolution of the idea of a diathesis as opposed to mere disposition is to be found in Bayle's paper, "Remarques sur les tubercules" (1803), and was based on the observation of tubercle, not only in the lungs but in many other parts of the body, showing *apparently* that the condition is not only local but general. As this doctrine was new, and was destined to be the cause of heated conflict during the subsequent sixty years of medical research, we quote Bayle's exact words. On p. 28 of the above-mentioned memoir, under the title, "Considérations pathologiques relatives aux tubercules," he says: "Il n'est pas rare de rencontrer

chez le même sujet des tubercules très-nombreux dans divers organes; par exemple, dans les poumons et le mésentère, dans le mésentère et le foie; dans les poumons, les reins et la prostate; ou même dans la plupart des organes où nous les avons précedemment décrits. Cette simultanéité paraît indiquer que la nature de tous les tubercules est identique, et elle montre qu'il existe souvent dans l'économie une disposition particulière qui détermine leur formation. Peut-être conviendrait-il de désigner sous le nom de diathèse tuberculeuse, tendance à la production des tubercules."

The opening years of last century witnessed extraordinary developments with regard to the whole question of phthisis and the establishment of the doctrine of tubercle as the morbid substratum of diseases which had hitherto been classified from their semeiological resemblances only. This development we owe largely to the magnificent work of Laennec. He laid the basis of the doctrine of tubercle as a definite pathological change irrespective of its site; he distinguished the nodular and diffuse forms of the infiltration, and established the concept of a "tuberculous degeneration," or "tuberculous affection," which was the real basis of conditions which had hitherto been misunderstood. The whole problem was elaborated by Laennec into the comprehensive concept of "tuberculosis," the real essence of which was some change taking place within the body as the result of various predisposing factors, many of which he specified. A great many of the details of Laennec's teaching, especially as regards the evolution of tubercle, were the subject of heated discussion immediately after his publications. To this period belong the researches of Gendrin, Lobstein, Andral, and particularly Louis in France; Barron, Carswell, and James Clark in England; Schönlein and Canstatt in Germany; and Rokitansky in Austria. But the dominating and accepted view was that tubercle as a general pathological process is something inherent to the body and dependent on a diathesis, or constitution, which is lighted into activity by various predisposing causes. Thus Andral says that "there are certain habits of body, certain constitutions, innate or acquired, that predispose to the development of tubercle, that is to say, it most frequently appears in those constitutions, and it is a remarkable fact that it is in such cases especially that we observe the morbid production developing itself in several organs at once, both in the young and in the old" (p. 537). Further on he says: "I think I am borne out by facts in the two following propositions—(1) that the tubercular diathesis is in indirect proportion to the development of the constitution above described;

(2) that in proportion as this constitution becomes less strongly marked the tubercles also become less frequent and especially occur in fewer organs at once." The peculiar constitutions he refers to are met with in individuals whose skin is very fair, and in whom the prominences of the cheek exhibit a red tint as if it were laid on with a brush, which forms a remarkable contrast with the dead white of the rest of the cheek. Their muscles are soft and slender and show little contractile power, their blood is serous and deficient in fibrin and colouring matter, and among their secretions the mucous predominate. "It seems to me," he says, "that such persons preserve in adult age several of the characters belonging to the age of childhood, considered in its healthy or morbid state; their organization is in a manner arrested in its development."

That the constitution could be transmitted was held as a fixed belief. It was also held, however, that the disposition might remain latent. Thus the illustrious Thomas Young wrote in 1815: "There is a very general prejudice respecting the transmission of a tendency to scrofula which is often the source of much unnecessary uneasiness; it is supposed by many to constitute a marked and decided character, incapable of being increased or diminished, and which must in all probability produce at some time or other the most melancholy effects in the individual and in his descendants. In fact, however, no man was ever born incapable of becoming scrofulous, and in this sense every person may be said to possess more or less of a scrofulous taint, which may become mischievous or fatal to all, under improper management, but which in other circumstances may easily remain latent throughout life." We commend the calm good sense of this passage to those enthusiasts who teach that "soil" is everything, just as we advise the more numerous band of zealots who hold seed to be all important to peruse another passage from the same work, which we shall cite directly.

The diathetic doctrine of Bayle, which spread through the works of the early years of the nineteenth century, appeared on so firm a basis that even with the rise of exact morbid anatomy it still continued as a fundamental belief. Thus in Rokitansky we find the prevailing idea of a dyscrasia: "Das Exudat kann in einzelnen Fällen das lokale Product einer Entzündungsstase sein, gemeinhin ist diese aber selbst die Lokalisation der Tuberkelkrasis, und das tuberculöse Exudat somit grösstentheils das Product allgemeiner dyskrasischer Vorgänge." As manifestations of this dyscrasia, Rokitansky, on the basis of his enormous pathological experience, formulated his conception of the "phthisical habitus," viz., a heart too small for voluminous lungs in a long narrow thorax, a certain crasis of the blood favouring exudations, a delicate arterial wall, a weak muscular system, &c. In spite of the additional powers given to Virchow through the perfection of pathological histology, even he was dominated by and permeated with the idea of a tuberculous disposition or dyscrasia. In the second volume of his great work, "Die krankhafte Geschwülste," published so late as 1864, he still maintained that the cause of tubercle is to be sought in a general or local disposition leading to a vulnerability of the tissues. "Die Tuberculose," he says, "ist wesentlich eine Krankheit des extrauterinen Lebens und wenn sie hereditär sind, was nicht bezweifelt werden kann, so ist sie doch nicht congenital. Hereditär ist sie nicht als Krankheit sondern als Disposition. . . . Die Gewebe sind die Träger der Disposition, der hereditären Vulnerabilität."

It was shortly prior to the publication of Virchow's great work that Buhl in Munich had introduced into the whole subject a new conception in so far as he emphasized the constant association of miliary tuberculosis with a pre-existing caseous focus, and he correctly interpreted these two factors as those of effect and cause. According to him tubercle is a specific resorptive disease originating in the absorption of a special poison into the blood, and he considered that the inheritance of the disease consisted in the transference of the special poison to the germ cells. In 1865 came the epoch-making communication of Villemin, in which he was able to assert that tubercle is a specific infective disease with its cause residing in an inoculable agent. His subsequent observations were collected in his great work, "Études sur la tuberculose," published in 1868. In spite of his demonstration that tuberculosis is due to a specific inoculable virus, he was not carried away with the idea that the mere presence of this virus explains all the phenomena of the disease, for he particularly emphasizes the fact that in the prophylaxis of tuberculosis two points are of cardinal importance, viz., the knowledge of the existence and condensation of the morbid ferment, and "l'aptitude du milieu organique." "L'hygiéniste," he says, in conclusion, "ne doit pas perdre de vue les principes qui reposent sur ces deux ordres de faits." In his last important paper, "De la propagation de la phthisie" (1869), he sums up in masterly style his views on the dissemination of the disease, views which it may be said have originated the whole trend of medical opinion since his time. By series of experiments he demonstrated anew the infectivity of phthisical sputum by inoculation, inhalation, or ingestion; pointing out the danger of sputum getting into the air as dust. He recognized the fact that expired air as such does not seem more capable of transmitting the disease than it is of conveying glanders. The virulent principles of both diseases do not seem volatile enough to be conveyed by air. Phthisis does not seem to spread among the attendants in consumptive hospitals in London because the expectorated matters are received into places where they can decompose. The most favourable conditions for contagion are to be found in the dwellings of the poor and in common habitations, such as barracks, convents, prisons, &c. He concludes that the prophylaxis of phthisis may be summed up by avoiding the morbific agent and augmenting the resistance of the organism. In reading the work of Villemin one cannot but be struck by the temperate view he takes even in the presence of the momentous discovery he had made that tubercle is an infective disease, for by demonstrating that its cause is essentially external he had destroyed for ever the belief that tubercle owes its origin to some internal transformation of the tissues excited by an accidental or occasional cause. Even in the light of the discovery of a specific virus he realized the importance of the terrain or soil on which this virus is to flourish.

The fifteen years following Villemin's publication may be described as the "Sturm und Drang" period in the research on tubercle. Its culminating point was Koch's famous paper, "Ueber Tuberculose," delivered before the Berlin Physiological Society on March 24, 1882.[1] In it we have the isolation of the actual virus of tuberculosis and the demonstration of the unity of tubercle as a pathological process—a complete and irrefutable confirmation of the doctrines of Laennec and Villemin. In his completed paper ("Die Aetiologie der Tuberculose," 1884) Koch also refers to the hereditary predisposition to tuberculosis in a particularly temperate manner. "Against the view," he says, "of the unity of all forms of disease caused by tubercle bacilli is the material difference in the course taken by the disease in different individuals of the same race, and in their susceptibility to tuberculous infection. These are, however, phenomena which appear in a more or less marked manner with all infective diseases. It is usual to get out of the difficulty by supposing that there is a difference in the predisposition to the disease both as regards taking it in the first place and the greater or less severity of the course it runs, but this specifies the fact rather than explains it. Some of the dissimilarities in the pictures presented by cases of tuberculosis may be ascribed to differences in the seat of infection. Then, too, the quantity of infective material coming into operation in the first place seems to be of essential importance. . . . . .

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[1] *Arch. f. Anat. u. Physiol.* (Physiol. Abth.), Leipz., 1882, p. 190.

But although a good many of the phenomena classed together under the head of 'predisposition' may be referred to simple and easily explained conditions, some facts still remain difficult or impossible to interpret, compelling us for the present to accept the view of a varying liability. Most important of all is the striking difference between the course of tuberculosis in children and in adults, and again the undeniable predisposition to tuberculosis which exists in some families. In the latter instance many of the cases of illness, where predisposition is supposed to be an important factor, might rather be ascribed to increased opportunities of infection, and there are also peculiar predisposing influences connected with the family constitution to be thought of, such as a tendency to catarrhal affections of the respiratory organs and imperfect development of the thorax. Still many carefully observed cases remain which cannot be explained in this manner." Koch also emphasized the fact that the disposition to the disease must be variable as regards time, for a tubercle might attain a considerable size and then contract and cicatrize. Such temporal changes show that there is nothing against the view that conditions favourable or unfavourable to the tubercle bacilli are present in some human beings not only temporarily but for the whole lifetime. With regard to the question of inheritance in tuberculosis, he expressed himself quite definitely in the statement that it "is explained most naturally by supposing that the infective germ itself is not inherited but rather certain peculiarities favourable to the development of germs which may later on come into contact with the body—in fact, it is the predisposition to tuberculosis which is inherited."

In spite of the temperate views expressed both by Villemin and Koch, the founders of the doctrine of infectivity of tuberculosis, many of their successors at once adopted much more advanced views, in which an overwhelming importance was ascribed to the infective agent. We have, however, seen the belief in a special susceptibility of persons conforming to a particular physical type recurring at intervals from the time of Hippocrates to the end of the nineteenth century. Great and often untrustworthy as may be the influence of dogma and tradition, it is surely unscientific to pretend that this belief is without any other warrant than mere authority. Evidently, if it is once admitted that a phthisical type exists, then it is clear that the question of an inherited disposition becomes of more than academic interest, since few will deny that the physical characters of mankind are heritable in a high degree. In our opinion there is no warrant for calmly ignoring, as has been done, the evidence we have cited.

CONTAGION.

It may be a fact that in pre-bacteriological days too little stress was laid upon the communicability of phthisis, but it is a vulgar error to suppose that the matter was never discussed. Aristotle, in the "Problems,"[1] inquires why consumption, but not dropsy, can be communicated to persons near at hand. "A consumption infects because it renders the breath bad and heavy. And those diseases are caught the most rapidly of all others which arise from putrid breath—such, for instance, as are pestilential diseases. But he who approaches such disorders as these draws to himself a breath of this kind. He becomes diseased, therefore, because such breath is noxious. But he becomes diseased through one person because this one person alone has a putrid breath and others have a different breath. And he is infected with the same disease because the breath of a consumptive person is vitiated through the consumption alone." Francis Adams, the learned translator of Hippocrates, adduces reasons for holding that Isocrates thought consumption to be contagious. Galen says explicitly that "it is dangerous to pass the whole day with the consumptive, and in general with all those persons whose diseases generate putrid effusions, so as to make the house in which they live offensive." Avicenna also considered consumption to be contagious.

Coming to modern times, Lommius (1560) has the following striking passage: "Jam etiam sputum mistum purulentum, idemque fœtidum quod halitus etiam contagione imprudentes alios labefactat, assiduaque febris, quae opportuna cibo tempora eripit et multain sitim movet, in tenui corpore certum esse periculum ostendunt." Morton (1694) was convinced that consumption is propagated by infection: "For this distemper (as I have observed by frequent experience), like a contagious fever, does infect those that lye with the sick person with a certain taint." He refers to the case of a phthisical youth who appeared to have infected both his sister and a nurse with the disease. We have already quoted similar views as expressed by Desault. The question was also discussed by Frederick Hoffmann (1740), who cites other observations by Riverius and Schenck. In his twenty-second letter, treating of purulent and ill-conditioned empyema and phthisis, Morgagni (1765) says that Valsalva, when a young man, having been in danger of a consumption, as is written in his life, was rather cautious of dissecting bodies that had been wasted away by disorders of this kind, and Morgagni adds: "Ego vero ut me tibi aperiam illa fugi de industria adolescens et fugio vel senex, tunc ut mihi nunc ut studiosae, quae me circumstat juventuti prospiciam, cautius fortasse quam opus sit; at tutius. Itaque non multa ille, ego vix aliquod dissecui." Van Swieten, in his commentaries on Boerhaave's aphorisms, refers to a youth who spit up such fœtid matter in the morning that he (Van Swieten) (who was "not so nice as to be easily affected with this kind of thing") was scarce able to endure the stench. "Certe juvenis ille cujusmodo mentionem feci, infecit sororem et ancillam quae ipsae in morbo assidue ministraverant." The value of Van Swieten's testimony is perhaps discounted by the somewhat improbable story he relates, according to which a man's wife expiring of a consumption, giving him a farewell kiss, all that part of his chin which her lips had touched remained ever after smooth, though the beard grew thick all round ("postea ille locus glaber mansit, licet densa barba cresceret in omni ambitu"). Cullen (1796), while not denying the possibility of contagion, had never, among many hundreds of instances of the disease, seen one which to him could appear to have arisen from contagion. Heberden (1803), however, a contemporary of Cullen, had seen "several die of consumption in whom infection seemed to be the most probable origin of their illness, from their having been constant companions or bedfellows of consumptive persons."

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[1] Taylor's translation, vii, p. 8.

In Italy in particular the contagious origin of phthisis was widely but not universally accepted. Thomas Young, for example, cites the writings of Borsieri (1782), who remarked that the College of Physicians of Tuscany had decided against the contagious theory, but had not succeeded in convincing the authorities of the uselessness of precautions. Boiseau, in his learned paper, also cites Cocchi (1761) as an anticontagionist. Young's own views on the subject appear to us worthy of reproduction in full. He says:—

> "It has been much disputed whether or no consumption is ever capable of being communicated by contagion, and it must be allowed that it would be extremely difficult to produce any strong demonstrative evidence of the fact. It may often have been observed that the husband or wife of a consumptive person has died consumptive within a few months after that person's death; but it will appear upon calculation, that this circumstance must happen in London more than once in about two months, before we can deduce from it any proof of the probable connexion of the two events. For one, at least, of 50 adults may be expected to die annually, and consequently one of 200 by consumption, or one out of 400 in six months; and if we estimate the number of married persons dying annually of consumption at 2,000, we may expect to find five instances of apparent contagion without any real foundation. The evidence which has been adduced to prove the infectious nature of consumption has

been by no means of this precise nature, but it has consisted in great measure of absurd and almost superstitious accounts of the propagation of the disease by articles of dress and furniture. At the same time, there has been a very general impression among medical men of the best judgment, with the best opportunities for observation, that they have met with unequivocal instances of the communication of the disease, especially between husbands and wives, and in families partaking of the same hereditary constitution; and Dr. Rush has given an account of a consumption manifestly contagious, which spread from the family of the proprietors of an estate among the negroes, who were neither related to the first victims, nor had been subjected to fatigue or anxiety on their account. I have myself known an instance of a carpenter who died of a laryngeal consumption, and whose wife died soon after, as I was informed, of a disease precisely similar, and I should think it unjustifiable, from a full confidence in the absolute impossibility of infection, unnecessarily to expose any person who appeared to have the slightest predisposition to the disease to any intimate communication with a consumptive patient—as, for instance, to sleeping in the same bed or living constantly in the same room, which is nearly the extent of the apprehension expressed by Galen himself, who observes that it is dangerous to pass the whole day with consumptive persons." (Young, pp. 46, 48.)

Had this passage been written in 1910 instead of 1815, we are not acquainted with any evidence which would disentitle it to be regarded as eminently worthy of the author's scientific reputation.

Laennec, while referring to the fact that consumption had long passed for a contagious disease "by the people, by magistrates, and some medical men, especially in the southern parts of Europe," did not think that it appeared to be contagious in France. "We frequently observe," he continues, "among the poorer classes, a numerous family sleeping in the same apartment with a consumptive patient, and a husband occupying to the last the same bed with his wife without any communication of the disease. The woollen apparel and the beds of consumptives which it is the custom to burn in certain countries are not generally washed, and yet I have never seen the disease communicated by them. It would be well, nevertheless, were it merely on the score of prudence and cleanliness that greater precautions were taken in this respect. It is well ascertained that a disease not usually contagious may become so in certain circumstances." Forbes, whose translation of Laennec we have just quoted, adds in a footnote that the opinion of the great majority of medical men in this country is opposed to contagion, an opinion which he considers is justified equally by statistical facts, by the truths of pathology, and by analogical reasoning. Forbes himself had never witnessed among the thousands of cases he had attended one unequivocal instance of transmission by infection. Sir James Clark, one of the most careful observers who have written on phthisis, was of the same opinion. That the tuberculous constitution was transmitted, however, he considered to be one of the best established points in the ætiology of the disease. Louis, likewise, whose work is a mine in which most subsequent writers have quarried, was no supporter of the doctrine of contagion. Sir Thomas Watson, in his well-known treatise, discusses the question of the contagiosity of phthisis in his usual temperate manner, and concludes that the "disorder does not spread by contagion."

We have referred above to the epoch-making work of Villemin, who established beyond doubt the existence of contagion as a factor in tuberculosis. About the same time William Budd arrived at a similar conclusion by a different process of analysis. He noted the importance of the geographical distribution of consumption in past and present times, and especially its great fatality now in countries which when first discovered by Europeans were entirely free from it. He also drew attention to the prevalence of tuberculosis in convents, harems, barracks, and penitentiaries, where the inmates live in relatively close contact with each other. It may be pointed out, however, that the exact data on which Budd based his assertions are not given in detail in his paper. Shortly after the publication of the writings of Budd and Villemin reports began to appear which seemed to favour more and more the idea that phthisis is communicable from one person to another (Bergeret, Wernich). It was not that these observations were of a kind new to medical literature, but that the interpretation of the findings was different. Immediately after Koch's discovery the question created enormous interest. It was one of the principal themes for discussion by Rühle and Lichtheim at the second Congress for Internal Medicine in Wiesbaden in 1883. Both these authors considered that, although contagion was not doubted, the question of a disposition was of the greatest importance.

One of the most ardent supporters of the contagion doctrine was Cohnheim, and it is probably largely through his dominant influence that the idea of a disposition came to be suddenly discredited. "The assumption," he says, "of an inherited predisposition to this disease is not, in my opinion, necessary to explain its subsequent development in the children of tuberculous parents. For the fact that several members of a family become tuberculous proves nothing but the presence in it of conditions calculated to give rise to tuberculosis; and what condition could be more favourable to its production than the presence of a phthisical patient in the family? When a child nourished by its tuberculous mother becomes tuberculous it is less far-fetched to assume the communication of the disease by the mother's milk into which the virus may pass than the inheritance of a diseased constitution. . . . In short, where the opportunities for acquiring tuberculosis are so manifold we must be furnished with proofs of at least a different character to those already brought forward before we can accept as demonstrated the existence of an hereditary predisposition to the disease" (p. 735). As late as 1899 a similar extreme position was taken up by Cornet in his exhaustive work in "Nothnagel's specielle Pathologie und Therapie." He dismisses the question of hereditary disposition in a few pages, finishing up with the following conclusion, printed in extra heavy type: "Ein maassgebender Einfluss auf die Entwickelung und Verbreitung der Tuberculose kommt auch der hereditären Disposition nicht zu." Apart from a hereditary disposition he considers that *dis*position is often really *ex*position, and that is the main line of his argument.

It may be pointed out, however, that in the period immediately following Koch's discovery great activity was manifested in civilized countries to settle the question of the contagiosity of phthisis. Collective investigations, so-called, were instituted—e.g., in America, Germany, France, Italy, and England. The results of the opinions of some 50,000 doctors who participated in this gigantic undertaking have been described by Cornet as "mehr als bescheiden." We may particularly refer to the "Collective Investigation Record, edited for the Collective Investigation Committee of the British Medical Association by Professor Humphry and F. A. Mahomed" (1883). In spite of the fact that the Association was urged to action by preliminary exhortations of coryphæi like Sir William Gull and Sir James Paget, the net result of the endeavours of British physicians was indeed modest. Out of many thousands who were circularized 1,078 made returns. A great many (673) of these returns were simply negatives containing the word "no," meaning that no case of contagion had been observed by the member making the return. These returns, admitting of no further analysis, were set aside. The remaining 405 observers were classified as follows, viz.: (1) 261 with affirmative answers, (2) 39 doubtful, and (3) 105 negative. In Class 1 there were several groups, as follows:—

(A) Return of cases observed between husband and wife only	158
(B) Return of cases observed between members of the same family: between parents and children, brothers and sisters, as well as between husband and wife	81
(C) Return of cases observed between persons unrelated as well as between persons of the same family	13
(D) Returns containing cases between unrelated persons only	8

A study of the details of many of the returns shows that they cannot withstand criticism. Thus one of the eight cases alleged to have occurred between unrelated persons was the following: Observation 260.—"A female, aged 24, mother and father both alive and healthy, *but all the maternal aunts died of phthisis* (*!*) Before her confinement she had slept constantly with a person, no relation, who had phthisis. She was before this free from all symptoms of the disease."

Among the negative returns we may direct special attention to Observation 392, which the key informs us was that of Sir Clifford Allbutt. It runs: "Although my attention has been given for many years to this question, I never have observed a case of even probable transmission of phthisis from one person to another."

Observation 311 was that of Sir William Church: "Like everyone else I have seen cases of two, three, or more of a family, generally sisters, suffering one after the other, but that does not enable me to answer the question in the affirmative."

The general result of collective investigation records in this country and elsewhere would seem to show that the value of the contagion factor in phthisis is not so easily determined as many have supposed.

We may conclude this section with some reference to the views of August Hirsch (1886), than whom no one possessed a wider range of knowledge or could draw his deductions in a more logical manner. He discussed the bearing of the infective theory of the origin of phthisis upon actual facts that had been ascertained by historico-geographical and statistical research, and concluded that "the contagious transmission of phthisis plays but a subordinate part in the spread of the malady, and it will be all the harder to exclude that conviction if we bear in mind that many parts of the world which offer in respect to sanitation favourable conditions for the occurrence of the disease have little or none of it, although there has been no lack of imported cases. All this seems to show that the transmission from person to person takes place only under certain conditions, and I hold it to be at least doubtful whether these conditions are created simply by individuals living together in close intercourse. . . . But no one need hesitate to affirm that the decisive factor in the genesis of the disease has to be looked for in the *disposition* of the individual to fall into the same." Our quotation from Hirsch brings us to a period now a quarter of a century remote, and it will be our endeavour in the next section to determine what advances have been made in this period, and what is the trend of opinion to-day on the doctrine of Disposition as a factor in phthisis.

The chief branches of inquiry through which it has been sought to advance the problems of infection and disposition to pulmonary tuberculosis have been (1) bacteriological and pathologico-anatomical; (2) epidemiological and statistical; (3) clinical.

With regard to the first of these a large amount of evidence has accumulated to show that the susceptibility of animals to a particular infective agent varies with the animal, and even among species of the same animal. The gradual deepening of the whole problem of immunity compels one to assume that the disposition is a variable factor, and in a large number of instances the study of acquired immunity has served to bridge the chasm between disposition and infection. In the case of tuberculosis in particular, however, unexpected difficulties have been encountered, in so far that it is not at all clear in what manner immunity is produced against the tubercle bacillus.

The pathological anatomists have made numerous contributions of first-rate importance. Among them is the establishment of the fact that tuberculous lesions are found in an enormous number of the autopsies made on adults. The very careful series of cases studied by Naegeli, and in particular by Burkhardt, show that over 90 per cent. of adults have tuberculous lesions. If the tubercle bacillus were ubiquitous, and we could assume that the opportunity of infection were equal for everyone, the occurrence of such an enormous amount of tubercle, compared with the fact that only, say, one-seventh of the human race die of tubercle, would show that the remaining six-sevenths possessed a degree of immunity which enabled them to overcome an infection which had already taken place. The problem is, however, by no means so simple, inasmuch as it remains doubtful to what extent the figures of Naegeli and Burkhardt are applicable to the population in general. In both these series the material was obtained in hospitals (Zürich and Dresden), and thus was selected. Further, as Flügge has correctly pointed out, the death-rate refers to all cases of death, children included, and does not refer to certain ages only. Calculating out Burkhardt's figures as percentage of healed and progressive tuberculosis to total deaths, Flügge makes out that the number of those "disposed" between the ages of 18—60 must approximate to 60—70 per cent., the number of non-disposed sinking to 30—40 per cent., a very different figure from that obtained by a superficial examination of Burkhardt's figures. Flügge points out further that, far from the tubercle bacillus being ubiquitous, it is in reality local, and thus the opportunity for infection must vary within wide limits. That a considerable number of human beings are tuberculous, however, is also shown by the inoculation of tuberculin carried out by Franz on the soldiers of two Austrian regiments. Over 60 per cent. gave a positive reaction.

The study of pathological anatomy in the last twenty years has gone to show more and more that the doctrine of a phthisical constitution is on a sound basis. It is impossible to analyse the publications dealing with this subject. Indeed, this is all the more unnecessary as it has been very completely done by Hart and Schlüter. This view is also supported by the work of Beneke, Birch-Hirschfeld, Schmorl, Bartel, and many others. The main problems studied have been in connexion with attempts to determine why the lungs, and especially the apices of these organs, are much more frequently affected than other parts of the body. Thus some have tried to uphold the doctrine of a general pulmonary disposition, others that there is an individual susceptibility of the apices on the basis of a general constitution. There certainly seems good ground for assuming that anomalies, congenital or acquired, of the upper thoracic aperture are responsible for frequent infections in this site.

Coming to work dealing more precisely with epidemiological and statistical inquiry, a large number of publications have made and are making their appearance. To notice these in detail would require more space than remains at our disposal; we shall, therefore, confine ourselves to a brief general review indicating what, in our judgment, are the salient features of different types of work. We can at once put on one side the very numerous lectures, addresses, and pamphlets which merely repeat evidence already discussed in earlier sections of this paper, or affirm conclusions (of most diverse natures) on the strength of general medical opinion, "universal experience," or the writer's (unpublished) observations. Such a process considerably reduces the amount of material to be examined. The residuum can be divided roughly into two classes: (1) Papers adducing evidence to prove that the environmental factor is the all-important one; (2) papers leading to a precisely contrary conclusion.

Work falling in Class 1 can again be divided in accordance with the method adopted into direct and indirect investigations. By direct investigations we understand papers in which facts are collected which are thought to demonstrate at once the all-sufficiency of the environmental factor. Most of these papers contain details of more or less extensive series of cases in which, according to the author, the hereditary factor seemed to be of little moment, or the existence of the disease in several members of a family could be referred to some common source of infection. A relatively favourable instance of this class of paper is afforded by a contribution published by v. Sokolowski in 1901. This author collected data respecting no less than 32,694 patients seen by him in private practice, including 8,074 tuberculous subjects. While recognizing the existence of an hereditary influence in certain cases, he was of opinion that its frequency was not so great as "die Aerzte es gewöhnlich annehmen." v. Sokolowski emphasized the magnitude of his series and called attention to the danger of basing conclusions upon but few instances.

In our opinion, however, v. Sokolowski's data are so presented that no conclusion whatever can be drawn from them with regard to the question of heredity. The sibships of the different individuals are not stated, nor the age distribution of the non-phthisical patients, so that it is impossible to say how many of them can reasonably be regarded as still "exposed to risk." The author's conclusion is, indeed, based upon a fallacy very common in medico-statistical writings. He seems to think that a knowledge of the frequency of tuberculosis among the ascendants of phthisical persons is a sufficient basis for deductions. It should be clear that to base a conclusion upon the single fact that in his series not more than 25 per cent. of the parents of tuberculous patients themselves suffered from the disease would be illogical, even if all the family histories were completed. The percentage of tuberculous ascendants in the stocks of non-phthisical patients was an essential preliminary, and this does not appear to have been ascertained; in fact, there is no evidence of a control experiment having been done. It is of interest to notice that the intermarital correlation, the only constant for the deduction of which v. Sokolowski provides sufficient data, is about 0·2 (Pearson and Pope, p. 8), a value indicating but little relationship.

We have dwelt upon v. Sokolowski's paper because it is a relatively favourable specimen of its class, the deductions not being of a very sweeping character. One cannot, however, fail to remark traces of a defective logical method which seems to us to vitiate so many similar results. In this connexion we may refer to the severe and, in our opinion, unanswerable strictures passed by Hamer upon the statistical evidence adduced by Cornet with regard to the Catholic Nursing Orders in Germany (Bulstrode).

It is not necessary to criticize a type of paper represented by the work of a German writer in which *three* cases of phthisis are described in two brothers and a sister. The author suggests that all may have been originally infected by the father, the disease being lighted up by sexual or other excesses, since the daughter had recently married, while of the two brothers, one, in particular, had exceeded "in Baccho et Venere."

Many writers have attempted to throw light upon the question by a study of the incidence of phthisis upon husbands and wives. As this subject was exhaustively discussed by Pearson and Pope in 1908, and was also the subject of a paper read by one of us at a meeting of this Section in 1909, we need not refer to the matter in detail. The conclusions expressed by the authors named, that interconjugal infection is a factor of but secondary importance, has not, we think, been seriously disputed; criticism having been mainly directed to the alleged imperfections of the data analysed.

On the whole, we venture to express the belief that no papers falling under the present category have materially added to the knowledge of thirty years ago, which has been detailed in earlier pages of this memoir. We accordingly turn to writers who have adopted a different and less direct process of inquiry.

During the last five-and-twenty years the tuberculosis-rate has markedly fallen in almost every, if not every, land. Within the same period a belief in the communicability of phthisis has been more and more disseminated. Much effort has been devoted to the diminution of possible sources of infection, attention has been directed to the necessity of disinfecting houses or rooms occupied by tuberculous patients, a war has been waged against the disgusting habit of indiscriminate expectoration, increasing numbers of phthisical patients have been segregrated in public institutions, and, generally, medical men and public health officials, both lay and professional, have laboured unceasingly to conquer the disease. It would naturally be expected that the fall in the phthisis-rate would be associated psychologically at least in many minds with the multifarious activities displayed. Such has been the case, and a considerable literature, both lay and professional, has sprung into existence. The fall in the tuberculosis-rate has, however, been differently interpreted by various authors. Some have laid stress upon the improved housing of the working classes, others have attached most importance to methods of disinfection, others to the influence of sanatoria and segregation. Newsholme, as the result of an elaborate inquiry, seemed to conclude that institutional segregation had been most powerful of the different factors, without, of course, neglecting other possible agents. Bulstrode and others have called attention to the difficulties of the problem, and among recent studies we would specially mention the dispassionate inquiry conducted by Professor Matthew Hay, of Aberdeen. Hay points out that the phthisis-rate was falling before any very serious measures had been collectively adopted to combat the disease. He remarks :—

> The decline began long before the germ origin of tuberculosis had been demonstrated by Koch, although the infectious character of the disease had never been without believers; but, what is more remarkable, the rate of decline was almost as great before any administrative attention was being paid to its infective character as it has been after such attention began. (Hay, p. 68.)

These facts give one pause and inevitably call to mind the fascinating subject of the rise, decline and fall of disease prevalence in instances not attributable to human agency. The epidemiological history of plague in England is a case in point.

We may further remark that the existence of a high correlation between the extent of, for example, institutional segregation and a fall in the phthisis-rate does not appear to us a fact of necessarily great significance. The difficulty that perpetually confronts the analyst of such data is caused by the existence of multiple correlation. The association between A and B may be indirect and dependent upon a common association with a third variable C. The theory of multiple correlation and its practical applicability have been so much extended by the labours of modern statisticians that at no distant time we may reasonably look for more light. It must, however, be remembered, that when more than a few variables are involved—and in the problem in question the number may not be small—arithmetical work becomes very laborious. In any case, whatever the future may have in store, no statistical proof of the all-sufficiency of any one of the possible factors or even of the predominating importance of any one, has yet, we think, been adduced. Some workers have realized the statistical difficulties of the problem. Speaking of infection in phthisis, Newsholme writes :—

> The evidence convincing one as to this does not lend itself to statistical statement, and when stated in print it may not be convincing : but the steady succession of cases in infected households at intervals of one or two years, or longer, the intercurrent destruction of children by tubercular meningitis or

"broncho-pneumonia" while their parents are suffering from chronic phthisis, and other similar evidence, can leave no doubt in the minds of those who come into continuous contact with the actual facts. (Newsholme, p. 70.)

With the addition of the words, "and are a priori convinced of the non-existence of a constitutional liability," this extract would not inaptly express the judgment we ourselves have been led to form. In sum, while the indirect method, the study of vital and sanatory statistics, has undoubtedly revealed the existence of phenomena well deserving further inquiry, it has not, we believe, substantially widened our knowledge of the particular problem which engages our attention.

Finally, we must examine recent contributions to the study of constitutional and hereditary factors. Here, again, a classification into direct and indirect methods can be made, but the number of papers falling within the latter sub-class is not great. Many, indeed most, of the writers who have emphasized the hereditary factor in phthisis seem to us to exhibit the same logical insufficiency which appeared to characterize writings of an opposed school, but a few papers are deserving of a fuller notice.

An attempt has been made, notably by Turban and Strandgaard, to obtain direct evidence of the hereditary transmission of a phthisical *habitus*. Turban published a series of cases in which members of the same family affected by phthisis exhibited a considerable amount of agreement in respect of the age at which the disease first appeared, the site of the lesion and the general course of the disease. From such data he argued that the members of a common stock resemble one another in the possession of a *locus minoris resistentiæ*, such peculiarity being the basis of an hereditary character in the disease. Since Turban did not publish, together with his family records, any data from which the relative frequency of primary disease in different lungs or regions of one lung could be deduced, it was difficult to assent to his conclusion. Quite recently Strandgaard has published another series of family observations, and at the same time provided somewhat extensive material from which the relative frequency of right pulmonary phthisis, left pulmonary disease, and their combinations in each stage (adopting Turban's classification into three stadia) could be calculated. Strandgaard, himself arrived at substantially the same conclusion as Turban with regard to the main issue of an inherited *locus minoris resistentiæ*. If we classify pulmonary lesions into (1) mainly right lung affections, (2) mainly left lung affections, (3) cases in which both lungs are equally involved, then in a pair of phthisical patients taken at random six combinations are possible—viz., RR, LL, LR, LB, RB, BB, where R signifies class (1), L class (2) and B class (3). From a knowledge of the relative frequencies of the three classes in the general "population" of phthisical patients, it is evidently possible to calculate the probable frequency of each of the six combinations in a sample of pairs which might be expected to occur as a matter of chance. Evidently the same method can be extended to groups of three, four, &c. We found as a matter of fact, on applying this arithmetical test to pairs of brethren at the same stage of the disease, that the frequency of the combination RL was decidedly less than the calculated value, so far bearing out Strandgaard's interpretation of his material. The actual frequencies were, however, based on too small numbers for the conclusion to have much importance attached to it.

Time has not permitted of fully analysing the material, but our general impression is that a complete study is fraught with grave difficulties, and that this class of inquiry, although of interest, is not particularly hopeful. A judgment as to the exact localization and extent of the pulmonary lesion is based, in the large majority of instances, upon the results of physical examination. Not being clinical workers, we express an opinion on the value of the methods of diagnosis in current use with great diffidence; we think, however, that we are but echoing the opinions of highly skilled practitioners when we suggest that, however expert the observer, such results are subject to much error. That final court of appeal, the post-mortem table, has not always confirmed the decision of the clinical judge as to the extent or even the site of the pulmonary lesion. Apart from this, an examination of Strandgaard's paper will show that it is, in a fair percentage of the cases, difficult to decide which lung is chiefly involved, so that two independent classifications of the same material might lead to appreciably different results.

The English Biometric school have devoted much attention to the hereditary factor in phthisis. In 1908, Pearson analysed material from the Crossley Sanatorium; he concluded that the intensity of the relationship between the present or past existence of tuberculosis in the parent, and the same disease in the offspring, was of such an order that hereditary transmission of a tendency to develop the complaint must be assumed to exist. The values of the coefficients of correlation led Pearson to conclude that the inheritance of a phthisical predisposition, or, as he termed it, diathesis, was of the same order of intensity as had been found to characterize the transmission of normal physical characters in man. Pearson further expressed the opinion that this factor was of more importance in the genesis of the disease than ordinarily infective conditions (Pearson, p. 25). Similar conclusions were reached by Goring, who analysed statistics relating to 1,500 criminals.

These results have been subjected to considerable criticism, it having been urged in the first place that the correlation between parent and child might be a measure rather of infection than of true inheritance; in the second place, it was pointed out that data of this kind cannot conform to a high standard of accuracy. In a subsequent paper, Pearson argued that, were infection the chief factor, the correlation between husband and wife should be of the same order as between parent and child, but that it was in fact much smaller.

It appears to us that the second objection, viz., the alleged untrustworthiness of the statistics, has been to some extent met by the uniformly higher values of the ancestral correlations as compared with the inter-marital constants obtained from material of essentially the same class. It is difficult to understand how errors of record, apart from widespread and intelligent falsification, could lead to such a result. This impression is confirmed by the concordance of Goring's conclusions with those of Pearson, since the former's material was not subject to precisely the same kind of error as that analysed by Pearson, Pope, and Greenwood.

We should not, however, be prepared to regard a study of marital tuberculosis as fulfilling all the conditions of a true control, from the point of view of infective possibilities, since resistance to infection *may* increase in adult life,[1] although we are not acquainted with any convincing evidence in man that this is so. It will perhaps be thought that, while biometric studies have somewhat strengthened the evidence in favour of a belief in the importance of an hereditary factor, they have not materially altered the position of thirty-five years ago. These remarks must not be interpreted as implying any disparagement of the care and precision which distinguish the papers under notice.

Lastly, a word may be said as to indirect methods of approaching the subject. If it could be shown from a statistically adequate sample of persons suffering from tuberculosis that they differ significantly in such anthropological characters as skin and eye pigmentation, craniological constants, &c., from the population out of which they come, it would be fair to argue that the factor of inherited predisposition is of importance, since few will now deny the heritability of many such characters. Of work dealing with this aspect, the papers of Shrubsall may be mentioned. This author has found some association between brunette traits and pulmonary tuberculosis. It can, of course, be urged that work of this kind is difficult to interpret, since even were some such association definitely established, it might be indirect. It might be, for instance, that a certain type of physique is ill fitted to survive in the struggle for life, and that its possessor tends to be driven to those conditions of environment under which the chances of infection are maximal. Obviously, however, such a criticism is double edged. These anthropometric inquiries undoubtedly deserve further extension.

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[1] *See* Greenwood, pp. 7-10.

The reader will have gathered from what has gone before that, in our opinion, the many workers who have dealt with the problem of inheritance and infection during the last few years have failed to achieve any really solid advance from the classical position. Such a conclusion will, of course, have been anticipated by anyone who has contemplated the development of a science, noting the rapid progress up to a certain point, followed by advances each so small as to be inappreciable. Nevertheless, an attentive consideration of the facts relative to our knowledge of tuberculous disease, some of which we have endeavoured to review, would be of much benefit to those persons who say or imply that what has been done in our own time far exceeds in importance the combined efforts of all previous generations. Any such assertion is repugnant to sound scholarship, calculated gravely to mislead the general public, and an injury to the cause of medical science.

More than twenty-eight years ago Leichtenstern made the following remark in the first blush of enthusiasm following the isolation of the tubercle bacillus, and it remains substantially true to-day: "For all Koch's brilliant discovery there are still many lacunæ and open questions facing us in the ætiology of consumption, and these offer to the practitioner, to the statistician, to the pathologist who studies the history and geography, to the pathologist who experiments, and to the bacteriologist, a wide field wherein to co-operate."

BIBLIOGRAPHY.

ANDRAL, A. "A Treatise on Pathological Anatomy," translated by Townsend and West, Lond., 1829, i, p. 535.

AVICENNA. "Medicorum Arabum principis. Liber Canonis, etc., Venetiis, apud Juntas, 1557."

BARTEL, JULIUS. "Probleme der Tuberkulosefrage," Leipz. und Wien, 1909.

BAUMGARTEN, P. "Die Lehre von den Krankheitsanlagen" in Krehl und Marchand's "Handbuch der allgemeine Pathologie," Leipz., 1908, i, p. 387.

BAYLE, G. L. "Remarques sur les tubercules," *Journ. de méd., chir., pharm.*, &c., Par., An. XI (1803), vi, p. 3.

Idem. "Sur la dégénérescence tuberculose non enkystée du tissu des organes," *ibid.*, Par., An. XIII (1805), ix, p. 427, and x, p. 32.

BENEKE, F. W. "Constitution und constitutionelles Kranksein des Menschen," Marburg, 1881.

BENNETT, CHRISTOPHER. "Theatri tabidorum vestibulum," London, 1654.

BERGERET, L. E. F. "La phthisie pulmonaire dans les petites localités," *Ann. d'Hyg.*, Par., 1867, 2 sér., xxviii, p. 312.

BIRCH-HIRSCHFELD, F. V. "Grundriss der allgemeinen Pathologie," Leipz., 1892, p. 41.

BOISEAU. "Historique de la contagion de la phthisie pulmonaire," *Recueil de mem. de méd., de chir., et de pharm. milit.*, Par., 1869, xxii, p. 353.

BUDD, WILLIAM. "Memorandum on the Nature and Mode of Propagation of Phthisis," *Lancet*, 1867, ii, p. 451.

BULSTRODE, H. T. "The Milroy Lectures on the Cause, Prevalence, and Control of Pulmonary Tuberculosis," *Lancet*, 1903, ii, p. 73.

Idem. "Sanatoria for Consumption and Certain Aspects of the Tuberculosis Question," Lond., 1908.

BURKHARDT. "Ueber Häufigkeit und Ursache menschlicher Tuberkulose auf Grund von ca. 1400 Sektionen," *Zeitschr. f. Hyg.*, Leipz., 1906, liii, p. 139.

CLARK, Sir JAMES. "Cyclopædia of Practical Medicine," Lond., 1835, iv.

COHNHEIM, JULIUS. "Lectures on General Pathology," Lond., 1889, sect. ii, p. 735.

CORNET, G. "Die Tuberculose," Wien, 1899.

CULLEN, W. "First Lines of the Practice of Physic," Lond., 1796, ii, p. 434, § 886.

DESAULT, PIERRE. "Dissertation sur les maladies vénérienes . . . avec deux dissertations, l'une sur la rage, l'autre sur la phthisie," Bordeaux, 1733.

FLÜGGE, C. "Die Ubiquität der Tuberkelbazillen und die Disposition zur Phthise," *Deutsche med. Wochenschr.*, 1904, xxx, p. 161.

FRANZ, K. "Die Bedentung des Tuberkulins für die Frühdiagnose der Tuberkulose und die erste Anwendung desselbe in der Armee," *Wien. med. Wochenschr.*, 1902, lii, pl. 1689, 1746, 1794.

DE GIOVANNI, A. "Die Prädisposition zur Tuberkulose," *Allg. Wien. med. Zeitung*, 1907, lii, pp. 484, 496, 508.

GORING, CHARLES. "On the Inheritance of the Diatheses of Phthisis and Insanity: a Statistical Study based upon the Family History of 1,500 Criminals," Lond., 1909.

GREENWOOD. "The Problem of Marital Infection in Pulmonary Tuberculosis," *Proc. Roy. Soc. Med.*, 1909, ii (Epid. Sect.), p. 259.

HART, C. "Die anatomischen Grundlagen der Disposition zu tuberkulöser Erkrankung," "*Ergebn. der allge. Pathologie*," Wiesb., 1910, xiv, i Abt., p. 337.

Idem. "Konstitution und tuberkülose Lungenphthise," *Berl. klin. Wochenschr.*, 1911, xlviii, p. 557.

HAY, MATTHEW. "Report by the Medical Officer of Health for the Year 1909," Aberdeen, 1910.

HEBERDEN, W. "Commentaries on the History and Cure of Diseases," Lond., 1803, 2nd ed., p. 375.

HIPPOCRATES. The Genuine Works of, translated by Francis Adams, Lond., 1849, i. p. 406.

HIRSCH, AUGUST. "Handbook of Geographical and Historical Pathology," Lond., 1886, iii, p. 232.

HOFFMANN, F. "Opera omnia physico-medica," Genevae, 1740, Tomus tertius. De morbis ex viscerum labe partiumque solidarum atonia provenientibus, Cap. XI. De affectione phthisica sive tabe pulmonali, p. 287, § xviii.

HUBER, JOHN B. "Predisposition to Tuberculosis," *Med. News*, New York, 1903, lxxxiii, p. 1201.

LAENNEC. "Traité de l'auscultation médiate," 2me ed., Par., 1826, i, p. 649.

LICHTHEIM. "Ueber Tuberkulose," *Verhandl. d. Cong. f. inn. Med.*, Wiesb., 1883, ii, p. 18.

LOMMIUS, IODOCUS. "Medicinalium observationum," libri tres, Antwerpiae, 1560, ii, p. 53.

LOUIS, P. C. A. "Researches on Phthisis," translated by W. H. Walshe, Lond., 1844.

MARCHAND, F. "Handbuch der allgemeinen Pathologie, hrsg. von Krehl und Marchand," Leipz., 1908, i, p. 13.

MORGAGNI. "De sedibus et causis morborum," Patavii, 1765, i, p. 194 (Epistola anatomico-medica, XXII, art. 3).

MORTON, RICHARD. "Phthisiologia, or a Treatise of Consumptions," Lond., 1694.

NAEGELI, O. "Ueber Häufigkeit, Localisation, und Ausheilung der Tuberkulose," *Virchows Archiv*, Berl., 1900, clx, p. 426.

NEWSHOLME, A. "The Relative Importance of the Constituent Factors involved in the Control of Pulmonary Tuberculosis," *Trans. Epid. Soc. Lond.*, 1906, new ser., xxv, p. 31.

PAIGNON, A. "Qu'entend on par diathèse? Peut-on admettre l'existence des diathèses présidant au developpement des maladies?" *Thèse de Montpellier*, 1838.

PEARSON, KARL. "A First Study of the Statistics of Pulmonary Tuberculosis," Lond., 1907.

POPE and PEARSON. "A Second Study of the Statistics of Pulmonary Tuberculosis: Marital Infection," Lond., 1908.

PUCHELT, F. A. B. "Beiträge zur Medicin als Wissenschaft und Kunst: Bd. I, Ueber die individuelle Konstitution und ihren Einfluss auf die Enstehung und den Charakter der Krankheiten," Leipz., 1823.

RÜHLE. "Ueber Tuberkulose," *Verhandl. d. Cong. f. inn. Med.*, Wiesb., 1883, ii, p. 4.

SALMON, W. "The Practice of Physick, or Dr. Sydenham's Processus integri, translated out of Latin into English with large animadversions and Practical Observations on the same," Lond., 1716, p. 244.

SARDA, G. "Cours de pathologie générale," Montpellier, Paris, 1896.

SCHLENKER. "Beiträge zur Lehre von der menschlichen Tuberkulose," *Virchows Archiv*, 1893, cxxxiv, p. 145.

SCHLÜTER. "Die Anlage zur Tuberkulose," Leipz. u. Wien., 1905.

SHRUBSALL, F. C. "The Anthropometric Investigation of Hospital Patients," *Brit. Med. Journ.*, Lond., 1904, ii, pp. 1689, 1747.

Idem. "Physical Characters and Morbid Proclivities," *St. Bart.'s Hosp. Rep.*, Lond., xxxix, p. 64.

v. SOKOLOWSKI, A. "Statisches betreffend gewisse Momente welche zur Lungentuberkulose veranlagen," *Zeitschr. f. Tuberk.*, Leipz., 1901, ii, p. 210.

STEPHANUS, E. H. "Medicae artis principes post Hippocratem et Galenum," etc. Anno 1567, excudebat Henricus Stephanus.

STILLER, B. "Lungentuberculose und Asthenia universalis," *Trans. of the Sixth Intern. Cong. on Tuberculosis* (Washington, Sept. 28—Oct. 5, 1908), Philad., 1908, i, pt. I, p. 145.

STRANDGAARD, N. J. "Vererbung der Disposition zur Lungentuberkulose," *Zeitschr. f. Tuberk.*, 1911, xvii, p. 54.

VAN SWIETEN. "Commentaria in Hermanni Boerhaave Aphorismos," Leiden, 1764, iv, § 1206.

TURBAN, K. "Die Vererbung der locus minoris resistentiae bei der Lungentuberkulose," *Zeitschr. f. Tuberk.*, 1900, i, p. 30.

ULLERSPERGER, J. B. "Die Contagiosität der Lungenphthise," Neuwied und Leipz., 1869.

VILLEMIN. "De la propagation de la phthisie," Paris, 1869, 21 pp. (reprint of article in *Gaz. med. de Par.*, April 17, 1869).

Idem. "Études sur la tuberculose; preuves rationelles et expérimentelles de sa spécificité et de son inoculabilité," Par., 1868, 640 pp.

Idem. "Cause et nature de la tuberculose," *Bull. de l'Acad. imp. de méd.*," Par., 1865, xxxi, p. 211, and 1866-67, xxxii, p. 152.

VIRCHOW, R. "Die krankhafte Geschwülste," Berl., 1864-65, ii, 21ste Vorlesung.

WALTHER, J. A. "Ueber das Wesen der phthysischen Constitution und der Phthysis in ihren verschiedenen Modificationen, etc.," Bamberg, 1819-23.

WATSON, THOMAS. "Lectures on the Principles and Practice of Physic," Lond., 1848, ii, p. 208.

WERNICH. "Wann steckt Lungenschwindsucht an?" *Berl. klin. Wochenschr.*, 1879, xvi, p. 354.

YOUNG, THOMAS. "A Practical and Historical Treatise on Consumptive Diseases," Lond., 1815.

DISCUSSION.

The PRESIDENT (Dr. Theodore Thomson) said that all present would feel greatly indebted to the authors for the excellent contribution, which set the facts of a difficult subject so well before them. Apart from the scientific merits of the paper, it was a pleasure to listen to one which had such a historical flavour. It was extremely well done. There were many scientific papers which would make more agreeable reading if that aspect were more considered. Then there was also the philosophical aspect; for a discussion of the terms dyscrasia, diathesis, "anlage," reminded him of the days of his youth, when he was struggling through the works of Hegel and Spinoza. There was also a strictly personal aspect of the paper, about which he felt somewhat curious, namely the extent and identification of the combined authorship. He thought he had one small clue, but it was such a harmonious whole that it became difficult to know the respective shares of the authors. The Section was favoured by the presence of not a few who were particularly competent to discuss such a paper, among them Mr. Udny Yule, whom he would call upon to open the debate.

Mr. UDNY YULE said he did not expect to be called upon to speak on a medical subject in regard to which he had no authority. But the paper had interested him greatly; it was one of the most sane and balanced discussions of that very difficult problem which he had read. It was a difficult subject from the statistical standpoint; the statistics seemed to be so constantly open to more than one interpretation. If one was biased in one way or the other, to lean to the greater importance of the soil, or the greater importance of the seed, one was apt to overlook or to minimize the importance of the other interpretation. In that regard he would point out what had not usually been emphasized—namely, the difficulty of knowing what was meant by relative importance of soil and seed, if it was admitted that both were factors. If, on the one hand, it was admitted that the character of the body affected the possibility of infection; if, on the other, it was also admitted that persons, even of the same character of body, in different classes of life or engaged in different occupations, were subject to different probabilities or intensities of infection—and he thought most people were coming to admit that—then surely it was the important conclusion, from the medical standpoint, that both soil and seed were factors. If one were then going to ask which was the more important factor, one must define how the importance was to be measured, and he did not at present see how that was going to be done. With regard to interpretation, the crux seemed to him, statistically, to lie in the interpretation of the coefficient of correlation between parent and offspring and the coefficient between husband and wife. The coefficient between parent and offspring was about on a par with the coefficient observed for many physical characters. It was true that the data had been criticized, and it must be admitted that the value of the coefficient found by Pearson was not only based upon data of a doubtful character, but also on an estimate as to the proportion of persons in the population who suffered from tuberculosis; and in so far the coefficient was subject to a considerable margin. But, at any rate, the coefficient between the appearance of tuberculosis in parent and offspring was considerably higher than the coefficient between the appearance of tuberculosis in husband and in wife. That suggested the importance of the soil. But could one regard as being solely due to difference of *soil*, differences of hereditary disposition, the different rates among different classes of the population? It seemed to him scarcely possible that that could be the interpretation, so far as anthropological research had gone. It was known that different classes of the population differed in some respects, such as the colour of hair and eyes; but the difference was not a very great one, whereas the difference in tuberculosis-rates was very large. It might be supposed that there was some tendency to selection in the production of upper classes as compared with the lower classes. But could it be supposed that there was any large anthropological, quasi racial, difference between such classes as innkeepers and servants, costermongers, general labourers, and a few selected portions of the lower classes who were subject to high tuberculosis-rates on the one hand and the remainder of persons more or less in the same social class on the other? It was surely difficult to suppose that. He admitted that in the endeavour to find some hypothesis which would serve as a temporary standpoint in regarding these matters, he had come rather to the hypothetical conclusion that there might be some very different liability to infection amongst adults, as compared with children. If children were much more liable to infection by tuberculosis than adults, that might explain the great difference between the coefficients for husband and wife and the coefficient for parents and offspring, as observed by Pearson. He admitted that his hypothesis was not based upon any direct data known to himself, and Mr. Greenwood said that no evidence of less susceptibility to infection on the part of adults was known to him. He had simply tried to find some hypothesis which might unite apparently contradictory statistical data.

Dr. SHRUBSALL asked if the attention of the authors had been drawn to a reference by Professor Geddes to an Assyrian inscription alluding to a wasting disease transmitted from man to man by the discharges, since this would antedate the references given in the paper as to early ideas of the contagious nature of consumption. Anthropometric methods, and particularly investigations of colour types, were brought to the fore by Dr. Beddoe, of Bristol, whose works on racial characters in these islands would remain classics. So far as observations had been made in London, it appeared that the darker types, dark-haired and especially dark-eyed, were found more commonly among patients with tuberculosis than in the corresponding populations in the homes. It was, however, of interest to note that though these dark traits predominated even in children suffering from pulmonary tuberculosis, in cases of joint or glandular tuberculosis fair traits predominated. This suggested that the influence of colour type in determining the mode of onset of tuberculosis might be worthy of further investigation. The types might present different *loci minoris resistentiæ*. The difference in coloration did not depend on age, but was found on contrasting children with children and adults with adults.

Dr. F. W. Mott, F.R.S., said he had learned a great deal from the valuable paper that had been read. He was much interested in the subject. About ten years ago the Medico-Psychological Association instituted a collective inquiry as to the prevalence of tuberculosis in asylums, and they came to the conclusion that the patients acquired the disease in the asylums to an enormous extent; and therefore it was a matter of very great importance to the Council whether they should erect separate buildings for the isolation of the tuberculous insane. He recommended that a system of notification should be instituted. That was commenced about eight years ago, and two years ago he published[1] the results of five years' notification, and the post-mortem evidence relating to that subject. He had long doubted the reliability of the collective investigation of the Medico-Psychological Association, because the personal equation came in to such an enormous extent, and it was uncontrollable. He was much struck by the fact in making autopsies that when he found active tuberculosis he nearly always found also obsolescent tubercle in the apices of the lungs or in the bronchial glands. Therefore he concluded that although active tuberculosis was a frequent cause of death in asylum patients, it was probable that those patients had suffered from tuberculosis previously, and that it had lighted up again in the asylum.

In 14·8 per cent. of the autopsies made at the London County Asylums during the past five years, active phthisis was found. Again, a great variation in the returns from the various asylums was noticeable, for which the personal equation must be held largely responsible.

It cannot be shown that the variation in the incidence of tuberculosis in the various London County Asylums depends in any measure upon the class of patient received, the parish areas from which they are drawn, the construction and age of the asylum, or the dietary or treatment. Cane Hill Asylum, situated on a high chalk down, has a comparatively lower death-rate and a lower tuberculosis death-rate than any of the other London County Asylums. The relatively higher death-rate from tuberculosis at Claybury may be explained to some extent by the fact that the necropsies are made by a skilled pathologist, or it might be attributed to the clay soil and cold damp climatic conditions, although the total death-rate is lower than at most of the other asylums.

A comparison of the phthisis death-rate for 1907 at the several age-periods per sane living in London, and per 1,000 of the total living insane population (18,872) resident in the London County Asylums shows that the mortality from phthisis among the insane is highest at a much earlier age-period than among the sane. At the age-period 45—55, when it reaches its maximum among the sane, it is a question whether the incidence among the insane is much greater than among the sane pauper population.

The comparison also shows that the death-rate from phthisis for the insane between the ages of 15—35 is about fifteen times that for the sane for the same age-period. Allowance, however, must be made for the fact that the majority of the inmates of the asylums belong to the poorest classes. The Jewish population at Colney Hatch (mainly aliens) show a higher death-rate (25·7 per cent. of total deaths) from tuberculosis than the Christians, for which fact reasons are given. Evidence is not forthcoming to prove that the staff in the London County Asylums contract tuberculosis from the patients.

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[1] *Archives of Neurology*, 1909, iv, p. 70.

During the past ten years 1,982 necropsies were made at Claybury Asylum by skilled pathologists, and active phthisis was found in 416 cases (20·9 per cent.). Tuberculous ulceration of the intestines was found in 73 males (36·3 per cent.) and 115 females (53·4 per cent.) of the cases dying with active tuberculosis. Twenty-five per cent. of the deaths with active phthisis occurred within one year of admission, and practically 70 per cent. within five years of admission. The post-mortem statistics for the past six years show that 51·6 per cent. of all the patients dying exhibited either obsolescent or active tuberculosis or both.

It was inferred from the post-mortem examination that not more than 10 per cent. of the cases with active phthisis at autopsy were free from tuberculosis when they entered the asylum. It was admitted that the old lesion might have remained obsolescent and a new acquired affection had occurred in some instances. The general conclusion arrived at was :—

Tuberculous infection depends upon dosage and resistance. There is a distinct correlation of pauperism, tuberculosis and insanity. The prophylactic measures for the prevention of tuberculosis are good in the London County Asylums as regards milk, food, personal cleanliness, ventilation, clothing, warmth and exercise in the open air, and the liability of infection from preventable causes, in his judgment, was less than in the houses and tenements from which the majority of the pauper insane are taken.

At the time of the report certain prophylactic measures, such as the open-air treatment, had either not been adopted or had only recently been adopted in some of the asylums. At one institution, Colney Hatch, a separate building has since been provided. It will be interesting to see what effect the earlier and more frequent diagnosis of phthisis with a view to isolation and treatment will have upon the death-rate from tuberculosis in the London County Asylums in the next quinquennium. If there is a considerable fall it does not imply that this is wholly due to sanatorium treatment, for there is a steady decline in the tuberculosis death-rate of the outside population, and this would influence, of course, the number of tuberculous patients entering the asylums.

Dr. Mott also referred to the valuable help he had received from Dr. Bulstrode in reading through the paper and the statistics upon which it was based.

The following were the conclusions he arrived at in consequence of this investigation : Infection in the asylums is not the most important cause of the prevalence of tuberculosis ; still less could the view be endorsed that the causes of tuberculosis inhere in the asylums and not in the condition and character of the patients sent to them.

Notification during five years from all the London County Asylums does not show any ward incidence comparable with that shown by the notification of dysentery during the same period.

The average proportion of living patients reported as tuberculous was 20 per 1,000 inmates of the London County Council Asylums, but the incidence of reported cases showed very considerable variation. The personal equation must largely contribute to these wide differences.

Association with mental disease of the cases reported during life indicates that young subjects suffering from melancholia, dementia præcox, and imbecility are especially prone to the disease.

Post-mortem statistics confirm this correlation, but in addition show that a large number of general paralytics die with recent active tuberculosis which is not diagnosed during life; this is especially the case in regard to female general paralytics, for which fact reasons are given.

Dr. C. J. MARTIN, F.R.S., said the paper was full of scholarship and sound criticism, and also contained a sly humour, which added flavour to it. As far as he could understand, the authors had come to the conclusion that there was no evidence available for measuring the relative values of predisposition as against contagion in the case of phthisis. Mr. Yule had pointed out that there was nothing to measure it by, and it seemed that one was in much the same position as when trying to ascertain how much in the development of an individual was to be attributed to heredity and how much to environment. In the manufacture of a great mathematician, both factors admittedly had their part. If one took a child who had a hereditary predisposition towards mathematics, and placed him under advantageous circumstances, a mathematician was the result. But if that child were kept away from opportunities of instruction, and put to live on a desert island, a mathematician would not result. In intermediate conditions it was very much a matter of taste which was regarded as the more important factor. The authors had suggested that the activity of the medical profession in recent years had been directed by a policy based on the assumption that infection was the all-important factor, and could be got rid of, by means at our disposal. It was easy to see why that should be so. The most striking advance which had been brought about in our time in this subject had been the discovery of the infective nature of tuberculosis, and of its exact nature. This knowledge had overshadowed the influence of soil. Although we might at present neither know to what extent we could by our measures diminish the opportunities for infection, nor whether such diminution were material, we were working in the right direction in trying to minimize the possibilities of infection. To breed a race which should be less predisposed to tuberculosis, just as the agricultural botanist selected a smut-resistant wheat, might be sound policy, but we should not be able to do much in that direction. Every effort towards diminishing opportunities for infection was right; but perhaps where some members of the profession had gone too far was in explaining to all and sundry that they were going to abolish phthisis by such measures. It was presumably towards the justness of encouraging expectations of that kind that the authors had directed some of their remarks.

Dr. DUDFIELD thought that the gist of the paper, which was of great interest, could be summed up in the old Latin proverb "*in medio tutissimus ibis.*" "Soil" and "seed" were of great importance, but there was not any justification for holding either one as of greater importance than the other. He wished to suggest that it was not right to regard disease as the resultant of two factors only, such as "soil" and "seed." Expressing that opinion in quasi-mathematical language—if x represented "soil," y "seed," and z the resultant disease, the processes involved could not be represented by either of the equations $x + y = z$, $xy = z$. He was of opinion that the facts would be more nearly represented by indefinite integrals, which, as was well known, involved the introduction of unknown constants in the process of integration. There were many factors to be taken into consideration in endeavours to elucidate the causation of disease. Some of those factors it was at present practically impossible to evaluate. Questions relating to the parts played by heredity and acquired infection in the production of pulmonary tuberculosis would, he thought, receive fresh light if the family histories now accumulating at tuberculosis dispensaries were properly examined and collated. Such histories were not based solely on vague statements of patients and their relatives, but upon the results of clinical examination of the majority of the living members of the patients' families.

Dr. G. S. BUCHANAN, commenting upon the historical aspect and the views of ancient writers as to the existence of a tuberculous diathesis, said one would like to be satisfied that when talking of tuberculous diathesis or predisposition, they meant the same thing as was meant to-day. The present meaning was the liability of a person, apparently healthy, to become tuberculous; but it occurred to him that all those writers up to Laennec, and even beyond, were not dealing with tuberculosis, including phthisis, as they understood it: their standards in some cases did not include any conception of tuberculosis in the pathological sense, or confirmation by the tubercle bacillus, or thermometrical observations, or auscultation, or percussion; therefore one could not help thinking that many of what the ancient authors thought were physical characteristics of a diathesis were really what they had frequently observed as the actual consequence of the disease, and that what determined types of "disposition" with them was the characteristic of people who, though not acutely suffering, were already tuberculous. With regard to the question of the relative importance of disposition and contagion, he agreed with much that had been said about the difficulty, and, perhaps, the futility, of attempting to appraise the relative importance of disposition and contagion, the more so if the term disposition were not limited to hereditary predisposition, as seemed to be the case in the paper. There was either a third factor to be added, or one must divide disposition into hereditary and acquired disposition. If one took the analogy of seed and soil, one wanted something else, to correspond with the necessary sunlight and rain. Such conditions as those of feeding, fresh air, &c., were generally considered to make an enormous difference, not only to the cure of the person who had contracted phthisis, but also in determining whether the people exposed to tuberculous infection became phthisical or not. And if one was balancing between disposition and infection, one must consider the two factors; not merely the hereditary, but also, and probably the more important, the acquired antagonism to disease due to environment.

Dr. KENTISH WRIGHT considered that one point that had been overlooked in many of the investigations on this subject was a clear distinction between cases of tuberculosis which occurred, and deaths from tuberculosis. That was particularly important in view of the question of the two classes of disposition which had been mentioned in the discussion, the hereditary and the acquired disposition. He thought something might be done by investigating the after-courses of cases of tuberculosis or of phthisis when the conditions were made as favourable as possible, such as by sanatorium treatment and by educative methods such as those practised by dispensaries. In that way some facts might be gained as to whether the predisposition was hereditary or acquired, by observing the effect of improving the conditions of life, and so to some extent eliminating both the acquirement and the disposition, and eliminating also the possibility of infection or re-infection.

Dr. BULLOCH in replying said that the only object which he and Mr. Greenwood had in bringing the paper before the Epidemiological Section was to enter a protest against the wild statements now being made in the lay and medical Press, that the whole problem of phthisis was one of infection. Medical history showed that in tuberculosis, as also in the case of other diseases, the most extreme views were taken, not by those who had contributed the actual advancement in knowledge, but by those whose business it was to apply those advancements for the needs of the public. There were a large number of well-ascertained facts which were not entirely explicable on the doctrine that disposition was not an important factor in the genesis of the disease, and that before rigorous measures were applied on a wide scale the actual facts should be ascertained. He did not agree that public health authorities must always "do something." This "doing something" should be put a stop to until there was a reasonable supposition that it was going to achieve its end. He did not wish it to be understood that the tubercle bacillus was not a potent factor. What he did refuse to believe was that it was the only factor. He considered that the disposition, the power of the individual to resist the aggressive inroads of the bacillus, was greater than many people held at the present day.

Epidemiological Section.

May 26, 1911.

Dr. THEODORE THOMSON, C.M.G., President of the Section, in the Chair.

The Scientific Uses of the Notification of Disease.

By ARTHUR RANSOME, M.D., F.R.S.

THE greater part of the present paper was written in November last. Its object was to urge the importance of making a more complete and scientific use of the returns of infectious disease now made to all medical officers of health. On submitting it to Dr. Bulstrode, however, he at once informed me that, by a circular issued by the Local Government Board on December 14, 1910, a most important step had already been taken by that authority in the desired direction. By this new Order (No. 56, 217) "an additional duty is imposed upon the medical officer of health, by sub-clause (13) of sending to the Board, weekly, a list of infectious diseases notified in his district, and a duplicate of the list to the medical officer of the county."

Before the issuing of this important Order, the chief use that had been made of these returns was to indicate the place and time at which cases of such diseases were first discovered in a district, their notification enabling the medical officer to adopt measures for the prevention of their spread. Owing to these warnings, undoubtedly many thousands of lives have been preserved to the nation.

This practical object of the returns, however, was not the only one aimed at by the original promoters of the measure. They had also pointed out that these documents might very well serve for scientific uses as well. Now, after many years, it seems probable that this additional object may shortly be attained.

The immediate consequences of the new Order will undoubtedly be so far-reaching, in the direction of scientific research, that it might well seem unnecessary for me to trouble the Section with my own few and belated views on the subject, but Dr. Bulstrode and your Hon. Secretary, Dr. Hamer, have persuaded me still to bring the matter before your notice, in the hope of starting an interesting discussion on the best modes of utilizing the new material now brought together for the first time.

Before attempting this task, however, permit me shortly to trace the history of the movement for what was called at first "The Registration of Disease." It is probably not known to the majority of your members, and it is desirable that it should be placed upon record.

In the year 1859 it fell to me, as Hon. Secretary to the Manchester and Salford Sanitary Association, to propose some means by which the Committee could be kept informed as to the health of the town. It was desired to obtain something more definite than the written or verbal reports, which up to then had been made by the various branches, and which had not been very satisfactory. I ventured to suggest that an attempt should be made to imitate the weekly returns of disease then being made to the Society of Medical Officers of Health in London, under the direction of the Hon. Secretary, Dr. R. D. Thompson, of Marylebone; these returns being a statistical enumeration of all cases of certain classes of diseases, coming under treatment at all the various hospitals, dispensaries and Poor Law institutions in the Metropolis. Some opposition was met with at first, on the ground that the undertaking was too vast for our small Association to tackle, but eventually I was given a free hand, and, after a few weeks' exertion, by making personal calls on all desirable contributors, I was able to obtain the help of twenty-nine men in Manchester and Salford, who promised to fill up forms of returns every week, and post them to our Secretary. It says much for the public spirit of our profession that no refusal was met with, and that the work was done for many years without fee or reward, and with no more than a recognition of their great services in the weekly returns of disease which were then commenced, and which were continued for more than ten years without a break.

Throughout the whole of that time no alteration was made in the character of the returns, except a slight change in the list of diseases, introduced in deference to the wishes of the British Medical Association. From first to last, the returns were made from the same institutions and from the same districts. No contribution was discontinued, no fresh institution was entered upon the records. They were thus entirely homogeneous, and suitable for comparison from one period to another. Moreover, the returns were singularly complete, notwithstanding the voluntary character of the agency; in the course of the ten years scarcely a single return was missing. Omitting the first few months, before the organization was complete, in the first year only two returns, out of 1,456, were not sent in, and after that not a single contribution was lacking!

Some of the useful results obtained by this long series were summarized by me in a paper, entitled "Ten Years of Disease in Manchester and Salford," which was read in August, 1870, at the Annual Meeting of the British Medical Association at Newcastle-on-Tyne. It was published in the *British Medical Journal* for December 3, 1870, and I beg to refer to that paper for details of what had so far been accomplished.[1]

With regard to our present subject, it is important to point out that, in the very first of these returns, in addition to their obvious use in giving information as to the health of the several districts, attention is called to their importance as "medical statistics, from which conclusions might in future be drawn respecting such subjects as the causes of disease, the laws of epidemics, the relations of atmospheric changes to various disorders, the influence of times of plenty or scarcity of food, and many other questions of interest to medical science."

The movement, however, did not end here. The advantages of a similar registration of disease, throughout the country, were brought before the Lancashire and Cheshire Branch of the Association, and afterwards before the council of the central body. Committees from each of these Associations were appointed to consider the subjects, and, at the same time, to promote the "combined observation of disease," by the profession. Both of these committees did good work. The branch started an inquiry into the "duration of infection," in the course of which some useful results were obtained; and it also encouraged the registration of the same diseases as we had scheduled in Manchester and Salford; but, with the exception of returns made by Dr. R. Brown from Preston, and by Dr. Alfred Hill from Birmingham, no progress was made towards the nationalization of the registration of disease. The promotion of this latter object was therefore left in the hands of the parent Association, and, although its work in the desired direction extended over many years, its course may be briefly indicated. A subcommittee "on the registration and observation of disease" was appointed

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[1] *Brit. Med. Journ.*, 1870, ii, p. 597.

(and to this I was first Hon. Secretary, and afterwards Chairman). It met many times, issued reports, and memorialized successive Governments, but it is now important to point out that, in all these documents, not only was comment made on the immediate value of the notification of disease, but stress was laid upon the scientific importance of the records. Ultimately, after thirty years of strenuous effort, in the year 1889, the invaluable "Notification of Infectious Diseases Act" was passed, and it has proved an enormous boon to local authorities, to the public, and, incidentally, to the profession. It remains for us now, more than twenty years after its passing, to see that its powers are brought still more fully into play. It will be acknowledged, I think, that in the direction of scientific research these powers have never yet been fully exercised. Several important diseases are still not included in its lists, and after the warning to the medical officer of the presence of the case of infectious disease in his district, with a few important exceptions, no further use has been made of the return. It has either been destroyed or has been left, with others, to accumulate upon his shelves.

Thanks to the beneficent action of the Local Government Board and its permanent officials, all this is now altered, or soon will be; and we are now placed in the position of being able to make a few suggestions as to how these valuable data may further be utilized. It may be assumed that all the collected facts will be marshalled with the great skill which we have been accustomed to look for in the head officials of the Local Government Board. That they will be projected upon charts, plotted upon maps, and district compared with district. That they will be collated with meteorological data, and with all the other circumstances likely to have a bearing upon medical questions. It only remains, therefore, to attempt to indicate some of the possible directions of future researches. Before doing this, however, let me incidentally point out how much better adapted to our purpose are records of sickness than those of deaths.

Until the coming of the notification of disease, the only means we had of studying the relation of disease to time and space were the mortality returns of the Registrar-General; but these, even when marshalled by the genius of Dr. W. Farr and his successors, were always most inadequate for this inquiry. With few exceptions, the real origin of the disease, the true cause of death, preceded the fatal issue weeks, months, or even years before this event, and thus afforded little or no information as to its ætiology or predisposing causes. The returns of fresh cases of sickness, on the other hand, are made known immediately on the discovery of their nature, and can thus be brought into relation with concurrent circumstances.

There are still many points with regard to the natural history of epidemics that require elucidation. Without in any way ignoring or belittling the magnificent results that have been obtained in bacteriological laboratories, and in the various expeditions undertaken for the purpose of studying bacteriological or parasitic diseases in their native haunts, it must be acknowledged that we still need a systematic study of these and other disorders.

There are, indeed, many interesting points still requiring study, and some of them may be here indicated. Probably sundry others will occur to the members of our Section.

(1) *The Relations of Epidemics and certain other Diseases to States of the Weather.*—The observations of Hippocrates, Sydenham, and many other old writers, need to be repeated in the light of more exact data, though it is marvellous how accurate these have proved to be. Bronchitis and catarrh, and pneumonia, diarrhœa and dysentery have long been recognized as the chief diseases to be affected by the weather. In successive papers, based upon the returns of sickness made to the General Board of Health in London, in 1857, and on those already mentioned made to the Manchester and Salford Sanitary Association between 1859 and 1879, I adduced statistical proofs of the truth of this observation.[1]

In the case of pneumonia, bronchitis and catarrh, the rise and fall of these disorders very closely followed the fall and rise of the thermometer, and the highest and lowest readings of one series of figures accorded with the lowest and highest of the other (Fig. 1, Chart II). I venture to think that these observations are worthy of repetition; but, in any case, they open the way to further research. It is now well known that mere exposure to cold will not of itself cause any of these diseases, but that they are due to specific microbes; and yet their curves show the close relation that exists between them and any extreme lowering of the temperature. Hence, therefore, we are constrained to admit that the time-honoured cautions against "catching cold" and "taking a chill" have some reason in them. I am not aware that any sufficient explanation has yet been given of this fact; whether it is due to nerve influence, to a blanching of the mucous membranes by cold, and the consequent lack of white corpuscles in the parts, or to a reduction in the opsonic power of the blood? Surely we have here some need of further research!

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[1] "Influence of Atmospheric Changes upon Disease," *Memoirs of Lit. and Phil. Soc. of Manchester*, 1859-60; "Health and Meteorology of Manchester," 1862; "Disease in St. Marylebone and Manchester" (Manchester and Salford Sanitary Association), Manchester, 1866; "Epidemics, studied by Statistics of Disease," *Brit. Med. Journ.*, 1868, ii, pp. 207, 386; "Ten Years of Disease," *Brit. Med. Journ.*, 1870, ii, p. 597.

The case of diarrhœa and dysentery is somewhat similar, but here it is the rise of temperature that coincides with the increase of the diseases; and further, something more definite has been alleged as to the critical degree of temperature at which the organisms of these disorders begin to be virulent (Fig. 2, Chart VI). In our remarks upon the subject in 1859, Mr. Vernon and I noted the fact that "A high mean temperature of the (preceding) week seems to have a powerful influence in predisposing to diarrhœa; when continuous, causing a rapid increase in the number of cases"; and again, that "a temperature below 60° F. appears to be unfavourable to its progress." This "action is generally most evident when the temperature is above or below the average of the season." In a subsequent Report to the Local Government Board, in 1887, Dr. Ballard confirmed this observation, but fixed the critical point as being when the "mean temperature, recorded by the 4-ft. earth thermometer has attained somewhere about 56° F." Here, again, I should be glad to be informed whether any explanation of the fact has been given either by bacteriologists or biologists, and, if not, would call attention to the need for researches bearing upon it.

These so-called "climatic" diseases are not the only ones apparently influenced by the weather. Measles and whooping-cough (Fig. 1, Charts I and III), and even scarlet fever (Fig. 2, Chart V), and possibly enteric fever, have been shown to be amenable to its changes. This, again, needs some explanation.

(2) The next group of questions for investigation includes those concerned with the causes of the *Epidemic Constitution of the Air*, as it was called by Sydenham, *ἥ κάτάστασις λοιμῶδης τῶν ὡρεῶν*, as it was named by Hippocrates. Sydenham expressly says that these "constitutions originate neither in their heat nor their cold, their wet nor their drought." He ascribes them rather to "hidden and inexplicable changes within the bowels of the earth."[1] Undoubtedly, the charts which I constructed from our own returns, and those obtained by Dr. Thompson, showed a remarkable correspondence in their curves.

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[1] Med. Obs. 1, 2, and 5.

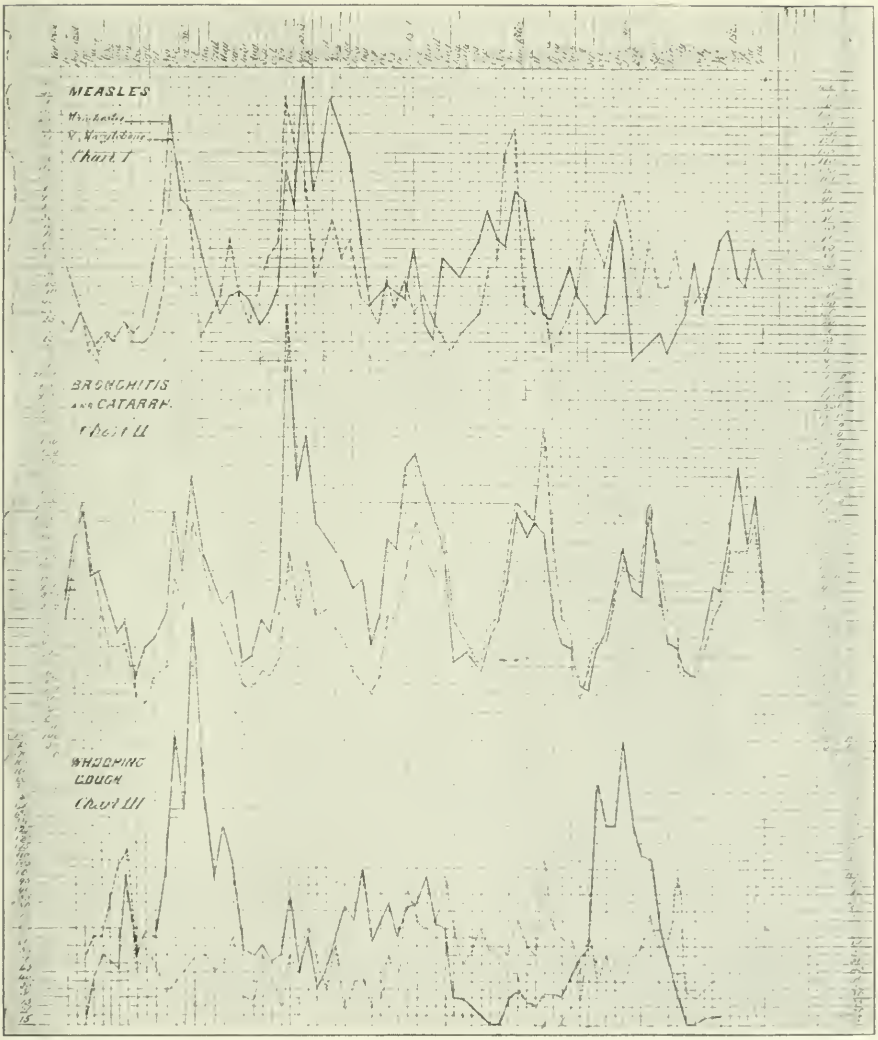
MEASLES
Chart I
BRONCHITIS AND CATARRH.
Chart II
WHOOPING COUGH
Chart III

FIG. 1.

Notwithstanding the distance of these places from one another, their different climates, their various manufactures, and the different occupations and mode of living of their inhabitants, there is a close relation between even the lesser undulations of the waves of their diseases, such as could only be accounted for by the hypothesis of a common cause. There must be, in fact, some "pandemic" influence at work. This, I think, has never yet been accounted for and needs a more extended inquiry than has yet been given to it.

(3) *The influence of endemic conditions upon epidemics* also needs to be studied. Even with the limited data at our command there is evidence of the existence of some such influence. Thus, affections of the air-passages (Fig. 1, Chart II), bronchitis and catarrh, and pneumonia, are more common in Manchester than in St. Marylebone, and we find, accordingly, that measles (Chart I) is rather more prevalent in Manchester than in London. It is interesting to remark that in the two winters 1860 and 1864 the lung diseases in St. Marylebone mounted above those in Manchester, and in those years only did measles become more prevalent in London. Whooping-cough again, another zymotic attacking the air-passages, never rises in St. Marylebone to the height it attains in the smoky towns of Preston and Manchester. Cholera, on the other hand, has usually been more severe in London than in Manchester. It seems highly probable that when regular returns of disease are made all over the country, many interesting and, perhaps, important conclusions may be drawn respecting the favourite haunts of particular diseases, and the reasons for their prevalence in various parts of the country.

(4) Again, we need to know more about the *causes for the varying fatality of some epidemics.* Scarlet fever, for instance, is sometimes as virulent as the plague; at others so mild that it passes almost without notice. Similar observations have been made regarding measles. Is there any satisfactory explanation of these facts?

(5) Another point demanding research is the reason for the varying incidence of the poison of these disorders upon different parts of the bodily frame. Why, for instance, influenza should sometimes content itself by attacking only the upper air-passages, at others cause serious mortality from pneumonia, and at others, again, seize upon the brain and nervous system? Why also should simple catarrh sometimes attack the bile-ducts and cause epidemic jaundice; at another, work mischief to the stomach and bowels? Measles, again, may be associated with an unusual number of attacks of pneumonia, or with severe forms of inflammatory croup.

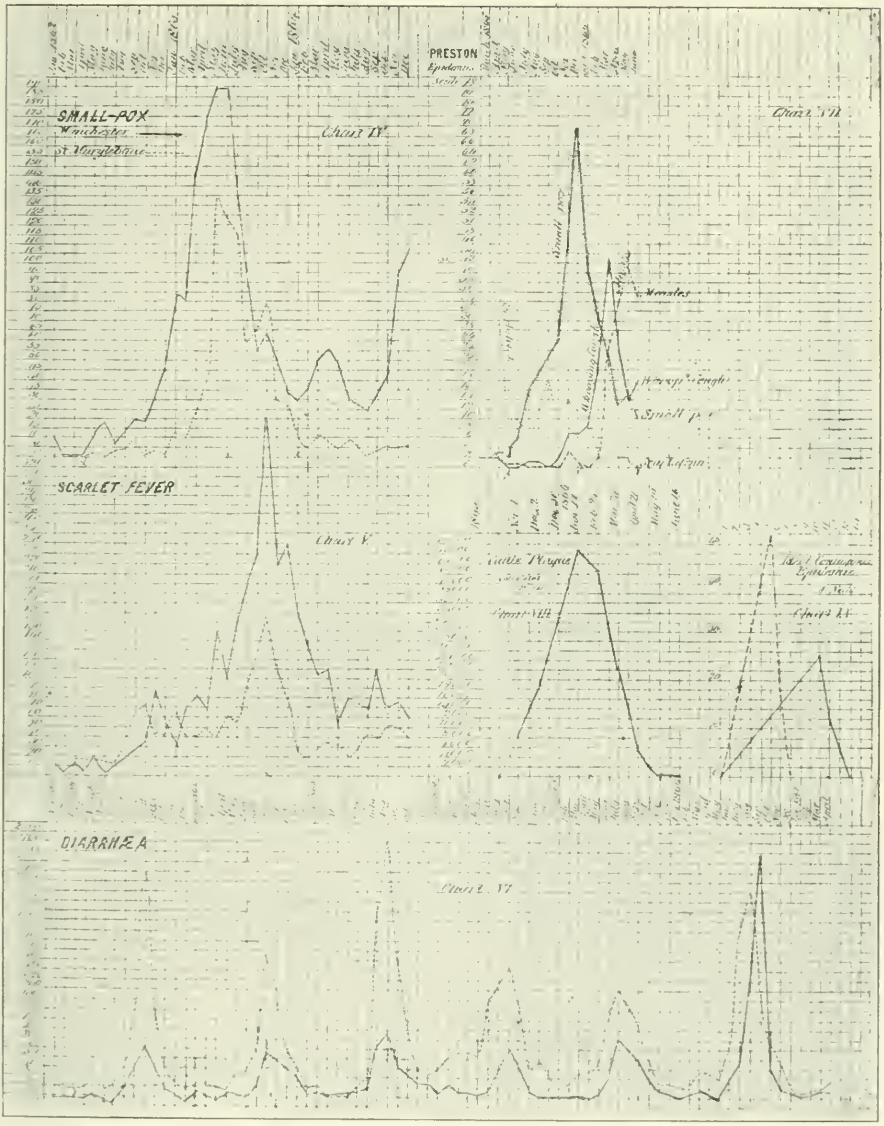
SMALL-POX
Winchester
St. Marylebone
Chart IV
PRESTON
Chart VII
Measles
Whooping-cough
Small-pox
SCARLET FEVER
Chart V
Cattle Plague
Chart VIII
Chart IX
DIARRHŒA
Chart VI

FIG. 2.

(6) What, moreover, are we to say as to the varying nature of *the sequelæ of some epidemic complaints?* Why should enteric fever at one time seem to lead to an improvement of the general health, and at others, like measles, lay the foundations for tuberculous disease. Scarlet fever is sometimes followed, in a large number of cases, by acute nephritis, with a heavy mortality. It has been my lot also to witness three severe epidemics of diphtheria, and in each there were, as sequelæ, characteristic lesions of different parts. The first of these outbreaks was the most fatal; and it was remarkable for the number of cases of paralysis that followed it. The second was much milder, but it was singular how very irregular was its mode of attack. In place of appearing on the tonsils, or in addition to its manifestation there, the exudation appeared in the nasal passages, on the lips, or on the buccal mucous membrane. There was evidence of it in the ears, on the vulva, on blistered surfaces, and even on wounds of the skin. In this epidemic, moreover, the peculiar scarlet rash was rarely entirely absent, though it appeared irregularly, sometimes a fortnight or three weeks after the first commencement of the disease. The third epidemic was chiefly remarkable for the rheumatic symptoms that commonly followed the attacks. It is hardly likely that peculiarities such as these were confined to the place in which they were observed; but owing to the absence of any general oversight of the epidemics, it was impossible to learn whether they were purely local phenomena or whether they were dependent upon more universal causes. It is precisely on points such as these that we may hope now to obtain most valuable information from the officials of the Local Government Board.

We may perhaps place under the same heading the tracing of cases of supposed immunity from certain of these diseases; or again, the strong predisposition of certain families to such diseases as enteric fever or diphtheria; and inquiry might elicit facts worthy of being placed in apposition with the well-known immunities of certain races, and the proneness of others to contract certain epidemic disorders. All persons are not equally liable to take infectious disorders. As there are some people who can resist, to a greater or less extent, the action of drugs or poisons, so there are individuals who pass through life without suffering from any epidemic disease, and others who take every complaint that is going.

(7) Another curious phenomenon which needs attention is the *succession of epidemics* (*see* especially Fig. 2, Chart VII); it is one often noted by Sydenham. In his "Description of the Epidemic Constitution

of the Years 1669-72, in London,"[1] he speaks of one epidemic being driven out by another "ut clavum a clavo," thus using the simile employed by Shakespeare, "As one heat another heat expels, or as one nail by strength drives out another," or as Erasmus puts it, *ἡλῳ τον ἧλον εκκρούει.* We are certainly still ignorant of the connexion between certain epidemic and other precedent, or concurrent, disorders; and yet they would often afford an explanation of the predisposition to these epidemics, as I have endeavoured to show in several articles.

(8) *The Form of the Wave of Epidemic Disease.*—I have already dealt with this subject in a paper read before the Epidemiological Society in June, 1882, and must refer to this for the conclusions then formed; but I would now recall the fact that I then discussed Dr. Farr's theory of the progressive attenuation of the epidemic virus,[2] and also the "smouldering spark" theory, as it might be called, of simple contagion, and, at the end of that paper, I expressly submitted them as open to criticism, but as worthy of further study. The data for such study will now, I hope, soon be forthcoming in the Notification Returns to the Local Government Board.

(9) A further subject of interest in this regard would be the corroboration, or otherwise, of Pasteur's views upon the means by which an infective virus of lowered intensity may again regain its virulent power. He demonstrated the fact that, if an attenuated virus can be made to destroy some very young or enfeebled animal, the infection from this animal will regain all its former virulence and fatality. Now we know that very often epidemics, after rapidly spreading for a time, subside a little, and we might hope that the true decline had begun, but, unfortunately, they make a fresh start, and either rise still higher than before or they show one or more lesser, but still considerable, fluctuations on their descending track. It would surely not be very difficult to ascertain whether, in the case of the sudden rise of an epidemic after a preceding fall, the renewal of virulence could have been due to the deaths of either very young children or of enfeebled persons. I had the privilege of submitting this point to Pasteur himself, at the meeting of the International Congress in London, and he expressed himself as very anxious that the observation should be made.

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[1] Med. Obs., Sect. 4, chap. 1.

[2] Dr. Farr's curve for cattle plague is given in Fig. 2, Chart VIII, and that due to simple contagion in Chart IX.

(10) And, so far as I am concerned, lastly, we have to account for what I have ventured to call *epidemic cycles,* the remarkably definite "periodic times" observed by most forms of epidemic disease. In a paper on this subject, read in January, 1880, before the Manchester Literary and Philosophical Society, I put forth a sort of provisional hypothesis to account for these "cycles." I then suggested that before a true epidemic can take place it is necessary that there should be present in the community a sufficient density of the population at ages and conditions susceptible to the disease. I then said: "Probably all the facts would be accounted for if we suppose that these disorders can only become epidemic when the proximity between susceptible persons becomes sufficiently close for the infection to pass freely from one to another."[1] Surely this and other hypotheses need to be put to the test of further investigation.

Many of these problems seem now to be in a fair way to be solved, together with others that will, doubtless, present themselves to members of our Society. Owing to the new Order of the Local Government Board the whole of the returns of notified sickness will be available as medical statistics, and will serve as material for an adequate study of epidemic disease. With a little method and organization the needful machinery may now be set up, and with it will be woven a valuable tissue of scientific results.

In conclusion, I would fain remark upon the favourable position in which we stand.

(1) We have the whole of our profession enlisted in the army of observation. These men are well trained to the work, and many of them are exceptionally well equipped for the campaign. All of them have studied, more or less closely, the features of these disorders, their usual course, their sources, their incubation periods, and the probable duration of their infectiveness. Many of these men have also a practical acquaintance with bacteriology, and will take a keen interest in the solution of the problems that will be presented to their notice. Need I add that the inquiries to be set on foot will increase the interest in their daily labour?

(2) The facts, gathered by such intelligent observers, are sure to be accurately taken down; the diagnosis of the diseases will be carefully made; the time and place of their occurrence is certain to be correctly noted; and thus we shall have a remarkably trustworthy record, far

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[1] "On the Form of the Epidemic Wave," *Trans. Epid. Soc.* (1881-82), 1883, N.S., i, p. 96.

more trustworthy and more suitable for our purpose than the returns of deaths now made to the Registrar-General.

(3) Furthermore, the diagnosis of the cases need not be left to the unassisted acumen of the original observer. In any case of doubt, I believe that the assistance of the local medical officer of health could be obtained, and this verification of the facts would tend to make the records of sickness admirable from a statistical point of view. In this respect also, therefore, the records of new cases of disease could be made superior to the registers of death. The great statist, Dr. W. Farr, in the later years of his life, vainly endeavoured to obtain the appointment of officials whose duty it should be to verify the fact and the cause of death. It would be easy to obtain this verification in doubtful cases of disease.

(4) For the purpose of a serious study of epidemics all these observations, so carefully and accurately made, will now be preserved by the local authorities in each district of the Kingdom, and they may at once be utilized for this object by the officials of these authorities. There will be nothing to hinder any local authority from becoming a sort of medical observatory, like a meteorological station or an astronomical centre.

(5) And lastly, we have in the Local Government Board an admirable central body to supervise the whole scheme. It will, no doubt, appoint officials, whose duty it will be to study carefully all the returns, to draw conclusions from them, after collation with other facts, and, finally, to indicate the points requiring further elucidation and special study. We may fairly hope that from this office will come, not only forecasts and warnings of coming outbreaks of epidemics, but that, from the vast amount of material that will soon accumulate, will be extracted the solutions of most of the problems with which we are now especially concerned.

The Epidemiology of Poliomyelitis.

By FREDERICK E. BATTEN, M.D.

INTRODUCTION.

THE purport of this paper is to collect and put together in a concise form the facts which are known in relation to poliomyelitis as an infective and epidemic disease. For this purpose the subject will be dealt with under the following heads:—

(I) Definition of the term "poliomyelitis" and clinical aspect of the disease.
(II) Nature of the virus.
(III) History of past epidemics.
(IV) Certain details in the investigation of some recent epidemics:—
    (*a*) Wickman's investigation of Swedish epidemic, 1905.
    (*b*) Lovell's investigation of Massachusetts epidemic, 1907.
    (*c*) Krause's investigation of Westphalia epidemic, 1908.
    (*d*) Potpeschnigg's investigation of Steiermark epidemic, 1909.
    (*e*) Lindner and Mally's investigation of Steyr epidemic, 1908.
(V) Epidemics of poliomyelitis in Great Britain in 1908, 1909, 1910.
(VI) Distribution of the disease in London in 1904.
(VII) Conclusion.
(VIII) Appendix, list of epidemics, literature.

(I) DEFINITION OF THE TERM "POLIOMYELITIS" AND CLINICAL ASPECTS OF THE DISEASE.

The term "poliomyelitis" has been used to designate the disease under discussion because it is a name that has come into general use, and calls up to most people the clinical picture of a child with a flaccid palsy of one or more limbs. Personally, I prefer the term "polioencephalomyelitis," indicating that not only the spinal cord but any part of the nervous system may be affected. The term "infantile paralysis" is objected to because, although children are to the largest extent affected, yet the disease is by no means limited to them.

The terms "Heine-Medinsche-Krankheit" and "epidemic paralysis" have been suggested. With regard to the first, it has the objection common to all diseases described by men's names—viz., that the name conveys no indication of the nature of the disease. With regard to the second, I think there is much to be said in its favour; paralysis is a wide term and would include most of the manifestations of the disease, the word epidemic, although true with regard to a large number of the cases which occur, yet is by no means always so.

It is important to have a clear understanding of the clinical aspect of this disease. Poliomyelitis or polio-encephalomyelitis is an acute infective disease, having a special selective action on the nervous system, and giving rise to a variety of symptoms dependent on the portion of the nervous system affected. Such symptoms may be (1) a localized paralysis of one or more limbs; (2) an ascending paralysis, often rapidly fatal owing to the involvement of the respiratory centres: (3) an acute ataxia; (4) a paralysis of one or more cranial nerves; (5) hemiplegia; (6) the symptoms of meningitis; (7) an acute mental defect; and (8) a type of case in which pain, especially on movement, is the most marked feature—so-called neuritis.

Such is the modern conception of the disease. It is fully recognized that there are abortive cases with transient symptoms with or without paralysis; but these cannot be identified except when occurring in direct association with a marked case of the disease. Their true nature will only be fully proved when some serum reaction or animal test is forthcoming.

(II) NATURE OF THE VIRUS.

It is not the purpose of this paper to deal with the nature of the virus of poliomyelitis, but no paper on the epidemiology of the disease can be complete without a clear understanding as to what is known of the virus. The work of Landsteiner, Popper, Levaditi, Flexner, and Lewis has shown that the disease is transmissible from man to monkeys by inoculation, that it is transmissible from monkey to monkey, that the virus is not killed by glycerination, and that it will pass the finest filter. The organism has not yet certainly been cultivated *in vitro,* has not yet been stained, and has not been seen under the highest powers of the microscope. The virus can be obtained from the mucous membrane of the nose of a monkey which has suffered from the disease, and may be obtained from this situation six months after the date of the infection in a still active condition (Osgood and Lucas).

Animals other than the monkey and ape seem to be immune, although Meinicke states he is able to infect rabbits. The similiarity of the virus with that of rabies is striking. The pathological lesion in poliomyelitis is also very similiar to that of rabies. It has recently been stated by Vipond that cases of poliomyelitis give a positive Widal reaction. In no case which has been tested for me by Dr. Forbes, Pathologist to the Children's Hospital, has a positive Widal reaction been obtained, neither has he succeeded in finding the organism described by Dixon, Fox, and Rucker in the blood of cases of poliomyelitis, but the number of such cases is at present too small to be of any great value.

On experimental grounds the incubation period of the disease would seem to be from five to six days. The clinical evidence would seem to indicate that the period is shorter, the evidence being in favour of a period below four days.

(III) History of Past Epidemics.

I do not propose to give a detailed account of all the past epidemics of poliomyelitis that have occurred. A good review of 35 epidemics up till 1907 will be found in the paper by Holt and Bartlett, and further details in the papers of Wickman and Job and Froment.

Since 1907 I have collected the record of some 26 epidemics in all parts of the world, and these, together with those previous to 1907, are recorded in a list which is appended to this paper.

The leading features of the past epidemics may, however, be shortly stated. The first record of the disease in an epidemic form comes from Sweden in 1881. (I have purposely omitted an epidemic recorded by Colmer in America in 1841, because I can find no account of it.) In 1883 some groups of cases were recorded in Italy, and in 1886 in Norway, Germany, and France. All of these epidemics were small in numbers. In 1887, however, Medin described an epidemic in Stockholm of 44 cases, and this is the first important work on the subject. In 1894 an epidemic occurred in Rutland, Massachusetts, of 132 cases, which were recorded by Caverly and McPhail. Small epidemics were recorded in the 'nineties in Italy, France, Australia, England and America, and a larger epidemic of 42 cases occurred in Vienna in 1898, and in Norway in 1899, and in Sweden in 1899. The latter epidemic was recorded by Wickman, to whom so much is due with regard to the investigation of the pathology and the spread of the disease. In the middle of the next decade, viz., 1900 to 1910, the record of cases, which before had been limited to two figures, now reached to four figures, and during the years 1903-07 it may be said that the disease was pandemic in Norway and Sweden, and during the years 1907-10 large epidemics occurred in the State of New York and in the State of Massachusetts. At the same time epidemics of the disease were recorded in Australia (Stephens), in Vienna (Zappert), Westphalia (Krause and Reckzeh), in Paris (Netter), in Austria (Fürntratt, Potpeschnigg, Lindner and Mally), Switzerland (Hagenbach), in Russia (Jogichess). (*See* maps, figs. 1, 2, 3.)

EPIDEMIC POLIOMYELITIS.

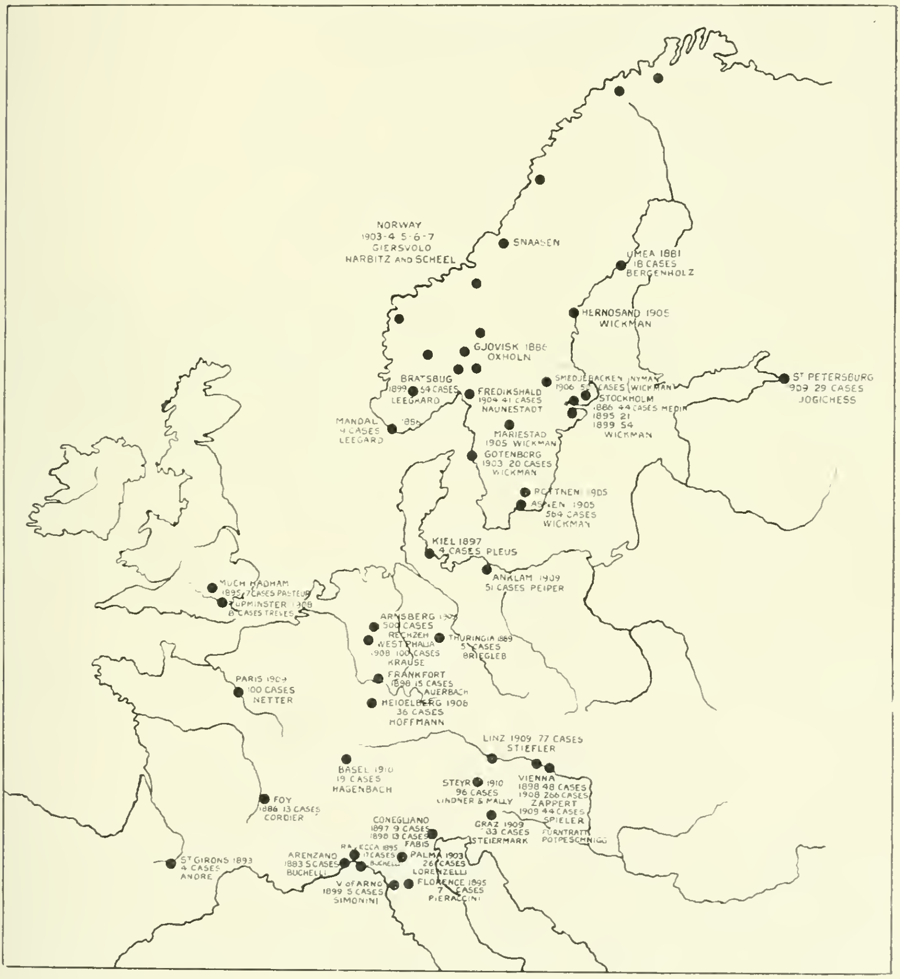

FIG. 1.

Map of Europe, showing the places at which epidemics of poliomyelitis have occurred, the number of cases in the epidemic, the date of the epidemic, and the name of the recorder.

During the year 1910 the number of cases of poliomyelitis, and deaths caused by the disease, recorded in the United States is as follows: Pennsylvania heads the list both in prevalence and in mortality, there were 1,097 cases and 172 deaths in that State. In Iowa there were 638 cases and 161 deaths. In Massachusetts there were 588 cases; the number of deaths is not given. Apparently the epidemic was most continuous in Iowa, for that is the only State from which cases and deaths were reported for every month of the year. The maximum number of cases in any month was 378 in September in Pennsylvania; the maximum number of deaths, 64, in the same month and the same State. The maximum number of cases in any month in the entire Union was 1,146, in September; the maximum number of deaths, 182, in August. The total number of cases and deaths of poliomyelitis in the United States during the year 1910 was 5,093 and 825 respectively, a mortality of about 13·75 per cent.

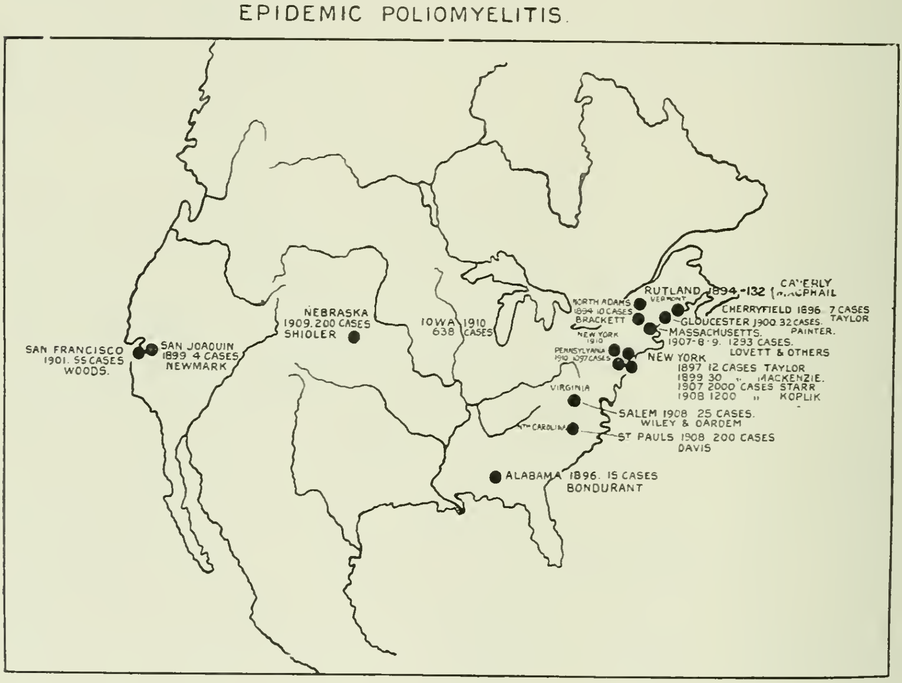
EPIDEMIC POLIOMYELITIS.
SAN FRANCISCO 1901. 55 CASES WOODS.
SAN JOAQUIN 1899 4 CASES NEWMARK
NEBRASKA 1909. 200 CASES SHIOLER
IOWA 1910 638 CASES
NORTH ADAMS 1894 10 CASES BRACKETT
RUTLAND 1894 -132 VERMONT
CAVERLY MACPHAIL
CHERRYFIELD 1896 7 CASES TAYLOR
GLOUCESTER 1900 32 CASES. PAINTER.
NEW YORK 1910
MASSACHUSETTS. 1907-8-9. 1293 CASES. LOVETT & OTHERS
PENNSYLVANIA 1910 1097 CASES
NEW YORK
1897 12 CASES TAYLOR
1899 30 " MACKENZIE.
1907 2000 CASES STARR
1908 1200 " KOPLIK
VIRGINIA
SALEM 1908 25 CASES. WILEY & OARDEM
Nth CAROLINA
S^T PAULS 1908 200 CASES DAVIS
ALABAMA 1896. 15 CASES BONDURANT

FIG. 2.

Map of America, showing the same facts as in fig. 1.

I have made no attempt to give all the rest of the epidemics, and for these I must refer you to the list in the appendix, but I have said enough to show you that epidemics were widely distributed over the whole of Europe, and were present in America and Australia. Though I can find no record of South Africa having suffered, yet I know from private information that the disease was epidemic in Pietermaritzburg in 1902, and I have seen other cases which have occurred in South Africa during the past ten years.

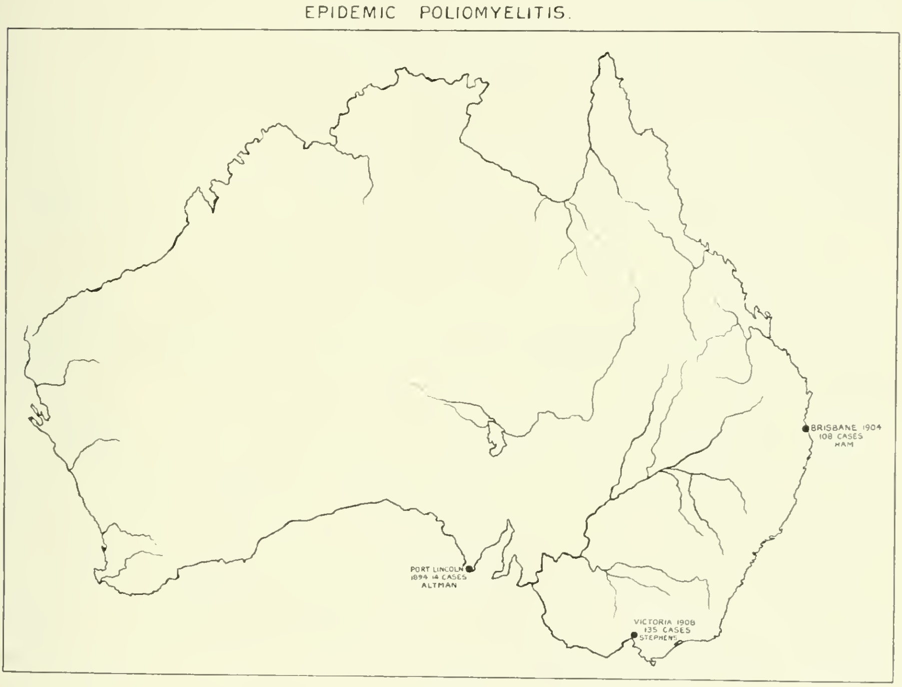
EPIDEMIC POLIOMYELITIS.
BRISBANE 1904 108 CASES HAM
PORT LINCOLN 1894 14 CASES ALTMAN
VICTORIA 1908 135 CASES STEPHENS

Fig. 3.

Map of Australia, showing the same facts as in fig. 1.

(IV) Certain Details in the Investigation of some recent Epidemics.

Most epidemics have their particular interest, and it is sometimes from the investigation of small epidemics that most is to be learnt. In the present instance, however, the evidence is so largely on the negative side that I have selected a few of the best investigated recent epidemics as instances of what has been done. I propose to select the following for consideration: Wickman's investigation of the Swedish epidemic of 1905; Lovell's Massachusetts epidemic, 1907, 1908, 1909; Krause's Westphalian epidemic, 1908; Lindner and Mally's Steyr epidemic, 1909; and Potpeschnigg's Steiermark epidemic, 1909.

Factors common to all Epidemics.

There are certain factors common to all epidemics, and these may be shortly stated.

Seasonal Relationship.—In the Northern Hemisphere the disease always has the greatest prevalence during the months of July, August, September and October, the month of August being nearly always that in which the greatest number of cases occur. In the Southern Hemisphere the disease has occurred during the months of March and April—i.e., months which, I believe, in atmospheric conditions correspond fairly well to September and October in the Northern Hemisphere.

Age Incidence.—The incidence of the disease on young children is a constant feature, and it commonly falls upon children during the second and third years of life. Babies in arms are rarely affected; as the age advances the incidence rapidly declines. In the Massachusetts epidemic of 440 cases under 6 years of age, 304 occurred in children between 1 year and 4 years old.

The Mortality.—The mortality in the various epidemics varies considerably; over the total number of cases it amounts to from 11 per cent. to 12 per cent. One epidemic had a mortality of 18 per cent. (Rechzeh), and another had a mortality of 5 per cent. to 6 per cent. A great deal depends on how these statistics are made up and whether the abortive cases are included. The mortality is higher for children of older age than it is for young children. In the Massachusetts epidemic the total death-rate was 8 per cent., whereas the mortality in children over 10 years of age was 20 per cent., for under 1 year of age 16 per cent., and between the ages of 1 and 10, 4 per cent.

(a) The Swedish Epidemic of 1905 (Wickman).

In this epidemic the incubation period was shown to lie between one and four days, 74 of the 127 cases coming within this period. There are some striking individual cases of isolated contact which show that the incubation may be as short as twelve hours. As a good instance of school infection one may take the outbreak that occurred in the parish of Trästena.

The first cases occurred in Sandbäcken, a neighbouring village, in February and March; three children were affected and one died. An interval of three months now elapsed in which there were no cases. At the end of May and beginning of June, two cases occurred in the same house in Marienstad, a village a mile off. There was then another interval till the end of June and then the outbreak at Trästena began. The first case occurred on June 28 in an isolated house. Early in July a series of cases occurred, mostly in connexion with the school. In this village, out of 102 houses 19 were affected. The numbers of the individuals affected in each house were as follows: In six houses, one case; in three houses, two cases; in five houses, three cases; in three houses, four cases; in two houses, five cases. (*See* diagram, fig. 4.)

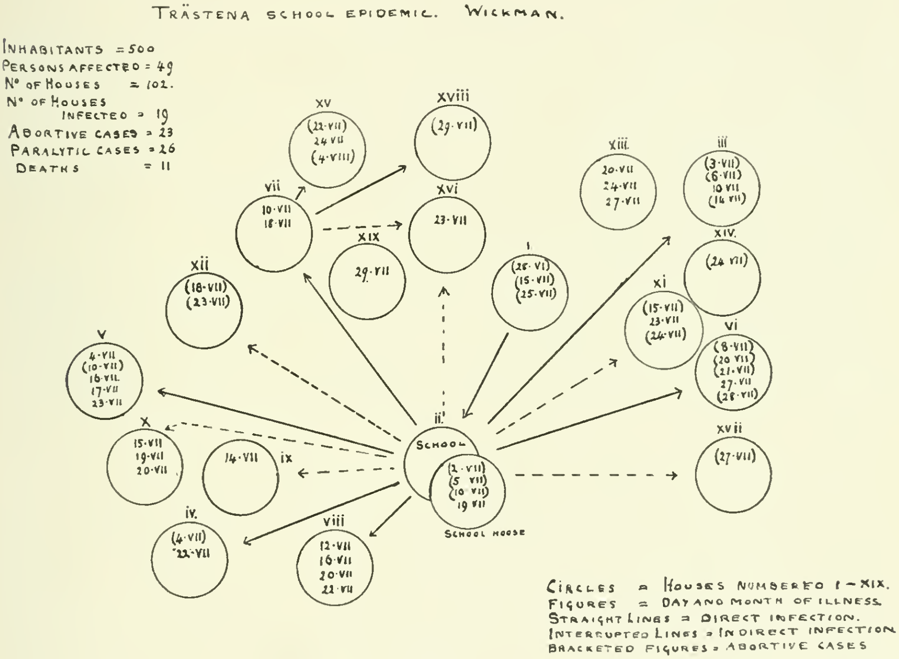

FIG. 4.

The school was infected on June 28, and a series of cases occurred on July 3, 4, 8, 10, and 12. The school was shut on July 15. Other cases occurred during the month, and the last on August 4.

In this little parish of Trästena, with 500 inhabitants, 49 persons were affected, 23 with the abortive form of the disease, 26 with the paralytic form. Of the cases with paralysis, 11 died—i.e., 42·3 per cent.

Three other instances of a school being a source of infection are given by Wickman. The disease appears always to be carried by contact, but the contact may be by means of a perfectly sound and healthy individual. The disease was most prevalent in Sweden in 1905 and was less in 1906 and 1907.

(b) The Massachusetts Epidemic.

Epidemics of poliomyelitis have been investigated in Massachusetts in the years 1907, 1908, 1909. In the year 1907 some 234 cases were recorded; in the year 1908, 136; and in the year 1909, 923. Most of the cases occurred in and around Boston. The transmissibility of poliomyelitis is shown to be considerably less than either scarlet fever or diphtheria. Of persons known to have been exposed to diphtheria, scarlet fever, and poliomyelitis, the percentages of those infected were as follows: Scarlet fever, 22 per cent.; diphtheria, 17 per cent.; poliomyelitis, 6 per cent. The investigators dealt with 150 of the cases of the 1909 epidemic in detail.

Rainfall, temperature, surroundings, nearness to railroad, nearness to water, age of house, sanitary conditions, location of house, character of house, floor of house inhabited by a family, sewage disposal, character of water supply, relation to dust, prevalence of vermin, insects and rodents, data as to domestic animals kept, occurrence of paralysis in animals, swimming and wading, exposure to heat, cold or damp, diet, and attendance at school, were all investigated. I do not propose to give the results of the investigation of all these various points, most of them are strikingly negative. One or two, however, deserve some mention. Of the 150 cases, 62 were swimming or wading in water more or less contaminated by sewage before the attack. In 87 of 142 families affected, animals of some sort were kept, and in 34 of these there was some illness or paralysis. In no case was a child living on breast milk alone affected by the disease. Thirty-five of 150 cases attended school: the school could *not* be shown to be a source of infection. Another most interesting point is that only one child out of 3,600 who lived in forty-five institutions was affected with the disease. With regard to animal infection the following note from Dr. C. S. Shore is of interest:—

> "In my veterinary practice during the past five or six years, I have found a disease appearing among one- and two-year-old colts that shows a line of symptoms corresponding very closely to anterior poliomyelitis of children.

I have had from five to ten cases a year during this time, the cases always occurring during the summer months, and the majority of them during the month of August. The affected colts are usually found in the pasture, unable to stand. The owner sometimes will notice an unsteady gait for twenty-four hours before entire loss of motion occurs. At first these colts have a rise of temperature ranging from 103° F. to 104° F.; pulse and respiration accelerated; animal sweats profusely; appetite remains fairly good, but there is some trouble noticed in swallowing, especially water; slight derangement of the bowels, tending toward constipation; more or less tympanitis present; retention of urine, for a few hours at least; head drawn back so that the end of the nose tends to assume a position somewhat on a line with the neck. The death loss is less than 10 per cent., but in those that do not recover the market value is depreciated to a very great extent because of the faulty gait the animal assumes after an attack of this disease, due to atrophy and contraction of certain muscles, or certain groups of muscles. It seems that the flexor muscles of the limbs especially are more often affected than the extensor, and in almost all the cases some of these deformities are likely to remain permanent. The flexors of the limbs are liable to contract and cause volar flexion of the fetlock. The elevators of the head are also likely to become affected, so as to cause the head to have a poky appearance—that is, it is carried out from the body. After one of these attacks the colt will remain down from one to three weeks, and will then continue to improve for a period of one year, but seldom, if ever, makes a complete recovery.—Dr. C. S. SHORE, Lake City, Minnesota."

I have referred this question to Sir John McFadyean, and he states that he has never seen such a disease in young colts.

(c) The Westphalian Epidemic of 1908 (Krause).

In this epidemic, which occurred in and around Hagen in June, July, August, September, and October, 1908, there were 634 cases with 66 deaths. The epidemic is not worked out carefully from the point of view of its spread from town to town. Individual instances are given of the carriage of the infection from person to person. A grandmother is said to have carried the disease from one grandchild to another who lived in a different town. A mother infected her own child after visiting her infected nephew, and in two instances members of the same family were simultaneously affected. An instance is given in which, at the same time that the daughter of a family was taken ill, two young hens died with paralytic symptoms. The examination of these animals gave a negative result. Krause says the disease not infrequently occurs in children at the breast. He believes that the entrance of the virus is at the intestinal tract, since 90 per cent. of the cases had intestinal symptoms (*see* diagram, fig. 5).

(d) The Steiermark Epidemic, 1909 (Potpeschnigg).

This epidemic began in September, 1909, and amounted to 434 cases, 60 of which occurred in Graz, and there was a mortality of 13 per cent. The question of the contagiousness of the disease was discussed. In favour of such a view is the fact that more than one member of the family may be affected, and also that persons coming in contact with the infected develop the disease. As an instance of this the following case is given: At the end of June the two-year-old child of a tailoress was taken ill with meningeal symptoms, and died in a few days of the "Heine-Medinsche" disease. To this child, when ill, the two-year-old child M. K. was brought to play, and sat on the bed. Six to eight days later the child developed typical poliomyelitis, there being no other possible contact. In Leibnitz, a place in Mid-Steiermark, the seven-year-old school child M. P. was taken ill on June 20; two days later the six-year-old child A. S., who sat next to her at school and was her playfellow, had follicular tonsillitis. In the course of the week the mother of this child and five brothers and sisters had tonsillitis, and of these the mother and two children had poliomyelitis.

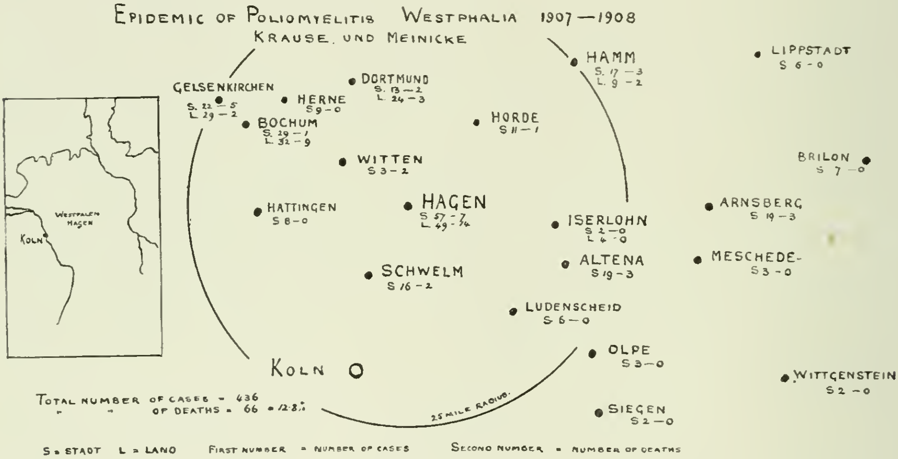

FIG. 5.

Against the infectivity of the disease may be urged, *first*, the absence of spread of infection in hospital. The cases of poliomyelitis admitted to hospital freely mixed with other cases in the ward without any isolation or disinfection, some 70 children came in contact, but no infection took place. On these grounds it is probable that the paralytic stage of the disease is not contagious. *Secondly*, the striking absence of infection when contact has been most close. In November, 1909, H. E. was taken ill with poliomyelitis; all five brothers and sisters, although in closest contact, remained unaffected. In October, 1909, M. K., aged 2½, was taken ill; two sisters, aged 6 and 11 respectively, slept with and were in close contact with the child and remained unaffected. Twin sisters, aged 2½, one was affected, the other unaffected. *Thirdly*, that contact is not the only possible method of dissemination of poliomyelitis is proved by the fact that the disease may arise in persons in whom all contact with affected persons can be excluded.

The instances given of this are as follows: In 1908 the child of a cowkeeper, 30 kilometres removed from the next place in the Alps, was taken ill with poliomyelitis. The child of a railway watchman, whose house could only be reached through a long tunnel, was affected. The contact in school to which Wickman attaches so much importance could not be shown to be a factor in the Steiermark epidemic. It is striking that a large percentage of children are affected before the school age—viz., between 1 and 5 years.

The Steyr Epidemic.

In the epidemic which occurred in Steyr in the late summer of 1908, described by Lindner and Mally, 92 cases are described; 16 of these died. Of these

1 case	occurred in	34 houses	4 cases	occurred in	1 house
2 cases	,,	16 ,,	6 ,,	,,	2 houses
3 ,,	,,	1 house	7 .,	,,	1 house

Two points especially attract attention, the first is the short period between the infection of members of the same family, and the second is the way the disease distributed itself along the railway line and the valley running out of the main valley in which the rail runs. The conclusion arrived at is that the infection of poliomyelitis is carried from individual to individual, and not by means of water or food material, and that the school *is* a method of disseminating the disease.

(V) Poliomyelitis as an Epidemic Disease in Great Britain.

So far as I know, no record of any epidemic of poliomyelitis was recorded in England before 1897, when Dr. W. Pasteur described the affection in seven members of one family. That record has become historic, as it is the record number of members affected in the same family.

Dr. Thomas Buzzard, in 1898, published a lecture "On Cases illustrating the Infective Origin of Infantile Paralysis." He gives an instance of two sisters residing in the same house who were attacked with paralysis within six days of each other. Four other children of the same family escaped. Two other children living in a neighbouring street were taken ill in a similar way in the same week. Dr. Buzzard also mentions a case in which a man cut his knee and stopped the bleeding by using moss from the roadside. A week later he was affected with poliomyelitis. He also mentions a case, seen in 1895, of a brother and sister who were taken ill within two days of one another; the sister was feverish and recovered without paralytic symptoms, the brother was paralysed in the left arm. These two cases are alluded to in Dr. Buzzard's presidential address before the Clinical Society, 1895.

It has, of course, long been recognized that poliomyelitis is prevalent during the summer months in London, and especially during August and September. I have a record of undue prevalence in 1902.[1]

Upminster Epidemic, 1908.

In 1908 Treves reported an epidemic of eight cases in July, August, and September, 1908, at Upminster, a small village in Essex on the London, Tilbury, and Southend Railway, with a population of 1,477.

	No.	Name	Age	Taken ill
	1	Gertrude H.	3½	July 27; paralysis August 3
	2	John C.	13	August 24; paralysis August 27
Return from Westgate August 18 (sisters)	3	Enid H.	11	August 24; paralysis August 30
	4	Maud H.	12	August 24; not paralysed
Return to Upminster, September 3 (brother and sisters)	5	Stanley M.	8	September 9; paralysis September 11
	6	Eva M.	10	September 11; paralysis September 13
	7	Gladys M.	6	September 12; paralysis September 14
	8	Basil J.	13	September 16; paralysis September 16

With regard to incubation period, the child in whom the disease first started had not been out of Upminster, so the disease must have originated in that village. E. and M. H. returned to Upminster on August 18, and were attacked on August 24. The M. family returned to Upminster on September 3, and the first was taken ill on September 9, so the incubation period could not have exceeded six days.

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[1] *Lancet*, 1902, ii, p. 1677.

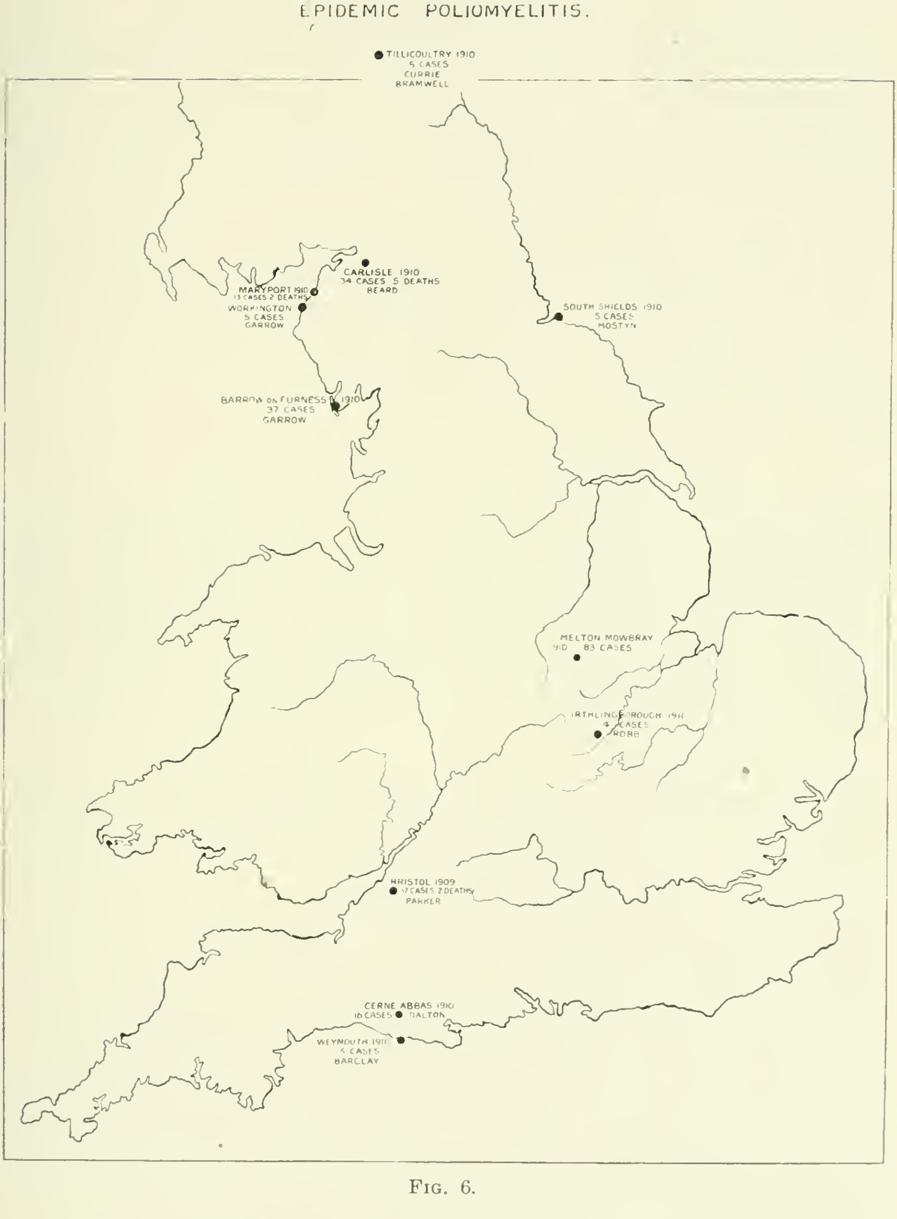

FIG. 6.

Map of England, showing the places at which epidemics of poliomyelitis have occurred during 1909-10, the number of cases in the epidemics, the date of the epidemic, and the name of the recorder.

Bristol Epidemic, 1909.

An epidemic of 37 cases is reported by Dr. George Parker in Bristol during the year 1909. Thirty-two of these occurred between June and October—i.e., during the summer (a cold and wet summer). The ages of the patients varied from 4 months to 16½ years. Three were adolescents. The average age of the children was 2 years. There was only one family of eight children in which two members were found to be affected. Two pairs of cases lived within a few doors of one another, but the interval between their attacks was, in one instance, July and November, and in the other, September and December. This epidemic was investigated after its occurrence, and probably only represents the most severely affected individuals of a hospital class. Two patients died, giving a mortality of 5 per cent.

Epidemics in England, 1910.

Dr. Garrow reported 13 cases of poliomyelitis in the town of Maryport, Cumberland, in August and September, 1910. One of these cases occurred at the village of Crosby, a little way out of Maryport—this child, aged 1 year and 10 months, died. It is worthy of note that her elder sister suffered from an attack of poliomyelitis on June 16, 1908, when 2 years and 3 months old, and recovered almost completely in four months.

Dr. Garrow has been most kind in furnishing me with further information which he has collected about the epidemics of Workington and Barrow-in-Furness, and Mr. Beard, the Medical Officer of Health, has furnished me with the following details of the epidemic in Carlisle in September, 1910. He, recognizing that the disease was occurring in an epidemic form, obtained the sanction of the Health Committee to make the disease notifiable. In Carlisle 34 cases were notified; five died. Of the remaining 29, one only appears not to have had the disease. Of the 28 cases, 12 will probably be incapacitated for the remainder of life, either from total or partial paraplegia, hemiplegia, or monoplegia of leg and arm. In Carlisle there are three examples of two cases occurring in the same houses, all three examples being in the part of the town where the cases were most common, the pairs are enclosed in a circle on the map (*see* fig. 7).

Dr. Garrow investigated the West of Cumberland and came upon scattered instances of the disease at various villages, as indicated on the chart (*see* map, fig. 8). At Barrow-in-Furness he found about 37 cases of permanent cripples. As he says, one can only guess at the actual number of cases. The severe cases which died and the slight cases which recovered are lost for statistical purposes. The incidence of the disease seems to have fallen heavily on North and South Vickers Town: four cases in the former, six in the latter. Dr. Garrow says these are recently built model villages, situated on the slopes of Walney Island, about 50 ft. above sea-level. The houses are well built, streets well paved and clean, and the sanitation modern. The houses have good water supply, and are lighted by electric light. In short, the hygienic conditions are well-nigh perfect (*see* map, fig. 9).

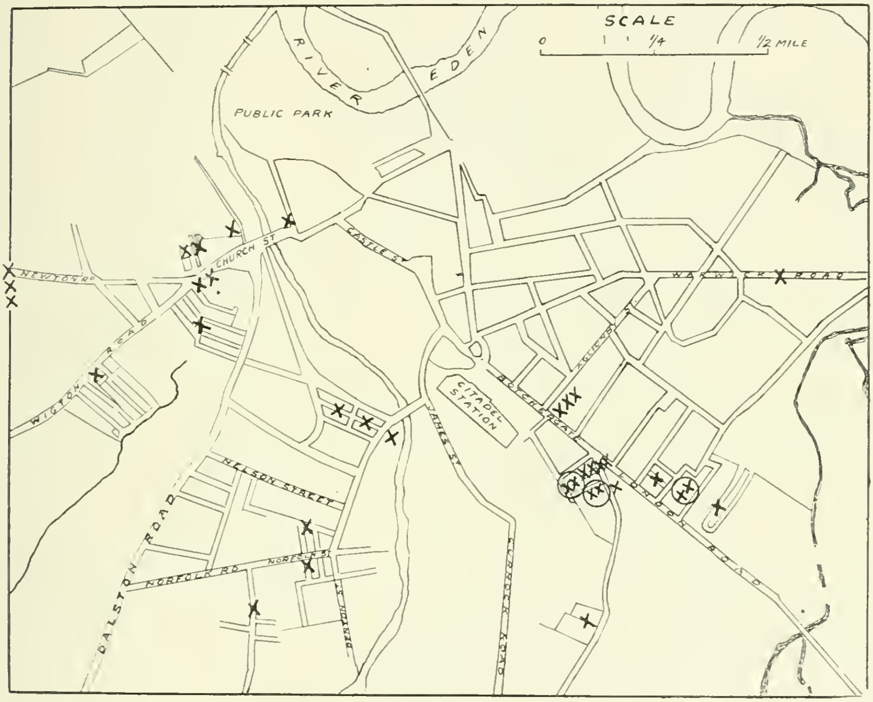

FIG. 7.

Street plan of Carlisle. The X's indicate the cases of poliomyelitis occurring during September, 1910. There were thirty-four cases, of whom five died. There were three instances of two members of one household being affected. These are marked with a circle on the map. Reported by Mr. Beard.

With regard to the epidemic in the Midlands, which occurred around Melton Mowbray, I can furnish no detailed information, as the matter is under investigation by the Medical Officer of the Local Government Board, and his report is not yet published. It is known, however, that 83 cases occurred, with 14 deaths—i.e., 16·8 per cent. The epidemic was at first reported as one of cerebrospinal meningitis, but probably because observers did not recognize the meningeal form of poliomyelitis.

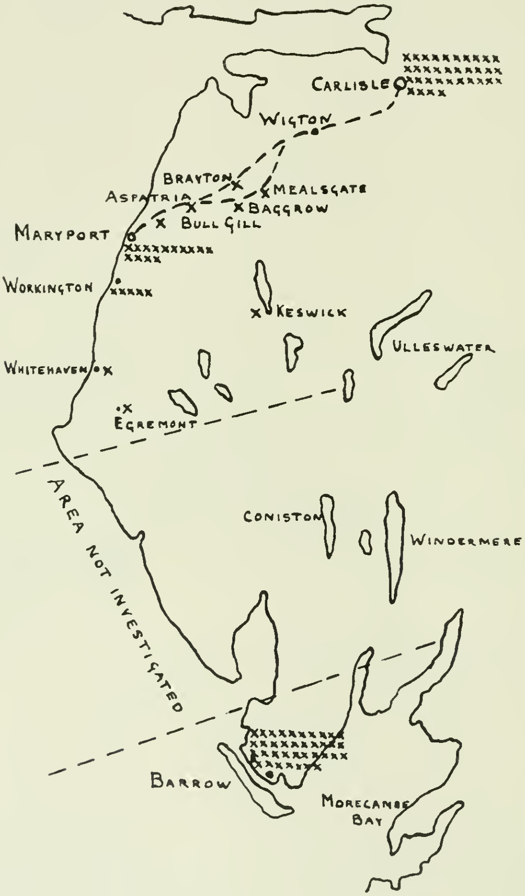

FIG. 8.

Coast of Cumberland, investigated by Dr. Garrow. The X's indicate the cases of poliomyelitis which occurred in 1910. Epidemics occurred in Carlisle, Maryport, Barrow-in-Furness, and other places.

Dr. Robb, of Irthlingborough, Northamptonshire, reported a small epidemic of four cases occurring within a radius of 200 yards, and as far as he could gather there was no intercommunication between the respective houses. The cases were as follows:—

Case I.—E. M., aged $2\frac{1}{4}$ years, was taken ill at the end of August, 1910, with feverishness and fretfulness, which the mother put down to teething: paralysis of the left leg was noticed next morning.

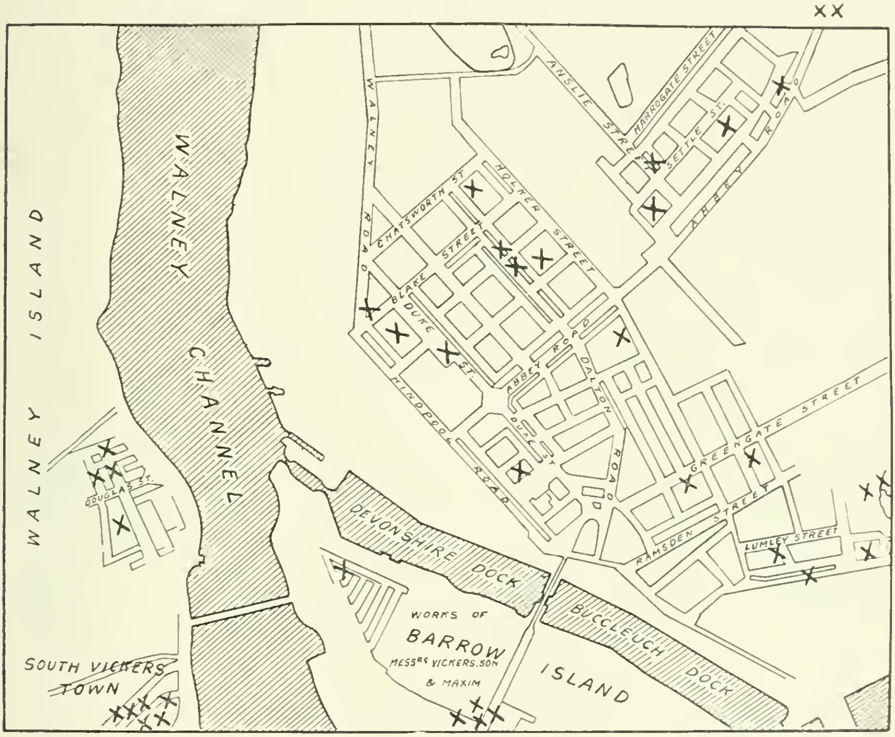

FIG. 9.

Street plan of Barrow-in-Furness. The X's indicate the cases of poliomyelitis occurring during 1910. There were thirty-seven cases. Map prepared by Dr. Garrow.

Case II.—J. H., aged 2, taken ill at end of August, 1910; paralysis of left leg and arm.

Case III.—W. M., aged 3, taken ill first week in September, 1910; paralysis of the left leg.

Case IV.—T. H., taken ill end of September, 1910; paralysis of left arm and leg. He quite recovered by Christmas.

Dr. Robb says: "Although I have been in active practice for over twenty years I have not, to the best of my recollection, had a series of such cases within so short a time."

Dr. Dalton, of Cerne Abbas, reported another small epidemic of 16 cases which occurred in Cerne Abbas, in Dorsetshire. The first of these cases occurred on September 29 and the last on October 24, 1910. Fourteen of these cases were in children under 8 years of age, one case in a boy 14 years old, and one in an adult 36 years old. There were two deaths—in children aged 14 years and 6 years respectively. Four remained badly paralysed, the others made good recoveries. Except for one case in an isolated cottage on the hills the disease was limited entirely to the parishes of Cerne Abbas and Upper Cerne. The former has a population of about 600, the latter, a mile away, is just a small village. In one house Dr. Dalton reports three cases out of four children; in another, two out of three; in another, one out of six: in another, one out of three; in another, one out of four; in another, two out of three. The adult case, a man, the father of four children, lived in quite a small cottage. It seemed almost impossible for the children to escape infection, and yet they did. The possible source of infection in this case was that the man was clipping a horse in a stable with the father of a child suffering with the disease. No local condition could account for the disease, and many cases were in the better houses. The cases were quite scattered as regards houses. Isolation and disinfection of contacts and possible carriers seemed at once to stop further spread of the disease. Dr. Dalton suggests that foreign flour is the carrier of the disease.

In relation to the above epidemic Dr. W. B. Barclay, Medical Officer of Health for Weymouth, says there were five cases of poliomyelitis and one death during the year, and that he knew of none in the previous six years.

There appears to be some evidence that the disease was prevalent in and about Newcastle, but the facts obtainable are scanty and may possibly represent simply the normal incidence of the disease. Five cases were mentioned to Dr. Mostyn, the Medical Officer of Health of South Shields. A series of cases occurred in and about Hexham, and the Medical Officers of Health, Dr. Penford and Dr. Jackson, both consider that the cases have been more frequent than formerly.

In York two deaths were registered from the disease, and in Gloucester Dr. Bond reports three cases in one family.

The evidence obtainable from the Medical Officers of Health must of necessity be most defective in regard to a disease which does not come within their province; but I could wish here to thank all the Medical Officers of Health who have so kindly and courteously answered the questions and returned the post-cards sent to them.

One other small epidemic in September, 1910, is reported by Currie and Bramwell. It is known as the Harvieston epidemic, a private estate near Tillicoultry, in Clackmannanshire. The farmstead consists of four houses, which may be designated A., B., C., D. House A. is occupied by the factor, the three adjacent cottages by farm employees and their families.

The A. family consists of 2 children, A.A., aged 5; B.A., aged 2½.
,, B. ,, ,, 4 ,, A.B., aged 7½; B.B., aged 5½; C.B., aged 4; D.B., aged 7½.
,, C. ,, ,, 4 ,, All aged over 8.
,, D. ., ,, 2 ,, Aged 2, and a few months respectively.

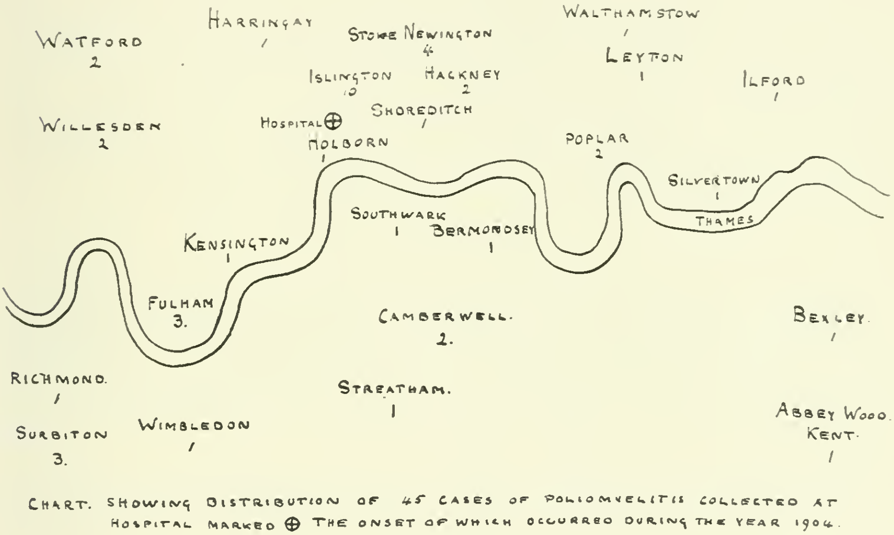

Fig. 10.

Map of London district, showing the distribution of forty-five cases of poliomyelitis collected at two hospitals. The onset of the disease occurred during the year 1904.

Five children in these four houses were attacked, A.A., B.A., A.B., B.B., and D.B. Children taken ill in following order:—

B.B.	...	September 12	...	Returned to school September 26.
A.B.	...	September 16	...	Paralysed on September 19.
D.B.	...	September 18	...	Squint.
B.A.	...	September 20	...	Paralysed right face.
A.A.	...	September 24	...	Paralysed both lower limbs.

From a consideration of the facts relative to these cases, the authors came to the conclusion that the incubation period in this epidemic was four days, or less, and from the fact that the children of the A. family did not visit the children of the B. family after the first child, B.B., was taken ill, but that the mother of the A. family visited and nursed the B. children whilst ill, the infection to the A. family was probably carried by her.

(VI) DISTRIBUTION OF CASES IN LONDON, 1904.

During the year 1904 I collected at the Children's Hospital some 45 cases of infantile paralysis. I have charted them in the districts in which they occurred. The chart has no special value except to show that the disease is fairly evenly distributed over all London areas (*see* chart, fig. 10). Ten of the cases occurred in the Islington District, but considering the position of the hospitals at which the cases were collected—i.e., National Hospital, Queen Square, and Children's Hospital, Great Ormond Street—it would be from this district that cases would be likely to come. So far as I know all these were isolated cases. With regard to the month of incidence, these few figures bear out what has been shown in all larger statistics—viz., that by far the larger number of cases occur in the month of August (*see* diagram, fig. 11).

(VII) CONCLUSION.

From the evidence which has been brought forward one is justified in regarding poliomyelitis as an infective disease occurring in epidemic form during the months of July, August and September. It tends to affect children rather than adults. It is probably communicated from person to person, or may be carried by a person who presents no sign of the disease.

The resemblance of the disease to rabies suggests the possibility of some animal infection, but none has yet been found. The disease can be communicated to the monkey, and passed from monkey to monkey. The infectivity of the disease is not great, for many persons in the closest contact with the infected escape, and monkeys living in the same cage with infected monkeys fail to contract the disease. But little is known of the incidence of the disease in London. That it occurs every summer, and with greater frequency in some years than others, is known. But it is not known if it affects one district more than another.

It will, I think, be admitted that a disease should be notifiable whenever there is liability of its being transmitted from person to person, provided that the disease itself or its sequelæ are a serious detriment to health, or a danger to life. That poliomyelitis is detrimental to health no one will deny.

That poliomyelitis can be, and is, transmitted from patient to patient has been made evident in all the recent epidemics investigated.

The advantages of notification are great. It will lead to the general recognition that the disease is infective, and will tend to prevent its dissemination by contact. Notification will show the prevalence of the disease, and the investigation which should follow must throw light both on the cause and means by which the disease is disseminated.

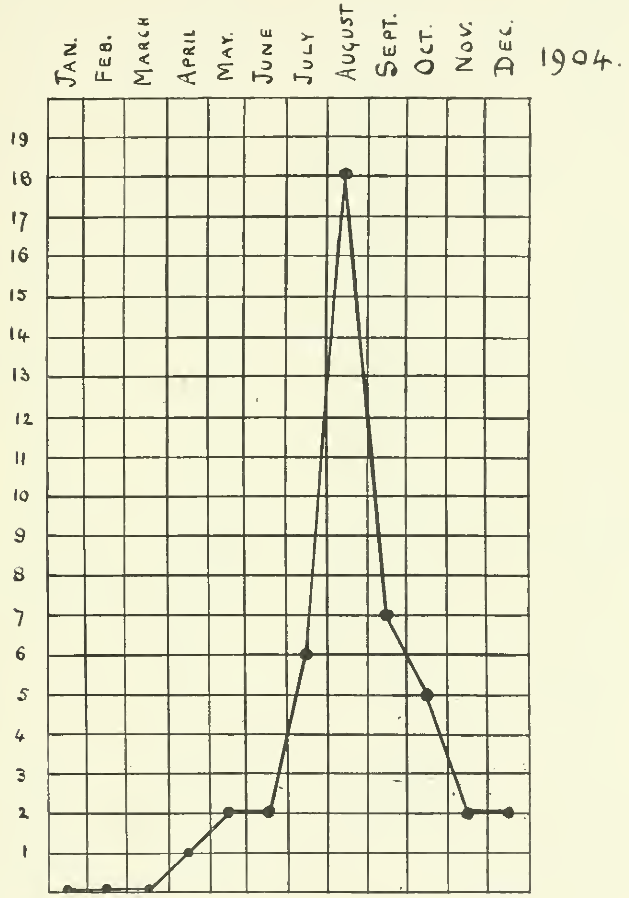

FIG. 11.

Chart showing the month of onset of forty-five cases of poliomyelitis occurring in London during the year 1904. These cases were collected at the National Hospital, Queen Square, and the Children's Hospital, Great Ormond Street.

The infectivity of the disease is not great and the isolation of the infected should diminish the incidence of the disease on the community, and thus prevent the effects which are so disastrous to the usefulness and happiness of many lives.

LIST OF EPIDEMICS OF POLIOMYELITIS.

Year	Author	Town or village	Country	Number of cases	Number of deaths	Mortality percentage
1881	Bergenholtz	Umea	Sweden	18	—	—
1883	Buchelli	Arenzano	Italy	5	2	—
1886	Cordier	Foy	France	13	4	—
1886	Leegard Oxholm	Mandal	Norway	9	2	—
1886	Strümpell		Germany	3	—	—
1887	Medin	Stockholm	Sweden	44	3	—
1889	Briegleb	Turingen	Germany	5	—	—
1893	Andre	S. Girons	France	4	—	—
1894	Brackett	North Adams, Massachusetts	U.S.A.	10	—	—
1894	Caverly, Macphail	Rutland, Massachusetts	,,	132	18	—
1895	Buchelli	Ravecca	Italy	17	—	—
1895	Medin	Stockholm	Sweden	21	—	—
1895	Pierracini	Florence	Italy	7	—	—
1896	Altman	Port Lincoln	Australia	14	—	—
1896	Bondurat	Alabama	U.S.A.	15	—	—
1896	Pasteur	Much Hadham	England	7	—	—
1896	Taylor	Cherry Field	Maine, U.S.A.	7	1	—
1897	Buchelli	Genoa	Italy	17	—	—
1897	Pleuss	Kiel	Germany	4	—	—
1897	Taylor	New York	U.S.A.	12	—	—
1897	Fabis	Conegliano	Italy	9	—	—
1898	,,	,,	,,	13	—	—
1898	Auerbach	Frankfurt	Germany	15	—	—
1898	Zappert	Vienna	Austria	42	—	—
1899	Leegaard	Bratsburg	Norway	64	2	—
1899	Mackenzie	New York	U.S.A.	30	1	—
1899	Newmark	San Joaquin	California	4	—	—
1899	Wickman	Stockholm	Sweden	54	4	—
1900	Painter	Gloucester, Massachusetts	U.S.A.	32	1	—
1901	Woods	San Francisco	California	55	—	—
1902	Private information	Pietermaritzburg	S. Africa	—	—	—
1903	Bergendal Wickman	Goteborg	Sweden	20	—	—
1903	Harbitz and Scheel		Norway	1,151	167	—
1906	Giersvold					
1903	Lorenzelli	Parma	Italy	26	—	—
1904	Ham	Brisbane	Australia	108	4	—
1904	Naunestadt	Fredrickshad	Norway	41	6	—
1904	Platou	Aafjorden	,,	20	6	—
1905	Giersvold		,,	436	69	—
1905	Wickman		Sweden	1,031	145	12·2
1905	Harbitz and Scheel		Norway	283	42	—
1906	,, ,,		,,	334	34	—
1906	Nyman Wickman	Smedjebacken	Sweden	50	5	—
1907	Lovett	Massachusetts	U.S.A.	234	—	—
1907	Starr	New York	,,	2,000	—	7
1907	Stephens	Victoria	Australia	135	—	4·5
1908	Davis	St. Paul's	U.S.A.	200	—	—
1908	Hoffmann	Heidelberg	Germany	36	—	—
1908	Löcker	Steyr	Austria	68	—	11·7
1908	Lovett	Massachusetts	U.S.A.	136	—	—
1908	Koplik	New York	,,	1,200	—	—
1908	Krause	Westphalia	Germany	100	9	9
1908	Treves	Upminster	England	8	—	—

List of Epidemics of Poliomyelitis—(*continued*).

Year	Author	Town or village	Country	Number of cases	Number of deaths	Mortality percentage
1908	Wiley and Dardem	Salem	Virginia, U.S.A.	25	3	—
1908	Zappert	Vienna	Austria ...	266	14	—
1908	Lindner and Mally	Steyr	,, ..	96	16	16
1909	Jogichess	St. Petersburg ...	Russia ...	26	—	—
1909	Lovett	Massachusetts ...	U.S.A.	923	—	—
1909	Netter	Paris	France ...	100	5	5
1909	Parker	Bristol	England ...	37	2	6
1909	Peiper	Pommern ...	Germany ..	51	6	11·7
1909	Fürntratt	Steiermark ...	Austria ...	433	—	13
1909	Potpeschnigg ...	,, ...	,, ...	433	—	13
1909	Reckzeh	Westphalia ...	Germany	500	—	18
1909	Shidler	Nebraska... ...	U.S.A. ...	200	—	7·5
1909	Spieler	Vienna	Austria ...	44	2	—
1909	Stiefler	Linz and Urfahr	,, ...	77	—	—
1910	Beard	Carlisle	England ...	34	5	—
1910	Currie and Bramwell	Tillicoultry ...	Scotland ...	5	—	—
1910	Garrow	Barrow-in-Furness	England ...	37	—	—
1910	,, ...	Maryport... ...	,, ...	13	2	—
1910	Hagenbach	Basle	Switzerland	19	—	—
1910	Public Health Report	... ...	U.S.A. ...	5,093	825	13·75

BIBLIOGRAPHY.

A good review of all the literature will be found in :—

Holt and Bartlett. *Amer. Journ. of Med. Sci.*. Philad. and New York, 1908. cxxxv, p. 647.

Job and Froment. *Rev. de Méd.*, Par., 1910, xxx, p. 162.

Lovett. "Infantile Paralysis in Massachusetts," 1910.

Wickman. "Beiträge zur Kentniss der Heine-Medinschen Krankheit," Berl., 1907, p. 141.

Additional Bibliography.

Buzzard, T. *Clin. Soc. Trans.*, 1896, xxix (Presidential Address, p. lvii); *Lancet*, 1898, i, p. 847.

Currie and Bramwell. *Brit. Med. Journ.*, 1911, ii, p. 363.

Idem. *Rev. of Neurol. and Psych.*, Edin., 1911, ix, p. 10.

Dixon, Fox and Rucker. *Brit. Med. Journ.*, 1911, i (Suppl., p. 44).

Flexner and Lewis. *Journ. Exper. Med.*, 1910, xii, p. 227.

Fürntratt. *Med. Blätt.*, Wien, 1910, xxxii, p. 453.

Garrow. *Brit. Med. Journ.*, 1910, ii (Suppl., p. 392).

Hagenbach. *Corr.-Blatt. f. Schweiz. Aerzte*, Basel, 1910, xl, p. 1218.

Jogichess. *Munch. med. Wochenschr.*, 1910, lvii, p. 2048.

Lindner and Mally. *Deutsch. Zeitschr. f. Nervenheilk.*, Leipz., 1910, xxxviii, p. 343.

Osgood and Lucas. *Journ. Amer. Med. Assoc.*, Chicago, 1911, lvi, p. 495.

Parker. *Brit. Med. Journ.*, 1911, i, p. 609.

Peiper. *Deutsch. med. Wochenschr.*, 1910, xxxvi, p. 398.

Potpeschnigg. *Archiv f. Kinderheilk.*, Stuttg., 1910, liv, p. 343.

Public Health Report. *Boston Med. and Surg. Journ.*, 1911, clxiv, p. 393.

Spieler. *Wien. med. Wochenschr.*, 1910, lx, p. 742.

Stiefler. *Med. Klin.*, Wien, 1910, vi, p. 1743.

Treves. *Brain*, 1909, xxxii, p. 285.

Vipond. *Brit. Med. Journ.*, 1911, i, p. 612.

DISCUSSION.

The PRESIDENT (Dr. Theodore Thomson) said those present would agree with him that, as a Section, they owed a vote of thanks to Dr. Ransome and Dr. Batten for their interesting papers. With regard to the first, many were apt to forget that the notification of disease, of which there was now so much, was not put to its full use. Those who were concerned in the administration of public health naturally thought primarily of its immediate practical utility. Notification of disease was required so that the infected cases could be sought out at once in order to ascertain, if possible, how the disease arose, and stop its spread. That being the first question, many were apt to forget that the information which had been acquired in those epidemics might be put to other uses which, although of less immediate service, might nevertheless result in acquiring valuable information. From that point of view they must be grateful to Dr. Ransome for calling attention to a matter which deserved the consideration of every epidemiologist.

He had listened to Dr. Batten's paper with much interest. The question there dealt with was a very difficult one from the epidemiological point of view, chiefly because it was difficult to ascertain how poliomyelitis was spread. One accepted the idea that the disease could be transmitted, and it had been shown experimentally that it could; yet those who inquired into outbreaks and prevalences of poliomyelitis were constantly face to face with the difficulty that again and again one found children with the closest exposure sleeping in the same bed with affected children, and yet nothing happened. And in another case something did happen, even in circumstances where exposure to infection had been slight or had not been known to have occurred. It was difficult to know how far poliomyelitis was infective. It might be that it was essentially a very infective disease, which was dependent for its transmission upon some intermittent agency, of which nothing was yet known, but which might only occasionally be present. But little was yet known about the propagation of epidemic poliomyelitis.

The papers dealt with subjects which were of great interest to the Section, and the discussion upon them was sure to be very interesting.

Sir SHIRLEY MURPHY agreed that the papers were of very great interest, and he sympathized with Dr. Ransome in his desire to see the returns made more use of for the purposes of study. There were a number of problems which, he thought, they would be able to have solved now that returns were being made from the Local Government Board, showing the number of notified cases in each week throughout the year. He supposed that one of the difficulties which would have to be contended with was that as all the returns were not sent in when they should be there would be gaps. But he did not doubt that efforts would be made to fill up those gaps, and after a time there would be more full records than were obtained in the first few months of the present year.

With regard to Dr. Batten's paper, it was of course possible to think of the groups of cases as being due to infection from case to case; but he thought there might conceivably be other explanations, although no doubt the idea of direct infection was that which suggested itself most prominently. The value of the paper was that it put together the whole story so concisely. The large amount of research which the paper represented might well be borne in mind by anybody who read it, because Dr. Batten had gone far and wide to collect his material. The College of Physicians had asked the London County Council to make an Order requiring the disease to be notified, and that at the instance of Dr. Batten. He did not yet know what action would be taken, but if that information could be got from the returns dealing with the large population of London there would be better opportunities of studying this disease than were afforded at present. In America so many outbreaks had been recorded that he was surprised that more evidence of similar occurrence had not been forthcoming in this country.

Dr. HAMER commented on the observation made on p. 192 of the paper. Dr. Ransome said: "Another point demanding research is the reason for the varying incidence of the poison of these disorders upon different parts of the bodily frame. Why, for instance, influenza should sometimes content itself by attacking only the upper air-passages, at others cause serious mortality from pneumonia, and at others, again, seize upon the brain and nervous system?" These remarks made by Dr. Ransome had perhaps a special interest for them on the present occasion: but, whether that were so or not, he (Dr. Hamer) thought that in some such direction as that indicated by Dr. Ransome there was an opportunity for much careful epidemiological study.

With regard to Dr. Batten's paper, he was much interested in the definition of poliomyelitis. Dr. Batten looked at the question in a very liberal way, and not only included cases which exhibited symptoms of meningitis, neuritis, &c., but even included a number of "abortive cases," which did not seem to exhibit any symptoms of disease of the nervous system at all. The facts relating to epidemics could be mentally focused in at least three different ways. One could focus them bacteriologically, and that was the common method nowadays. Search was made in connexion with each epidemic prevalence for associated micro-organisms, and the epidemic was classified according to the bacteriological findings. But that method failed in the case of poliomyelitis, because there were no commonly associated secondary or terminal invaders. Some other plan must therefore be adopted, and Dr. Batten had apparently started by classifying cases according to the part of the body upon which the stress of attack fell—i.e., the point of least resistance. He began with the anterior horns of the grey matter of the spinal cord; but he soon found that he must go beyond that, and so he included in his groups cases of meningitis, neuritis, and abortive cases. There was a third method, which Dr. Batten had really eventually adopted—namely, the epidemiological. All through the history of outbreaks variously classified as influenza, "new disease," and so forth, there were clearly discernible similar sequences of events, but with regard to symptoms, as had again and again been remarked, they had to deal with a veritable Proteus. At one time the respiratory system was attacked, at another the gastro-intestinal system, at yet another the nervous system. If one followed out the history of the outbreaks of pandemic influenza, it would be found that in the original invasions, following upon periods of quiescence, the disease behaved in a particular way, but that when it recurred it frequently presented aberrant symptoms affecting the nervous system or the gastro-intestinal system; and therefore he asked Dr. Batten whether he did not think that some of the outbreaks of poliomyelitis and cerebrospinal meningitis which had been recently described were not really part and parcel of outbreaks of influenza. Leichtenstern said that original primary outbreaks of influenza attacked the respiratory system as a rule, but that the "trailing epidemics" frequently attacked the nervous system. He was glad, as a result of reading Dr. Batten's paper, that he had cleared up a point which had been obscure in his own mind—namely, with regard to outbreaks of sweating sickness. There was good reason for looking upon these outbreaks as having really been prevalences of influenza. But the account given by Dr. Caius of the fifth outbreak of sweating sickness was at first sight a source of difficulty. If one imagined oneself, however, back in the sixteenth century and compelled to adopt the terminology of that period, the words of Dr. Caius corresponded quite well with Dr. Batten's conception of an outbreak of poliomyelitis, for the opening sentence of Dr. Caius ran: "Those whom the disease attacked had it in the following way: in some it first seized upon the neck or shoulder, in others the leg or the arm." Dr. Brorström had recently published in Leipzig a most interesting treatise in which he urged that epidemic poliomyelitis was really a manifestation of epidemic influenza. Much of Dr. Brorström's evidence was not convincing, because it merely related to demonstration of the presence of Pfeiffer's bacillus, a line of argument which he (Dr. Hamer) confessed he did not think finally conclusive. Dr. Brorström, however, gave numbers of groups of cases in families; in one instance, for example, three children exhibited what was called influenza with bronchopneumonic symptoms, and the two remaining children developed poliomyelitis. If they were all to devote themselves to collecting instances of a similar sort, some valuable information would be likely to result. The phenomena reported by Dr. Brorström occurred in sparsely populated country districts in Sweden. In London almost the whole population was saturated with the influenza virus, and hence largely immunized against its operation. That was not so markedly the case in country districts. There had been, it was true, outbreaks of cerebrospinal meningitis in towns such as Glasgow and Belfast, coincidently with notable prevalence of influenza in London, but by far the most noteworthy phenomena had been encountered in rural districts, for example in East Herts in 1905, and therefore it was in such places that one might most hopefully look for the solution of the riddle.

Mr. MAJOR GREENWOOD, jun., remarked that the statistics of plague showed how difficult it was to determine the way in which a disease spreads from epidemiological data. In bubonic plague it was generally agreed that the infection was not direct, from person to person, but mediated chiefly by the flea. Looking at the statistics for the Punjab, one found a seasonal curve of plague similar in shape to the curves which Dr. Batten showed of epidemic poliomyelitis; the seasonal prevalence was constant for any given district. In the Punjab it rose somewhat slowly in the early months of the year, reached a maximum in April or May, and fell more sharply than it rose. He had specially investigated the point when studying the differences in mortality in different villages. There were enormous differences in the mortality-rates of villages of the same size. It was important to determine how the spread occurred, because it was probably not from person to person. One would expect the spread of the epizootic disease, which is the substratum of the epidemic, to resemble the spread of a contagious disease, and that the human cases would conform to some regular distribution. But if one studied the maps in the last plague number,[1] one saw that it was impossible to *trace* a regular spread. There were scattered foci, and there was some evidence of slight spread round each focus, but no general law seemed deducible from the observations. Major Lamb and he had shown[2] that there was no evidence of plague in village communities remaining dormant, but that the facts were consistent with a re-importation of plague from season to season; beyond this it seemed difficult to go. It was very difficult, however complete the statistics might be, to get a satisfactory co-ordination of the facts as a basis upon which to found theoretical ideals. He mentioned these facts as an illustration of the care to be exercised in drawing conclusions, relative to methods of dissemination from spot maps and diagrams.

Fleet-Surgeon HOME thought the Section should be grateful to Dr. Batten for having brought the disease before them. It did considerable harm, and the public should know that it was to be regarded as infectious, as various causes were mistakenly attributed to illnesses due to the disease. He knew a case in Ireland in which a woman was convinced that her daughter got the disease because she was operated upon for adenoids, as, she alleged, the doctor had injured a nerve which went straight from the throat to the leg! The disease had been notifiable for a year in Hamburg. The disease seemed to be a definitely seasonal one, so one was apt to associate the periods of recurrence with meteorological conditions; yet they might be unrelated. Another disease which resembled it in that respect was typhus, though Americans had found that the virus of typhus differed from that of poliomyelitis in that it did not pass through a filter. An American last year showed that typhus could be communicated from monkey to monkey by means of lice. But they found the disease occurred only in Mexico in the upper plateaux, not in the coast zones. The conclusion was that the disease was carried by lice, which lived in the clothes of the person but were not kept at the temperature of the body. They found that lice at the temperature of the coast zone did not convey the disease. So the varying prevalence of typhus would not be due to temperature directly.

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[1] *Journ. of Hyg.*, 1910, x, p. 473 *et seq.*

[2] *Ibid.*, pp. 349-443.

but to the influence of the temperature on the carrier lice. When considering the question one should not be too ready to believe that when diseases had a definite relation to seasons of the year, that relation was entirely due to temperature; there might be other conditions which were involved.

Dr. BATTEN, in reply, demonstrated on a map of England the epidemics of the disease which had come under his cognizance, but he did not think it represented the number of cases. No doubt there were other epidemics which occurred throughout England in 1910. He had written to many Medical Officers of Health, but epidemic poliomyelitis did not come within their purview. There were very few cases which died of the disease; and for the epidemics of which he had obtained knowledge he was indebted to Medical Officers of Health who were also practitioners. The Cumberland district epidemic was investigated by Dr. Garrow, who was in practice at Maryport. When the disease was notified he hoped it would be possible to ascertain how widely it was distributed. With care in isolation he thought the disease could be largely controlled. Dr. Dalton's efforts in Cerne Abbas in isolating cases quickly stopped the disease. Dr. Hamer seemed to desire to suggest some relationship between the disease and influenza, but he (Dr. Batten) suggested that there was no such relationship. He admitted that influenza might affect the nervous system. There was influenzal meningitis, but one could do lumbar puncture and obtain fluid from the cerebrospinal canal which was distinctive, and on cultivation one could obtain the influenza bacillus. The cases did not resemble poliomyelitis. The Pfeiffer bacillus would not stand glycerination, and would not pass through the filter. One could take the spinal cord from a case of poliomyelitis, glycerinate it for six months, and then by injecting into the monkey reproduce the disease. That could not be done with the influenzal bacillus. He realized some of the difficulties there would be in regard to notification: but if the straightforward cases alone were notified much could be done to diminish the incidence of the disease among the population. What he was even more interested in was to find out, by notification and investigation, what was the cause, and how it was communicated from individual to individual. The clinician had gone as far as he could: he knew the clinical aspect and the pathology of the disease, but it was impossible for him to work out how the disease was communicated from person to person; that was the work of the epidemiologist, and of this Section of the Royal Society of Medicine.

Dr. RANSOME wrote, in reply, that he was glad that the object of his paper was so fully appreciated. He recognized that only persons devoted to epidemiology were likely to make full use of the "Returns of Disease"; but he had confidence that, in the near future, the energy of the officials of the Local Government Board would be turned in some of the directions now indicated. He thought that Sir S. Murphy's difficulty was a real one, but hoped that, as one small association in Manchester was able, for ten or twelve years, to secure returns without "gaps," so powerful a body as the Local Government Board would be able in time to get full and reliable records.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FOURTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE
SESSION 1910-11

LARYNGOLOGICAL SECTION

1805-1834
1907

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1911

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

Laryngological Section.

November 4, 1910.

Dr. P. Watson Williams, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

"ὁ ἀνεξέταστος βιος οὐ βιωτὸς."[1]

In electing me as President of the Laryngological Section of the Royal Society of Medicine I am conscious that you have paid me the highest honour that it is in your power to bestow. I wish sincerely I could feel equally conscious of being worthy to follow my two distinguished predecessors in office. Of one thing, however, I am certain: that is the pervading harmony which exists between the members of this Section, amongst whom criticisms are offered with only the friendly desire to help one another in points of difficulty, or to advance knowledge in this special branch of medical science, and it will be my constant aim to maintain this spirit of confraternity.

Coming from Bristol, where, as you are aware, we have developed a university, my thoughts have perforce been turned in the direction of educational matters, and, as a lecturer on diseases of the nose, throat, and ear, I have given much consideration to the position of our own speciality in medical education, which, in the interests of practitioners and of patients, leaves something to be desired.

We hear it said that this is a day of specialism, and specialism in medicine and surgery is thought to be a modern development. Yet we learn from the Ebers papyrus that in the thirteenth century, B.C., patients applying for relief to the medical temple at Thebes had to state their complaint, and that it was left to the principal of the medical staff to send the specialist best suited for the case. We find that the practice of surgery was distinguished from medicine even earlier than the thirteenth century, B.C., and it is recorded that in Egypt each physician treated a single disorder and no more, some devoting themselves to the eye, others to the teeth (they made artificial teeth, and some have asserted that they have found traces of gold stoppings in the mummies), and others again devoted themselves to disorders of the intestines, while others, again, were obstetric physicians, called in by midwives to the difficult cases.

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[1] "The life without examination is not worth living."—Plato.

In our own land, when in 1460-1 a charter was granted to the Barber-surgeons, and when the Edinburgh Charter was obtained in 1505, surgery in a humble way started on its career divorced from medicine, and under the paternal fostering of the Colleges of Surgeons became a science and art distinct from internal medicine. We find, in consequence, that in Great Britain, and later also in Ireland, the field of medical science was long occupied by rival schools of medicine and of surgery. Hence arose our academic distinction between medicine and surgery peculiar to this country. For in practically every European nation, except our own, a certain course of study is laid down, and State examinations must be passed by all practitioners: if a degree is obtained, it is always that of doctor of medicine; there is no distinct qualification in surgery, and it is the same in Canada and the United States. After qualifying on a common basis the graduate is free to follow his own bent, and if he seeks to become a pure physician, surgeon, or specialist, he must establish himself as such, not by passing further examinations, but by his clinical and research work. Thus, while the degree of M.D. serves for all physicians, surgeons, and specialists alike, their subsequent differentiation is clinical and not academical, as with us.

We alone carry examinations quite beyond the qualifying stage, to the higher standards of medicine and surgery, and he who aspires to the charge of a teaching clinic and consulting practice must, as a rule, first obtain a higher and special qualification in medicine or in surgery respectively, involving a further and more advanced course of study. Though not without drawbacks, this has the advantage of insuring that the more highly qualified physician or surgeon has received, over and above what is necessary for qualification, a very thorough grounding in current medical or surgical science and all that immediately pertains to it, and the custom has coincided with an increasingly high standard of original medical and surgical work in this country. The plan answered as long as the whole range of medicine on the one hand, or of surgery on the other, could be mastered by one individual; but with advancing knowledge pure medicine and surgery are no longer the only specialities, and with the growth and development of newer special branches the objections to our English system appear. Either we must continue our unfortunate custom of mentally placing each speciality under the category of medicine or of surgery, and of demanding as the *sine qua non* for teaching clinics the corresponding medical or surgical qualification, although it may have no special bearing whatever on the specialities in question (in my own hospital both the laryngologist and the obstetrician of the future will be distinguished in that they will have to possess all the higher qualifications, both of the physician and of the surgeon), or we must be consistent and offer opportunities for a qualified practitioner taking his higher qualification in his speciality, after a course of post-graduate study and practice *parallel* to that required for the physician or the general surgeon.

No one could be more opposed to too early or too narrow specialization than I am, for it is certain that a sound up-bringing in the whole range of general medicine and surgery is the only safe foundation for any special branch. But the tendency is to make another speciality (general surgery) the academic test of fitness in laryngology, whilst medicine, and even systematic training in our department, is made a secondary consideration, with a consequent danger of our becoming too exclusively surgical. In no other speciality would this be more deplorable than our own, for which, I believe, general practice is as good taking-off ground as pure surgery. We want contributions from all points of view, and it should always be open to every practitioner to specialize in any direction according to his opportunities for so doing.

To our fellow countryman, Robert Liston,[1] belongs the honour of publishing in 1837 directions for laryngoscopy which he recommended and employed for observing diseases in the larynx, eighteen years before Manuel Garcia published his epoch-making observations, to be followed by those early giants in our speciality, Türck and Czermak. Many of those who are here to-day have had the privilege of personal acquaintance with the slightly later but hardly less distinguished pioneers when laryngology was still in its infancy, and many members of this Section have not only witnessed, but have generously contributed to its exceedingly rapid growth and development. Indeed most of us have had to acquire our knowledge to a large extent first hand, and it has not always been easy to keep thoroughly in touch with the rapid advances all the world over.

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[1] Liston, "Practical Surgery," London, 1837, p. 350.

But things are very different from what they were when I started practice, and hardly any individual is capable of a complete mastery of the whole range of rhino-laryngology; and if otology be superadded, the ground is so extensive that, without devoting his whole time and attention to these subjects over several years, no one can hope to be a scientific expert throughout such a large territory.

It is no small matter to acquire an accurate knowledge of anatomy, pathology, and—in so far as they bear on the diseases of the nose, throat, and ear—of neurology and general medicine; the ætiology, diagnosis and treatment of diseases of these, together with a fair acquaintance with the various methods of treatment and operations which have been or are employed, as well as the acquirement of the operative and diagnostic technique, including the trachea and œsophagus; for it is to the honour of our speciality that our laryngological colleagues abroad—where the development of specialities has been better fostered—have originated and made practical tracheoscopy, bronchoscopy, and œsophagoscopy.

I think no one who is not ignorant of the work that is being done by laryngologists and otologists can deny that, as an addition to that sound general education in the principles of medicine and surgery for pass qualifications, all this involves as much, if not more, study as is demanded of those who present themselves for a Fellowship of the Royal Colleges of Surgeons, or a Mastership of Surgery at a university, or an M.D. in obstetric medicine, in State medicine, or in pathology, or an M.S. in dental surgery. All these degrees in special branches may be acquired at the London University, and I sincerely hope that on similar lines a graduate may ere long be able to take a higher degree in laryngo-otology. Surely the practitioner who so qualifies himself for work in laryngo-otology should have the opportunity of being tested by examination and of obtaining his higher degree in this department. He would be better equipped for making the utmost use of his riper clinical experience than by devoting himself, instead, to acquiring an exact knowledge of the anatomy of the whole body and the whole range of advanced medicine or general surgery, at the expense of systematic liberal study of his speciality and all that pertains to it. To test such study by examination too late in life would be a grave misfortune. Already the tendency in this country is to continue medical examinations unduly, and thus to trench too far on the precious years of early adult life, when a man's best original ideas are germinating and should be cherished and allowed spontaneous growth, instead of being trammelled and choked out of life by scientific pedagogy; as though Plato had urged that "life without examinations is not worth living"!

We gather fruits in autumn, but life's spring is the sowing time. It is worse than useless to spend too long a time in tilling and preparing the ground, and still worse to disturb the roots by examination when we should be looking for early blossoms of research. There is more truth in the world-renowned Oslerism than is generally suspected, and, particularly in the field of science, it is an open question whether, after the age of forty, one's mind ever breaks new ground, in the sense of being original.

Two Specimens of the Quadrilateral Cartilage (Nasal Septum) showing Fenestræ; Removed by Submucous Resection.

By Dan McKenzie, M.D.

One of the specimens was removed by Dr. Horsford, the other by the exhibitor. They are interesting for two reasons: first, because the presence of a window in the septal cartilage, with intact mucous membrane, renders the operation of submucous resection more difficult, unless the possibility of this condition is remembered; secondly, because the spontaneous production of septal perforation from ulceration is favoured by the presence of such windows. In the exhibitor's experience, acute perforating ulcer of the nasal septum penetrating not only mucous membrane but also cartilage is extremely rare. As to the cause of these windows, the opinions generally seem to be that they are sometimes developmental and at other times are induced by the long-continued bilateral pressure upon the cartilage of mucous scales or crusts.

DISCUSSION.

Dr. Horsford, referring to the operation on one of the cases, said he did submucous resection of the septum, and did not know there was a perforation in the cartilage. The mucous membrane on both sides was intact, but that on the convex side was very thin and was difficult to separate. Afterwards he found he had separated the mucous membrane on one side from the mucous membrane on the other; therefore when removing the cartilage he found it contained the excavation, as in the specimen shown. The margins of the hole were bevelled, proving that it was not cut or broken or torn by accident. He did not know what Dr. McKenzie meant by the theory of long-continued pressure of cartilage by crusts or scales. It was an ordinary rhinitis sicca, in which crusts had formed, and the pressure of the crusts produced ulceration.

When they were thrown off and the ulceration healed new crusts formed, and in that way the ulceration spread gradually through, so that when the cartilage corresponding with the ulcerated area was thrown off, the base of the new ulcer became the under-surface of the opposite muco-perichondrium, which becomes covered with mucous membrane in the ordinary way.

Mr. CLAYTON FOX thought the fact that the mucous membrane was healthy on both sides negatived the idea of pressure. He thought the perforations were congenital deficiencies in the ethmo-vomerine plate.

The PRESIDENT (Dr. Watson Williams) said that it was recognized that there might be a large dehiscence of the septum, congenital in origin, but it was when perforations of the cartilage were not associated with complete perforation that they became so important, for it was easy for an operator to be blamed for having made a perforation during a submucous resection when it was really due to a congenital deficiency. He did not understand how pressure on the cartilage by mucous crusts could produce the condition shown by the specimens.

Dr. DAN MCKENZIE, in reply, said that with regard to the ætiology of the fenestræ, their dependence upon a developmental error was well recognized by the authorities, as he had remarked in his notes. With regard to the action of crusts or scales, it seemed to him clear that anæmia and ischæmia of the mucous membrane could induce thinning and finally perforation of the cartilage without necessarily destroying or damaging the mucous membrane itself. Cartilage was devoid of blood-vessels and depended for its nutrition entirely upon the transudation of plasma, a process with which even slight pressure would readily interfere. Another point he wished to raise was that such fenestræ must predispose to the formation of pathological perforations of the septum. This was supported by some cases of submucous resection reported in the transactions of one of the American societies,[1] in which perforation of the septum had occurred, not at the operation but several months later, the fenestra in the cartilage made by the surgeon being transformed into a perforation of the septum, either by the pressure-ulceration of dried secretion or by finger-nail traumatism.

Case of Subglottic Stenosis after Tracheotomy.

By HAROLD BARWELL, F.R.C.S.

THE patient, a woman, gives a history of diphtheria in childhood, when tracheotomy was performed in great haste for urgent dyspnœa. Since then the voice has been affected, but there does not appear to be any serious interference with respiration. The "tracheotomy" was evidently performed through the cricoid cartilage, and a thick cicatricial web, with a posterior aperture, can be seen below the cords.

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[1] *Journ. Laryng., Rhin. and Otol.*, 1910, xxv. pp. 486-91.

Mr. BARWELL said he did not think the case required treatment; the patient only came for hoarseness, and the stenosis was not so severe as to be dangerous.

Scleroma of the Nasopharynx in a Polish Woman.

By StClair Thomson, M.D.

THIS patient was shown before the Laryngological Society of London in February, 1907, and her case is described in the *Proceedings*, vol. xiv, p. 65. It seems that for about ten years she has had increasing difficulty in nasal respiration. Seven years ago an operation was performed in Dr. Heryng's clinic in Warsaw, with some relief. When exhibited in 1907 there was a red, fleshy diaphragm extending from the base of the soft palate upwards and backwards to the roof of the nasopharynx. Through an oval opening in the centre of this fleshy membrane one could see a small part of the posterior edge of the septum. Under chloroform this fleshy membrane, which was found to be of almost cartilaginous-like hardness, was plucked away, partly through the mouth and partly through the nose. Nasal respiration was completely restored and the patient remained quite comfortable for two years. In March, 1910, the membrane was again seen to be re-forming, and the condition now is very like what it was in February, 1907. At the operation two years ago it was difficult to get a satisfactory piece of growth for examination, as it all came away in shreds. But examination by Dr. Emery failed to reveal Frisch's bacillus.

Dr. STCLAIR THOMSON said he believed only two other cases of the kind had been shown in this country—the first by Sir Felix Semon and the other by Dr. Dundas Grant. He made a mistake about Frisch's bacillus, as that had been given up as the causative organism; it was recognized now to be Friedländer's bacillus which was frequently found in this disease. He understood, however, that pathologists now looked for Mikulicz's cells as characteristic. Dr. Emery had made another search, but could find no such cells. He showed the case before operating on it because, although the disease did occur as limited to the nasopharynx, the present was not altogether a typical case. This post-nasal membrane was very cartilaginous, and was thoroughly removed, but slowly returned after three years. He proposed to operate and get some carefully selected specimens, and report later.

Fungating Carcinoma of Œsophagus. Disappearance of Fungation and Ulceration after Treatment by Radium.

By WILLIAM HILL, M.D.

MALE, aged 49. Note on admission, June 18, 1910: Gradually increasing dysphagia and occasional odynphagia for six months; for last six weeks there has been considerable regurgitation; during last two weeks has only been able to swallow fluids and with difficulty; is losing weight.

Patient was examined with œsophagoscope under cocaine and also under chloroform; gullet dilated, and pieces of mutton, partaken of three weeks previously, removed by lavage; ulcerating and fungating stricture found 28 cm. from incisor teeth; bled easily and profusely. Tight but short stricture dilated with graduated bougies and exhibitor's styletted modification of Symonds's long intubation tube inserted for ten days; swallowing improved. On July 9, 50 mg. pure radium bromide was applied for thirteen hours. Two months later radium was again applied for twenty-one hours. There has since been progressive improvement in swallowing, which is now quite normal. When examined on October 8 there was no sign of fungation or ulceration, no marked stricture, merely scarring of mucosa. The diagnosis was confirmed by microscopical examination (squamous epithelioma), but whilst under radium treatment all endoscopic evidences of malignancy have disappeared. The patient has put on 2½ st. in weight.

DISCUSSION.

Mr. BARWELL asked if Dr. Hill would report the case later, as it was almost impossible to talk about it at present; there seemed no growth, but observation over a longer period was required.

Dr. H. J. DAVIS wished to ask Dr. Hill whether radium was of use where nothing could be passed through the stricture. That day he had seen, for the second time, a very instructive case—that of a gentleman, aged 35, who lived a healthy country life, but could not now swallow his food. Several doctors had seen him, in London and elsewhere, who stated there was nothing wrong with deglutition, that the condition was functional and imaginary. The tonsils were much enlarged. That day he was X-rayed and given a meal of gelatine and bismuth by Dr. Morton. The bolus was held in the pharynx for some time, and when the act of swallowing was attempted it was seen to shoot to the back of the sternum and stop there. It was evidently not a functional trouble. He was given some water to swallow, but it made no difference. He (Dr. Davis) therefore took the patient to Mr. Butlin, under the impression that there might be a pouch. Mr. Butlin had been unable to pass the finest celluloid bougie, and he inclined to the opinion that in spite of his youth he had malignant disease and no pouch. He suggested that Dr. Hill should treat him with radium, but as nothing could be got through the stricture he (Dr. Davis) wished to know whether the case was a suitable one for radium treatment.

Dr. StCLAIR THOMSON asked whether Dr. Davis's case was examined with the œsophagoscope. He suggested that blind probing of an imperforate stricture of the œsophagus should be given up; the use of the newer instrument was easy, and they all knew of the disasters which might arise from the older methods. He saw Dr. Hill's case, and moved the œsophagoscope up and down, but could not see anything now to suggest a malignant growth.

Dr. HILL, in reply to Dr. Davis, said that the dysphagia of tight malignant strictures was almost invariably and markedly relieved by radium, and in three out of fifteen cases the objective evidences of cancer growth had now disappeared, temporarily at all events. In several cases that had succumbed to the disease they had "died cured" of their dysphagia after large and prolonged doses of radium applied in conjunction with Dr. Finzi.

Carcinoma of Œsophagus. Dysphagia Treated by a New Form of Intubation Apparatus.

By WILLIAM HILL, M.D.

MALE, aged 49. History of five months' dysphagia; much frothy mucus expectorated; cough; abductor paralysis. Œsophagoscopy showed fungating growths on anterior and posterior wall; tight stricture 28 cm. from teeth; portion removed for microscopy (epithelioma). After great difficulty and delay, the stricture was threaded and dilated up by graduated bougieing, and exhibitor's styletted modification of Symonds's long rubber intubation apparatus was inserted. Patient at first fed entirely through tube, but can now swallow soft food beside it; mucorrhœa much less; patient much less asthenic. The disease was considered too advanced, and the actual strictured area too long, for radium treatment, but the method of permanent intubation employed does away with the necessity of resorting to gastrostomy.

Apparatus with dental attachment by Mr. John Hall was exhibited.

A Case illustrating "Dents de Scie" of the Vocal Cords.

By W. JOBSON HORNE, M.D.

THE patient, a woman aged 40, came under observation towards the latter part of last August through huskiness of the voice of twelve months' duration. She is also the subject of middle-ear suppuration. The huskiness was sudden in onset; at first passing off, then suddenly recurring, and subsequently it became constant. Formerly she sang in chorus, taking part in oratorios up to twenty-five years of age. Upon coming to London she discontinued singing. The patient was married at the age of twenty-nine, there is no family history of tuberculosis, and, as far as has been ascertained, there is no clinical evidence of that disease.

The condition which the larynx presents is a little unusual, and has been figured in text-books and described under the above title. In the present case the lesion is strictly confined within the sesamoid cartilages—that is to say, to the middle portion of both cords, the part covered by squamous epithelium. The condition is symmetrical; and, broadly speaking, may be described as a thickening of the middle third of the cord with serrating of the edge. There has been no local treatment apart from rest of voice, and the condition has somewhat subsided since it was first observed. The question arises whether this condition is rightly described as being of a tuberculous nature.

DISCUSSION.

Dr. HORSFORD did not agree with the description that there was thickening of the middle third of the cord with serrating of the edge. He considered there was a soft growth on the surface of both cords, so soft that it could be blown up and down during breathing. The growths suggested some form of soft fibroma, or even papilloma. He thought they should be treated actively by forceps, tearing it away from both cords. He did not think it would tear away the healthy mucous membrane, and it would soon heal and produce a good result.

Mr. CLAYTON FOX thought the condition was typical of the description of Virchow's pachydermia verrucosa. She had been a singer, and the condition occurred at the point where the cords chiefly came into apposition. He thought improvement would occur after rest, and the application of a 6-per-cent. solution of salicylic acid and alcohol.

Dr. STCLAIR THOMSON did not think the title was applicable to the case. The thickenings were more or less symmetrical: one was larger than the other, and into the larger one the small one fitted. He recently saw a similar case in a colleague, and it was suggested it was early malignant disease with infection of the opposite cord. Wassermann's reaction was tested and was negative. Though the chest was passed by numerous physicians, the von Pirquet reaction was tried, and there was a typical skin response. A few days later tuberculin was injected, and the result was a temperature of 103° F., and a marked general reaction, with the re-awakening of the von Pirquet reaction spot. He suggested the present case should be tested further for tuberculosis.

Dr. DUNDAS GRANT said from the title given one expected to see a different appearance. He regarded these as only multiple fibromata. No one could tell beforehand that they were not tuberculous, nor until a portion had been removed for examination. Dr. Horne would have no difficulty in bringing about much improvement, probably first by removing a portion of the growth at the edge of the cords, and afterwards applying the galvano-cautery. He did not think it was likely to be benefited by salicylic acid. It did not answer to his idea of pachydermia.

Dr. JOBSON HORNE, in reply, stated that the appearances presented in the larynx were uncommon, and for that reason he had exhibited the case prior to removal by operative treatment. Moreover, the case was one that would provoke discussion. In his descriptive statement he had abstained from expressing an opinion. The term "Dents de Scie," when applied to a case presenting appearances such as the present one, was misleading. The term was associated with tuberculous disease of the larynx, and, in his opinion, tuberculosis was not a factor in this case. The disease was confined to that part of the larynx usually immune against tuberculosis except by continuity. The condition, to his eye, did not suggest pachydermia verrucosa laryngis. The growth on the cords was not suggestive to himself of a fibroma; it was too soft and friable. It was his intention to clear the edges and surfaces of the cords with a small spoon-shaped, double-cutting curette, and subsequently to apply astringents. Underlying the circinate growths some thickening of the cords would be revealed, and he hoped it would subside without vigorous treatment.

Ulceration of Soft Palate.

By H. FITZGERALD POWELL, M.D., and G. W. BADGEROW. F.R.C.S.

FEMALE, aged 17, suffering from a granular-looking superficial ulceration of soft palate, extending forward on to the hard palate. At the junction of hard and soft palate a perforating ulcer was observed, through which a probe could be passed. No rash to be found on body on first examination, but an iodide rash had appeared since. No history of syphilis. The case was thought to be tuberculous.

DISCUSSION.

The PRESIDENT said he was under the impression that the case was one of lupus.

Dr. JOBSON HORNE expressed the opinion that the disease of the soft palate was not due to tuberculosis, and subsequently, in reply to the President's question to him as to what it was due to, considered that this might be arrived at by a process of elimination.

Dr. FITZGERALD POWELL replied that his view was that the case was tuberculous, and that was strengthened by the fact that antisyphilitic remedies had no effect upon it. She had previously had treatment at Brussels, when the lower edge of the palate and the uvula were treated and seemed to have healed. He was desirous of having the opinion of the members, and he thanked them for having given it.

Tumour of Post-nasal Space.[1]

By H. FITZGERALD POWELL, M.D., and G. W. BADGEROW, F.R.C.S.

MAN, aged 27. Tumour of post-nasal space seen pushing soft palate forward and involving palate; growing from lateral walls of nasopharynx and infiltrating lining of same; extending down to tonsil, involving tonsil. Removed by operation, along with portion of palate involved and tonsil; good recovery. Obturator, closing opening, fitted by Dr. Pare. No recurrence up to the present.

Dr. PEGLER reported that the section exhibited at the previous meeting was undoubtedly one of lympho-sarcoma.

Tonsillolith in Left Supra-tonsillar Fossa.

By C. A. PARKER, F.R.C.S.Ed.

The patient was a man, aged 24, with a tonsillolith in the left supra-tonsillar fossa pushing forward the soft palate. History of discomfort in the throat for nine months.

DISCUSSION.

Dr. DUNDAS GRANT said the case was a beautiful one of calcareous concretion in the tonsil.

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[1] Shown at the last meeting; see *Proceedings*, 1910, iii. p. 160.

Mr. PARKER, in reply, said he proposed to remove it. In the early days of the Society he had shown a calculus removed from a somewhat similar case.[1] In that case, however, the calculus had ulcerated through the soft palate to the right of the base of the uvula and appeared to be in the substance of the soft palate.

Small Tumour of Soft Palate.

By G. W. BADGEROW, F.R.C.S.

THE patient, aged 27, a teacher, noticed about eighteen months ago a small, whitish lump in the soft palate, about the size of a small pea; it was now becoming darker in colour and gradually getting a little larger.

DISCUSSION.

Dr. JOBSON HORNE suggested removal of the tumour. He could not detect extension beyond the soft palate in the post-nasal region. Similar cases that had come under his notice had been reported to be of the nature of sarcoma when examined under the microscope, but they had not been of a very malignant form, as proved by the absence of recurrence.

Dr. DAN MCKENZIE said that two years ago he showed a patient with a somewhat similar tumour of the palate which after removal proved to be an endothelioma. There had been no recurrence so far.

Lingual Thyroid; Thyroid Gland in Neck not Palpable.

By W. G. SPENCER, M.S.

FEMALE, aged 23. Swelling first noted by dental surgeon four months ago. Had caused no appreciable disturbance. Hemispherical swelling, about 3 cm. in diameter, over situation of foramen cæcum, covered by the normal epithelium.

The PRESIDENT remarked that at least one case had been recorded where such a growth had been removed, and in the absence of thyroid gland in the neck the operation of removing the lingual thyroid resulted in the production of myxœdema.

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[1] *Proceedings*, i, p. 50.

Case for Diagnosis and Suggestions for Treatment.

By T. Jefferson Faulder, F.R.C.S.

R. P., female, aged 23, has a swelling at the left side of the nose and inner canthus of left eye. It first attracted her attention about three years ago by causing pain. She now has intermittent headache and, on stooping, feels giddy. She suffers from nasal obstruction; the mouth is always dry and offensive in the morning, when she coughs up some "phlegm and bad blood." This condition improves during the day. There are no ocular symptoms except some epiphora and irritation.

The swelling in the left inner canthus and the outward displacement of the left eye are easily seen. The swelling corresponds in position to the lamina papyracea of the ethmoid bone and the tendo oculi is stretched across its front. It is hard, rounded, sessile, and slightly tender. There is a swelling of similar character in the antero-superior part of the left nostril. The post-nasal space is normal in all respects except for dryness of the mucosa. Transillumination of antra equal and remarkably bright; of frontal sinuses negative, or, if anything, somewhat against the right sinus.

With the case were shown:—

(*a*) Photographs of the patient at the ages of six months and fifteen years, both showing asymmetry of the orbits.

(*b*) Skiagrams.

(*c*) An orbito-nasal (ethmoidal) exostosis removed from a female patient, aged 20, who had proptosis, diplopia, and defective vision ($\frac{3}{60}$) of right eye, also right nasal obstruction. The tumour was visible in the nasopharynx, blocked up the right nostril, and was shown, by skiagrams, to extend in the orbit as far back as the optic foramen. It consists of cancellous bone with a covering of dense eburnated bone. This patient has recovered her vision and remains well ten months after operation.

Dr. StClair Thomson said the Section would be glad to hear the conclusion later. It looked like osteoma or enchondroma. He suggested to Mr. Faulder that he should try Moure's operation. It was easy to carry out, gave a good view, and was followed by practically no deformity.

Case for Diagnosis.

By NOEL BARDSWELL, M.D.

MR. A., aged 32. History of pulmonary tuberculosis for the past three years. No specific history. History of hoarseness for the past eighteen months; was treated during this period in a sanatorium in Australia for laryngeal tuberculosis. The patient has chronic tuberculous disease in both lungs considerably arrested. In the larynx the left cord is absolutely fixed in the middle line, apparently with infiltration in front of the left arytænoid joint. and in the ventricular band. There is no ulceration. On the tongue there is ulceration strongly suggestive of tertiary disease. The Wassermann reaction (done twice during the past ten days) is negative. Tubercle bacilli are present.

Treatment: The patient has, as yet, been treated only by general sanatorium treatment and vocal rest.

DISCUSSION.

Mr. SPENCER said he did not think there was enough to call ulceration on the surface of the tongue. There seemed to be superficial glossitis, due largely to decomposition of toxins in the mouth. It was of the type called a geographical rash, commonly seen in boys after scarlet fever. It was not necessarily specific. Wassermann's reaction, having been twice negative, was fairly exclusive of syphilis.

Mr. BARWELL said he did not think the case was one of active tuberculosis at the present time. It might have been active tuberculosis twelve months or so ago. It would be interesting to hear the opinions of other members.

Mr. PARKER said he thought there were cases of tubercle of the larynx in which the process arrested itself and the tuberculous infiltration became a cicatricial mass without breaking down or ulcerating. He had shown a case before the Section with marked interarytænoid thickening which was thought by all to be pachydermia, yet three years previously the patient had undoubtedly had tuberculous infiltration, and tubercle bacilli had been found in the sputum. This was an example of what he meant, and he thought the present case was of the same nature. The active tuberculous process had been arrested, leaving cicatricial tissue, which was causing fixation of the left side of the larynx.

Dr. STCLAIR THOMSON said he saw the patient a few weeks ago, and thinking he had only been hoarse a short time, he concluded it was syphilitic. But the negative Wassermann reaction made him reconsider it, and he now thought it was what Mr. Parker said. Dr. Thomson had shown two cases before the Section in which spontaneous recovery took place in the larynx without local treatment, and in which both arytænoids were so fixed that the patients had to wear a tracheotomy tube permanently.

Dr. BARDSWELL, in reply, said he showed the case to Dr. Percy Kidd, who thought it was tuberculous. Dr. Kidd had seen cases go on four or five years in a chronic way without ulcerating. Dr. Bardswell thought that the evidence favoured a diagnosis of tuberculosis.

Case of Aphonia for Diagnosis.

By CYRIL HORSFORD, F.R.C.S.

PATIENT, a married woman, aged 46, suddenly lost her voice in July last, and all attempts to restore it have failed. She gives history of two previous attacks, both of which were cured spontaneously. For the first she attended the Throat Hospital, Golden Square, under Sir Morell Mackenzie for twelve months. In the chest there are signs of chronic phthisis in both apices. The laryngoscope shows paresis of the arytænoideus and swelling of the ventricular bands, which completely hide the anterior two-thirds of the vocal cords, and by their approximation they replace the latter during phonation. Is the case purely functional?

DISCUSSION.

Mr. CLAYTON FOX thought it was tuberculosis of the larynx. There was subglottic thickening on the left side of the cord, and the paresis could be easily accounted for by infiltration of the arytenoideus and other adductors; it was probably entirely myopathic in origin.

Dr. HORSFORD, in reply, said he brought the case for diagnosis. There was a functional element, and there might also be tubercle of the larynx, superadded on the laryngoscopic appearances. He thought the ventricular bands were infiltrated with tubercle and were enlarged accordingly. He intended to treat the case by plunging a cautery into the bands.

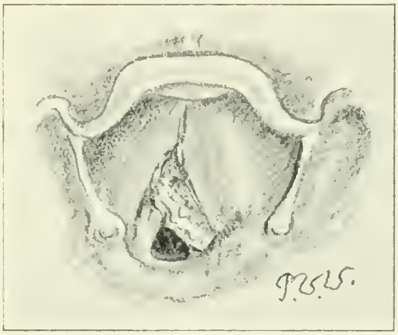
Case of Malignant Neoplasm of the Larynx.

By P. WATSON WILLIAMS, M.D.

FEMALE, aged 40, single. She had complained of hoarseness, on and off, for at least two years, but had become much worse during the last six months, and for five weeks had been short of breath on exertion.

She had had no pain, and a doctor whom she had consulted two months before had ordered inhalations under the belief that she had laryngitis. On October 27 she was seen by Dr. C. Bodman, who, suspecting the serious nature of her complaint, sent her to Dr. Watson Williams on October 29. There was no family history of tubercle, and the patient's lungs were found to be sound. No night sweats, but was losing flesh. No history of syphilis. She was ordered iodide of potassium, 45 gr. daily, and a von Pirquet test for tubercle done with negative result.

Laryngoscopic examination: A reddish granular neoplasm growing from the left ventricle of Morgagni completely conceals the left vocal cord and the anterior three-fourths of the opposite cord. The left arytænoid is immovable and left ventricular band is swollen, and a smooth swelling extends beneath the left aryteno-epiglottic fold so as to cause obvious tumefaction in the external surface corresponding to the left pyriform fossa. Sir Felix Semon, after examining the larynx, agreed with Dr. Watson Williams that the neoplasm was probably malignant, that a portion of the growth should be removed, and, if it proved on histological examination to be malignant, that a thyrochondrotomy should be performed and the growth removed, if not then found to be too extensive.



Malignant neoplasm of larynx.

Large Fistulous Opening into Anterior Wall of Left Maxillary Antrum.

By James Donelan, M.B.

Man, aged 40: obscure history of venereal disease twenty years ago. Three years ago, when removing with his fingers some loose, but sound, teeth from *right* upper alveolus, produced a fistula into maxillary antrum. Was treated for a week at Middlesex Hospital. Says fistula closed of itself soon afterwards. Three months ago found loose teeth in *left* upper alveolus. Removed them with his fingers, making similar fistulous opening into left antrum. This was followed by discharge of blood, pus, and bony detritus. Came to Italian Hospital in September; had very large oval opening into a foul antral abscess. A large portion of the anterior wall of antrum, and of the hard palate, was found to be absent. No exposed bone could be detected. His patellar reflexes are diminished, especially on right side, but he shows no other evidence of tabes, in which necrosis or absorption of upper jaw often occurs. The local condition improved greatly under anti-syphilitic treatment, and an attempt was made to close the fistula by sutures, a large counter-opening being made at the same time into the inferior meatus. The result was only partially successful, probably owing to persistence of constitutional condition. It has been suggested that an attempt should be made later on to restore hard palate by transplantation of bone.

Dr. Donelan said he would bring the patient up again when further treatment had been tried.

Report on Mr. Tilley's Specimen of Growth from the Nose.

This specimen was shown at the meeting in March, 1910.[1] The report of the referee of the Morbid Growths Committee—Mr. S. G. Shattock—states in conclusion: "The lesion may be regarded as a nasal polypus into which extensive hæmorrhage has for some reason occurred. The macroscopic appearances are in accord with this conclusion."

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[1] See *Proceedings*, 1910, iii, p. 96.

Laryngological Section.

December 2, 1910.

Dr. P. Watson-Williams, President of the Section, in the Chair.

The Origin of Cancer in the Larynx.

By R. H. Scanes Spicer, M.D.

A series of coloured and black-and-white engravings which have been published (1885-1910) by British laryngologists (M. Mackenzie, F. Semon, L. Browne, P. McBride, P. Hutchinson, Watson-Williams, and others), together with unpublished sketches from his own case-books bearing on the origin of cancer in the larynx, also numerous coloured drawings and diagrams after Quain, Luschka, Elsberg, and Killian, relating thereto, shown at the previous meeting of the Section, were again placed on the table.

An epidiascope demonstration of coloured lantern-slides of photographs of frozen sections after Braune, illustrating the topographical anatomy of the rigid resistances of the larynx and adjacent parts, and of other slides bearing on the problem, was given.

The object of this demonstration was to present some of the evidence available for the conclusion that the sites (*a*) of initiation, (*b*) of maximum development of laryngeal cancer, are determined by the mechanical interaction of the rigid resistances of the laryngeal endo-skeleton on the one hand and certain cell constituents (e.g., epithelial and connective) of the soft tissues (e.g., mucous membranes) on the other.

The larynx, from the ease with which it can be inspected, is peculiarly advantageous for the clinical study of the origin of internal cancer. It has the disadvantage that laryngeal cancer is comparatively rare. In both these respects a marked contrast appears between the larynx and the stomach. According to the American mortality returns, in every hundred cancer deaths 43 are stomach to 1 larynx, while in the English in every hundred cancer deaths 21 are stomach to 1·8 larynx (Roger Williams). In the seventieth annual report of the Registrar-General for England and Wales the total number of deaths from larynx cancer per year (seven years' average) is only 313. Again, Sir Felix Semon's series of 246 cases took him over thirty years to collect, at the rate of 8 per annum. As but few cases, then, are likely to be seen by any individual laryngologist at a sufficiently early stage to be watched throughout so as to test inductively the suggested causal relationship, it is the collective observation of many specialists directed to the point at issue that is required. This should soon confirm or disprove the alleged connexion. Deductive reasoning on the observations actually made will be, however, pre-eminently necessary, since the real origin is but seldom on the *free* surface of the mucous membrane, as seen in the laryngoscope, but somewhere on its *deep* surface, and perhaps in a hidden laryngeal recess.

If in an animal with an endo-skeleton—i.e., a vertebrate—certain muscular actions are, from any cause, habitually exaggerated, the corresponding rigid resistances and soft tissues are reciprocally influenced in such a way as to cause undue stress, strain, or friction of those soft tissues—i.e., to produce a chronic, intrinsic, mechanical irritation of the latter. The result is, under certain conditions of nutrition, warmth, moisture, senescence, obsolescence, &c., to stimulate an excessive and disorderly growth of the epithelial, endothelial, or connective-tissue cells entering into their structure.

The association of local irritations of extrinsic and palpable origin, such as the pressure of a clay pipe, rough tooth-plate, smoking, paraffin, soot, betel-chewing, Kangri stoves, and injury, with the starting of cancer is widely accepted.

The "sharp tooth" irritation cause of cancer forms a natural transition to the view now put forward that hitherto unsuspected intrinsic irritations are even more frequent and potent causes of the initiation of cancer, and this not only in the larynx but throughout the body. The sites of preference for the commencement of cancer, whether in the gullet, stomach, intestinal flexures, rectum, breast, skin, uterus, &c., are found to be those sites at which the resultant of excessive mechanical forces chronically impinge or act, and where, normally, there is no provision for escaping the consequences of such excess. This is seen if the springy joints, elastic lungs, and contractile muscles, which are little liable to primary cancer, are compared with such areas as the region of cricoid excursion, the œsophageal gastric junction and the rectum opposite the coccyx, where various rigid resistances and soft tissues undergo relative changes of stress and position with reference to each other with often excessive force.

The chief muscular mechanisms concerned in producing undue tissue stress, strain and friction are believed to be the motor mechanisms (1) for maintaining equilibrium in posture, (2) that for respiration, and (3) that for digestion; but important factors arise in the relative displacements and mutual pressures of the internal movable organs constrained in part by mesenteries and ligaments, and also from the artificial compression of the trunk by corsets.

Faulty postures and mode of breathing in man, whether from habits, occupation, imitation, intestinal derangements, or other causes, bring about persistent portal congestion, intestinal stasis, and general autotoxæmia. This is especially seen in marked types of belly-breathing, in which *habitual inspiratory descent* of the diaphragm takes place, with *no* expansion of the base of the thorax by the muscular mechanism of respiration.

Such abnormal muscular action of the diaphragm tends to produce, simultaneously but independently, two things: (1) Abnormal increase of intrinsic mechanical strains and irritation, causing cell overgrowth at sites of impact or restraint—e.g., the inspiratory pulling down of the cricoid; (2) through relatively lessened thoracic suction chronic portal congestion occurs, leading to intestinal autotoxæmia, general blood cachexia, and hence wasting of the tissues at large.

The central nervous system, through the deep afferents and visceromuscular reflexes, endeavours to co-ordinate the motor mechanisms so as to minimize undue strain and to adjust the deranged thoracic and abdominal functions, but under the influence of the chronic autotoxæmia or perhaps from the emotional depression of sorrow, grief or worry, sooner or later the higher co-ordinating centres strike work and the mechanical and portal derangements run further riot.

In this way the biological syndrome of cancer in the working of the human machine is believed to arise. The power wasted in undue stress, strain and friction appears to be converted into the energy of those unknown complicated chemical and physical constructive processes concerned in cell growth and multiplication—a marked contrast to those destructive results of similar forces on the material substance of a non-living machine in causing wear and tear.

During the epidiascope demonstration of lantern slides Dr. SCANES SPICER made the following remarks:—

My first slide is a schema or plan to indicate diagrammatically the centrifugal expansile action of the diaphragm in thoracic inspiration and the centripetal descending action of the same in abdominal inspiration, and the differing effects on the skeleton, viscera, and circulation. Abdominal breathing produces with every breath:—

(1) Excessive pulling down of the thorax and the thoracic viscera generally, and including the trachea, gullet, cricoid, and sternum, and therefore increased stress, strain, and friction of displaced, stretched, and compressed parts causing persistent rhythmic mechanical irritation at points of attachment of soft parts, or where these latter are compressed by rigid resistances of the endoskeleton.

(2) (*a*) Displacement, compression, and derangement of the abdominal viscera; (*b*) lessened thoracic suction of the venæ cavæ on the in-breath as compared with thoracic breathing; (*c*) loss of the normal influence of the expiratory contraction of the abdominal wall in emptying the portal area and promoting the general circulation.

Thus are produced simultaneously *irritation* about the larynx, throat, and gullet (and elsewhere as indicated); and likewise *concurrently* a *blood-cachexia.*

My next slides show normal and faulty erect postures, and how, in them, the transmission of the weight of the head and viscera to the ground differs, causing different degrees of stretch and compression about the larynx. In this connexion I call attention to the radiograms of the living subject, in which the different position of the head and curve of the cervical spine vary the positions, compressions, and stretching of the larynx.

The series of coloured lantern-slides of photographs of frozen sections after Braune's "Topographical Anatomy" (by kind permission of Messrs. J. and A. Churchill) show clearly and quickly the relations of the rigid resistances—i.e., the bones and cartilages—to the soft tissues about the larynx, and how their conditions would be altered by different postures and mode of breathing previously shown. If the cartilages are calcified, ankylosed, roughened, or thickened, such conditions must increase unduly the surrounding working stresses, strains, and frictions. These, if chronic and not too great, favour irritative neoplasia, in addition to the irritation factor provided by chronic and habitual exclusive inspiratory diaphragmatic descent.

It is curious how cancer in the larynx appears to dog the steps of cartilage. The two best series of operated intrinsic laryngeal cancers are those of Sir Felix Semon and Mr. H. T. Butlin. These demonstrate the close association between cancer and cartilage in the fact that the cartilage was usually involved in the diseased mass, and such cartilage had to be removed (if removable), or scraped at the radical operation. Thus, in Butlin's thirteen cases, ten were stated to have cartilage involved. Removal of cartilage was indicated in seven, and scraping in two; in one case cartilage was bare—no note as to scraping: growth recurred *in situ.* In Semon's ten cases cartilages had to be removed in six. It is generally assumed that cancer spreads, attacks, and causes disease of the cartilage secondarily. I suggest that it is excessive stress, strain, or friction in connexion with the movement, resistance, and sometimes ossification of the cartilage, that initiates the neoplasia. This latter subsequently takes its revenge by cutting off the blood supply of its irritating tormentor.

My theory affords a rational interpretation of the natural history of cancer of the larynx. It is in agreement with the age, sex, and site-incidences, the left-side predominance, the obscure commencement, the diffuse deep origin, the persistent hoarseness, the early vocal cord paralysis, the relatively greater extent in the depths than on the surface, and finally the success of laryngo-fissure and removal of all diseased parts. What other theory explains these facts?

Numerous anatomical diagrams are laid upon the table, after Quain, Luschka, Killian, Elsberg, &c., especially drawn and coloured to bring out the form and relations of the laryngeal cartilages, particularly the cuneiform and sesamoid cartilages, and the anterior and posterior vocal nodules. There is also a collection of published coloured illustrations of cancer of the larynx by leading British laryngologists. Though it is rare to publish diagrams of such at a sufficiently early stage to define the exact site of initiation, there are a few such. Sir Morell Mackenzie's sketches of the earlier stages of the laryngeal cancer of the Emperor Frederick are on view, and, taken together with Gerhardt's description, point to either a sesamoid cartilage or the posterior vocal nodule as probably the site of initiation. Sir Felix Semon's diagram of Mr. Montague Williams's case apparently opposes my theory, but on the growth being removed it was found to be attached at the back of the laryngeal ventricle, about which the arytænoid plays in functioning. At a later stage the relation of the bulk of a laryngeal cancer clustered over a subjacent cartilage is usually apparent, and I show many instances.

DISCUSSION.

The PRESIDENT (Dr. Watson-Williams) said he would be glad to hear any remarks, and especially called upon any member who felt able to support Dr. Scanes Spicer's thesis.

Dr. CATHCART remarked that someone had said that enthusiasm was a virtue; he was sure that every member of the Section would agree that the enthusiasm which Dr. Scanes Spicer had shown ought to be accounted to him for virtue. Dr. Cathcart wholly disagreed with the view upon which Dr. Scanes Spicer founded his entire theory. Instead of there being a greater downward movement of the larynx in belly breathing than in back breathing, as Dr. Scanes Spicer had stated, the exact opposite was the case, and anyone could test this for himself.

Mr. GAY FRENCH remarked on the fact that the evidence put forward by means of the specimens was that from the dead subject, and that the relative positions of parts in the living person were quite different from those in the cadaver.

The PRESIDENT said he asked if there was support for the views expressed by Dr. Scanes Spicer forthcoming from any member, because it was only fair to the Section to do so. Personally, he could not accept in any way either the premises or the conclusions of Dr. Scanes Spicer. Still, if a member felt that he had any scientific or useful information on any point he should have an opportunity of bringing it before the Section, which was the proper place for these topics to be brought to the notice of his fellow practitioners. There was a regrettable tendency, especially among some enthusiastic investigators of tuberculosis, to ventilate their particular views and practice in the public Press. But whether Dr. Scanes Spicer's views were correct or not, he thought there should be full opportunity for considering them.

Dr. SCANES SPICER, in reply, thanked the President and members for the opportunity of placing his views before them. He did not expect immediate concurrence, but at least they now knew what his contentions were. He feared Dr. Cathcart had not understood the problems at issue, since he confused the apparent movements of the larynx due to the rotation of the cartilages on each other with the tug on the trachea and cricoid now alleged. With reference to that gentleman's statements, however, it was the physiologists, and not the traditions of a singing school, that must decide. He (Dr. Spicer) had brought his physiological premises before the physiologists at Belfast[1] (1909) in connexion with "respiration," and his anatomical premises concerning the "normal" and "man's erect posture" before the anatomists in London[1] (1910). Mr. French had emphasized the conception which was at the starting point of his hypothesis—viz., that the position of parts in the dead body gave only a partial idea of the changes in their working relationships during life. He thanked those members for their remarks, and had the satisfaction that he had now been able to state his laryngological premises before the body competent to examine them.

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[1] British Medical Association Annual Meetings, *Brit. Med. Journ.*, 1909, ii, p. 673; 1910, ii, p. 1717.

An Instrument for the More Efficient Removal of the Nasal Spine in Submucous Resection of the Septum.

By Herbert Tilley, F.R.C.S.

The instrument is a bayonet-shaped chisel, mounted on a heavy handle, the proximal end of which is flattened so that, if necessary, a hammer may be used on it. The cutting edge of the chisel is V-shaped so as to prevent the instrument from slipping off the spine. The free ends of the V are blunted, so that there is little danger of wounding mucous membrane.

The President (Dr. Watson-Williams) said the instrument seemed to fulfil a want. He had felt some hesitation in using the chisels which had hitherto been brought to his notice on account of the damage they were liable to cause.

A Case of Leprosy in a Man aged 21.

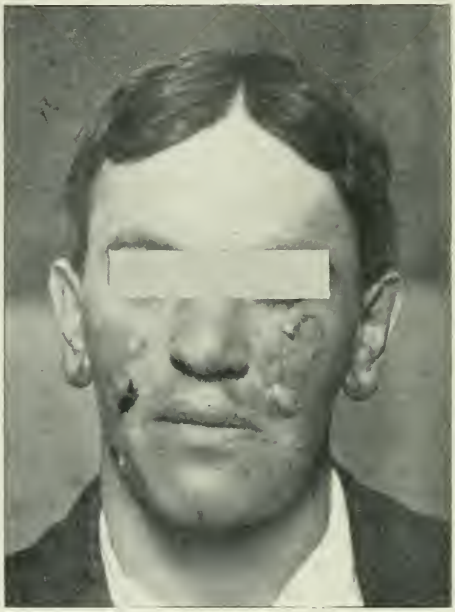
By H. J. Davis, M.B.

The patient is a lawyer's clerk, who contracted leprosy at the Cape five years ago. He is at present under the care of Dr. Abraham at the West London Hospital, where he is being treated with subcutaneous injections of nastin.

There is ulceration of the nasal mucosæ; the bacillus has been isolated from the discharge. The nasal cavities were affected three years after the first symptoms, which commenced in the little finger; larynx also affected; secondary laryngitis. Note the thickening of the nose and lobules of the ear, the nodules and pigmentation on the face and elsewhere, and the typical appearance of the hands, which are soft and feel like satin. The fingers are slowly withering; the toes are also affected. The ulceration in the nose resembles lupus, but the yellow discharge has subsided since treatment.

Patient states "that five years ago, when returning from Cape Town races one cold evening, he felt his little finger becoming numb and tingling. To see whether it was quite insensitive he put it into a cup of boiling water, but he felt nothing at all; the finger blistered and slowly withered, and so on to the next finger and to those of the opposite hand."

There are patches of anæsthesia and insensitiveness to heat and cold in all four limbs, and in consequence the disease was primarily diagnosed as syringomyelia, but its true nature slowly declared itself and he came to London for treatment.



Case of Leprosy.

DISCUSSION.

Dr. SEGURA (Buenos Aires), speaking in French, gave his experience of leprosy of the larynx, stating that it was often a late manifestation compared with the invasion of other parts of the body by the disease. He had fairly frequent opportunities of seeing the disease in South America, and had little doubt that the nose was in the majority of cases the site of the initial lesion; that here it might remain latent or be revealed simply by epistaxis and catarrh; and that it was by contact—directly or indirectly—with nasal mucus that the disease was often transmitted.

Dr. STCLAIR THOMSON said that two years ago he showed a case of leprosy in a coloured man from Jamaica. He had married an English wife, and was now at large wandering about England. He believed he preached on the sands. He was under the impression that leprosy was a notifiable disease in England, but this man had not been notified, and his presence in the community was dangerous.

Dr. DAN MCKENZIE said he saw a case of leprosy some years ago in an Englishman living in England, and he also was at large in the community; going to business in London daily for some considerable time. In the larynx of that man there were tubercular nodules affecting the whole of the epiglottis and ventricular bands. The man was very hoarse and had a rough cough. Like Dr. StClair Thomson, he had wondered whether it was desirable that a leper should be permitted to go freely in and out among healthy people.

Dr. DONELAN said he believed that the old statutes requiring the segregation of leprous patients were still unrepealed.

Dr. FITZGERALD POWELL said he had had some experience of leprosy abroad—in India and China—and agreed that this was a case. He was afraid that so far treatment had had very little effect in curing the disease. He understood this patient was being treated by injections, and should like to know what the injections were. In the past, in India, Chaulmoogra oil had been very largely used.

Dr. DAVIS, in reply, said the man had been having injections of nastin, given by Dr. Abraham, under whose care he was in the skin department. He called it nastin $\beta 1$, and Dr. Davis produced a specimen of it. The man developed the disease in South Africa; it was not at first recognized as such there, but looked upon as syringomyelia, because he had patches of anæsthesia in both limbs, on both hands, and elsewhere. On the right palm and right shin were extensive burns: when in the ward he had touched the stove and fender and had been unaware that he was doing so. The condition of the larynx was that of subacute laryngitis with thickening, but no ulceration was present as in the nasal cavities. The section exhibited showed bacilli in large numbers obtained from scraping the turbinals.

Thyro-lingual Cyst removed from a Girl aged 7; Operation followed by Symptoms of Acute Thyroidism.

By H. J. DAVIS, M.B.

THE child had enlarged tonsils and adenoids. She was admitted for operation as there was a small swelling under the jaw in the mid-line, which the parents wished removed as it was a disfigurement. This was done first. The cyst, as large as a grape, was attached to the tongue and hyoid bone; it was easily removed entire, the wound sutured with horsehair, and closed with collodion. In the evening the child became restless; the temperature rose to 104° F., the pulse was intermittent and irregular (160 per minute), the pupils were dilated, and the face was crimson—signs of acute thyroidism. The neck became swollen and puffy, and this persisted for six weeks. The wound healed by first intention. The child complained of no inconvenience beyond "her heart thumping."

Mr. W. G. Spencer, to whom I mentioned the case after the exhibition of his own case at the last meeting of the Section, attributed the disturbance to leakage of thyroid secretion and its absorption in a wound that was undrained. I have no doubt that this explanation is the correct one.

The PRESIDENT said he agreed with the suggestion of Mr. W. G. Spencer that the conditions which developed after operation were probably due to acute thyroid secretion absorption occurring at the time of the operation.

Unilateral Optic Neuritis and Complete Ophthalmoplegia Externa resulting from Acute Sphenoidal Sinusitis.

By H. J. DAVIS, M.B.

A GIRL, aged 25, was admitted under Dr. Beddard as a supposed case of cerebral tumour; he transferred her to me with a diagnosis of sinus involvement. She attributed her headaches "to standing for hours in the sun at the late King's funeral." Onset sudden, acute pain in right upper jaw and forehead, with general constitutional disturbances, sickness and giddiness.

Ophthalmological Report.—Diplopia, dilated pupil and right-sided optic neuritis only, and complete ophthalmoplegia externa. Transillumination showed right infraorbital shadow.

Operation forthwith: Antrum opened through cheek; outer wall of nose removed; middle turbinal and ethmoid mass removed; sphenoidal sinus full of pus, opened and drained. Ocular symptoms slowly subsided, and patient left hospital well six weeks later.

A one-sided optic neuritis points to naso-orbital disease, whereas disease of central origin is characterized usually by double optic neuritis.

The photographs show the patient's condition before, and three weeks after operation (figs. 1 and 2).

Remarks by Mr. Percy Dunn.—When first seen, some days after the operation, there was slight optic neuritis of the right eye, and almost complete ophthalmoplegia externa. As the optic neuritis appeared to be subsiding, and the right and left rectus showed some recovery of power, a good prognosis seemed possible. The pupil was dilated and immovable. Re-examining the patient on November 28 last, he found that all the extrinsic movements were normal. The only defect present was a semi-dilated pupil, which acted very sluggishly to light. The accommodative and consensual reflexes were natural. The disk was of natural colour, no atrophic changes being present, and the vessels were of normal size: R. V. = $\frac{6}{12}$, L. V. = $\frac{6}{6}$. The interest of the ocular signs was in their association with the sinus disease. The unilateral character of the optic neuritis excluded gross cerebral lesions; on the other hand, the fascinating researches and observations of Onodi, in respect to the topographical relationship existing between the optic nerve, optic canal, optic sulcus, and the posterior ethmoidal cells and sphenoidal sinus, afforded a complete explanation of the symptoms present. Onodi has shown that no fewer than thirty-eight variations of this intimate relationship could exist. These variations, moreover, were structural and not pathological, being due to changes in form of the parts concerned, occurring during fœtal and post-natal periods.

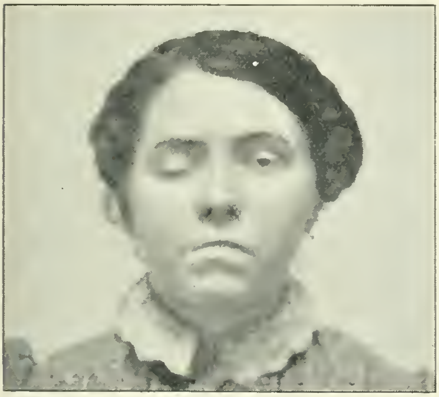


FIG. 1.

Acute sphenoidal sinusitis and ethmoid disease of only fourteen days' duration, right optic neuritis, ptosis, and complete ophthalmoplegia externa.

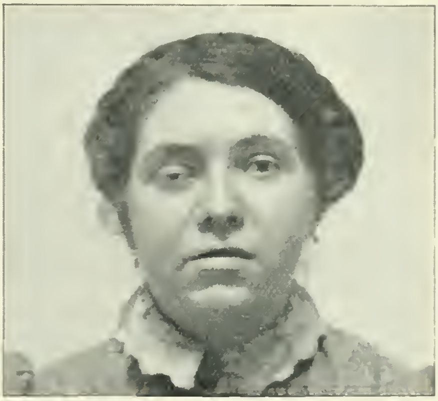


FIG. 2.

Recovery three weeks after the sinuses and antrum were opened and drained.

DISCUSSION.

Dr. WESTMACOTT reminded members that last session he mentioned a case in which there was marked optic neuritis, and almost complete blindness in the right eye, but no affection of the sphenoidal sinuses. There was an acute and foul empyema of the left maxillary antrum, and, on opening and draining it by the alveolar method, it healed up in eight weeks, and the sight eventually recovered.

Dr. DAVIS, in reply, said the other eye was normal. It primarily resembled a case of cerebral tumour, of which she had the signs: headache, vomiting, and optic neuritis, but the latter was on one side only. The antrum was affected secondarily to the sphenoidal sinus. He did not think opening the antrum alone would have cured her.

Empyema of the Antrum in a Man aged 71; now Malignant (Sarcoma).

By H. J. DAVIS, M.B.

AT the May meeting of the Section, 1909,[1] a case of chronic antral disease in a woman, aged 68, was shown in which the disease had become malignant. The jaw was removed for sarcoma. Some doubt was expressed by members as to whether malignant disease had developed in this way, and further evidence was asked for. The case exhibited is a parallel in every way. For three years the patient has had mucous polypi; both sides frequently snared and twice curetted. Left antrum became involved; it was opened and drained (Caldwell-Luc method). This had to be repeated, and on the second occasion tissue removed looked malignant, but the pathological report was against it. Swelling subsided, but again recurred.

A piece removed from the antrum a week ago is reported as "sarcoma." The growth is friable and very vascular, and there is no doubt now about the diagnosis. The palate and cheek are bulging, and it is proposed to remove the superior maxilla.

DISCUSSION.

Dr. STCLAIR THOMSON asked what was the point of origin in the antrum, and whether Dr. Davis could detect the base of the growth.

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[1] *Proceedings*, 1909, ii, p. 139.

Dr. Davis, in reply, said that the patient had polypi on both sides of the nose, and he later developed antral empyema. That was drained, but it recurred, and the operation was repeated, after which the patient was all right for some time. He afterwards developed a swelling in the cheek. A piece was removed from the antral cavity, which looked malignant. The antrum was filled with mucous polypi, which recurred. The patient got thinner, with bleeding at the nose. He therefore, two days ago, removed the upper jaw. He thought the whole disease had been removed. The specimen exhibited showed the disease had commenced in the antrum and invaded the malar bone and ethmoid region. The maxilla was removed in two pieces owing to this. A preliminary laryngotomy was performed and the fauces tightly plugged with gauze. The section exhibited showed the malignant nature of the growth. There was no base to the growth which infiltrated the antrum generally.

Empyema of the Maxillary Antrum in a Boy aged 7.

By H. J. Davis, M.B.

This condition is rare in children. The child was taken to a casualty department two months ago, and under the supposition that the swelling was due to carious teeth these were extracted and a hole made in the jaw. The case well illustrates the futility of attempting to drain the antrum by this old method, especially in children, for the permanent teeth shield the antrum, and it is probably never opened at all. The wound became septic, and little improvement resulted until the antrum was drained into the meatus three weeks ago. The jaw is still swollen, and several teeth have been shed.

DISCUSSION.

Mr. Robinson said it was a very early age for an empyema, but he had operated upon a boy 6 years of age, where adenoids had been previously removed three times at another hospital. The difficulty at this age was that the antrum was so small, and it was necessary to get high up in the front of the jaw so as to be above the roots of the permanent teeth. There was an unusual amount of swelling in Dr. Davis's case, especially on the palate, and therefore he thought that the antrum trouble was secondary to some acute bone mischief.

Dr. Adolph Bronner asked whether the condition had resulted from scarlet fever. Rhinitis was fairly common in scarlet-fever cases. He had seen several cases in which the ethmoidal and frontal cells had become infected.

Mr. LOGAN TURNER did not regard it as ordinary sinus suppuration, but agreed with Mr. Robinson that it was probably an osteomyelitis. He believed the patient had some dead bone on the alveolar aspect. The mapping out of the upper jawbone by the swelling of the cheek was very typical of osteomyelitis.

Mr. WAGGETT asked whether any member had seen empyema of the antrum in a child without external deformity. He had had to look up the literature a year ago, but could find no evidence that such had been seen.

Mr. HERBERT TILLEY said he had operated upon a child 8 years of age for chronic empyema of the antrum without any external deformity at all. It was due to intranasal infection.

Dr. DAN MCKENZIE asked whether the swelling was noticed before the operation, or since. Osteomyelitis might result from operation on the antrum; he had seen one such case.

Dr. DUNDAS GRANT said he related before the Odontological Section of the British Medical Association at Sheffield[1] the case of a child 6 years of age with empyema of the maxillary antrum in whom there was no external swelling. He opened it through the nose. It was the only case of the kind he had seen in so young a child, so he concluded it was rare. Dr. Bronner had drawn attention to the influence of scarlet fever in producing those malignant forms of inflammation in the sinuses, and Killian read a paper on the subject before the German Laryngological Society.

The PRESIDENT said cases came from time to time of empyema in young children. Mr. D'Arcy Power recorded a case 8 weeks old, which was supposed to be due to injury caused by forceps delivery at birth. He agreed that the case suggested osteomyelitis rather than pure empyema.

Dr. DAVIS, in reply, said the swelling was assumed to be due to a carious tooth, and it was removed. Then another was removed on the other side of it, before it was recognized to be empyema of the antrum. An attempt was then made to bore a hole into the antrum, through the alveolus, but this had failed. He thought the periostitis was the result of making the opening through the mouth, which became septic. It was not malignant, and the child was getting better.

A Large Nasopharyngeal Polypus Removed from a Boy aged 11.

By H. J. DAVIS, M.B.

THE polypus was attached to the base of the sphenoid, and produced considerable deformity; some paresis of the palate remains; the boy is otherwise well.

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[1] *Brit. Med. Journ.*, 1908, ii. p. 467.

DISCUSSION.

Mr. HERBERT TILLEY said such cases were not rare, and it was important to get rid of the growth early and thoroughly. Access could be obtained to it either through the nose, by removing the ascending process of the maxilla, or through the soft palate. He asked what course Dr. Davis pursued in this case.

The PRESIDENT said he had had a similar case, for which he did a Rouge's operation, and cut and deflected the septum to one side. He obtained very fair exposure of the nasopharynx. In order to remove the stump, he found it better to split the palate.

Dr. DAVIS, in reply, said he tried to remove it through the nose first, but found it was too large, so he had to remove it through the palate. On removal, the hæmorrhage was very great, but it was checked by plugging. The growth was the largest he had seen. A microscopic section showed the tumour was benign.

Recurrent Pedunculated Fibroma of Posterior Pillar of Fauces 2 in. long.

By ADOLPH BRONNER, M.D.

THE specimen was removed from a woman, aged 55, in October, 1910. The growth was 2 in. long, bifurcated, each end measuring 1 in., both ends thickened ($\frac{1}{4}$ in. thick) and elongated, and having a papillomatous appearance. The stem of the growth was about $\frac{1}{8}$ in. thick. A similar growth was removed in 1900. The patient complained of frequent attacks of coughing, during which she could feel a small growth on the side of the tongue, which disappeared. Another growth was removed in 1903. Both these were of nearly the same size as the one removed in October.

Specimen of a Tonsillolith.

By CHARLES A. PARKER, F.R.C.S.Ed.

THIS was removed from the patient shown at the last meeting.[1] An incision, $1\frac{1}{4}$ in. in length, was made through the anterior surface of the soft palate and the calculus was then exposed to view. By means of pressure from behind and traction with forceps in front, it was removed, but not without some little difficulty and slight laceration of the soft parts. The wound was stitched up with horse-hair sutures and has healed satisfactorily. The calculus is irregularly spherical in shape, measuring in its greatest circumference $4\frac{5}{8}$ in., and in the smallest $4\frac{3}{16}$ in. It weighs 490 gr.

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[1] *Proceedings*, p. 12.

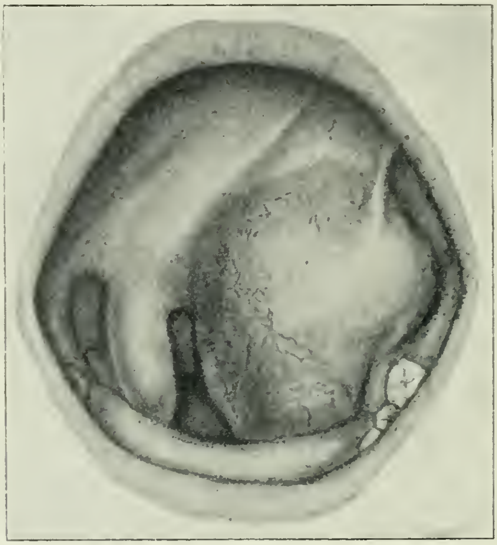

Fig. 1.

Tonsillolith in the left supratonsillar fossa, bulging the soft palate forwards and inwards.

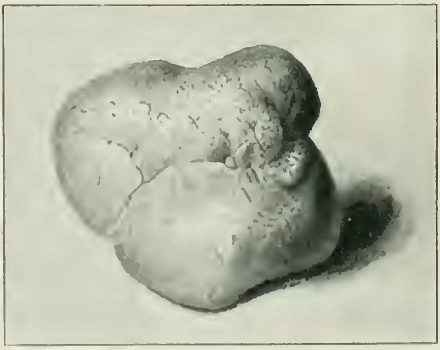

Fig. 2.

The tonsillolith removed.

Lupus of the Larynx in a Woman aged 36.

By W. H. Kelson, M.D.

Patient, who is married and has had one child and no miscarriages, came to hospital three years ago complaining of soreness and rawness of her throat. On examination, a red granular condition of the posterior wall of the pharynx was found with slight ulceration; the epiglottis was also swollen and presented the same red granular aspect; there was very little discharge. Auscultation gave no evidence of lung disease and no tubercle bacilli were found in such expectoration as could be obtained. The treatment, galvano-cautery (direct and indirect methods) and arsenic internally, has been partially successful, but the disease has shown markedly relapsing and slowly spreading characteristics. At present the pharynx has healed, but a cicatricial band can be seen; the epiglottis is defective; the ventricular bands are swollen and there is some arytænoid swelling. The view of the vocal cords is obstructed, but they are seen to be red and swollen when a glimpse is obtained. Since the inside of the larynx has become affected the patient has become hoarse.

DISCUSSION.

Mr. Logan Turner asked whether Dr. Kelson had tried tuberculin, or intended to do so. It would be a pity to interfere instrumentally, owing to the risk of producing stenosis. His experience with tuberculin had been variable; in some cases there was no apparent improvement, in a few cure followed in conjunction with general tonic treatment.

Dr. Dundas Grant said the difficulty in judging as to the effect of tuberculin was that such cases fluctuated, and, under favourable hygienic conditions, there was often much improvement. He had seen several cases under tuberculin treatment at the Brompton Hospital, and the tuberculin seemed to be beneficial.

The President said that in such a case he would try tuberculin BE, 1 in 5,000 of the solid substance, increasing the dose up to $\frac{1}{250}$ mg., being guided in the amount of dosage by the amount of reaction.

Dr. Kelson, in reply, said he intended to use tuberculin, but thought that it would be better to show the case first, and then the members could see what the effect was.

Stricture of the Lower End of the Œsophagus—Spasmodic?

By J. GAY FRENCH, M.S.

THE patient, a young woman, aged 32, states that she was perfectly healthy till she was 20 years old; she then noticed the first symptoms of her present complaint, which consisted in a difficulty in swallowing solids. At first, however, she could do so by taking a glass of water immediately after the solids. At this time her weight was 8 st. 7 lb. Slowly but gradually she got worse, till at the age of 27 she could take no solids at all, and even fluids caused vomiting. Her weight dropped to just over 5 st. She was admitted into a London hospital and treated for gastric ulcer. She was kept in for three months and discharged. She states that the vomiting was better, but that she still had the same difficulty in getting any food down.

The patient first came under my care about a year ago. Her weight then was 8 st. She could not take any solids, but stated that she could get fluids through, after swallowing, by lying on her right side and taking deep breaths. When given a couple of glasses of water she could regurgitate this without any great effort.

Direct œsophagoscopy revealed a very dilated œsophagus, the lower portion of which was filled with fluid and frothy mucus. Some of this fluid was removed and examined; no trace of free hydrochloric acid was found. The exhibitor has so far been unable to get a bougie through the cardiac orifice.

The patient has been examined with X-rays, and on the last occasion was watched for over an hour with a screen—after the administration of 2 oz. of bismuth carbonate by the mouth—and a series of photographs taken and direct drawings made. (These were shown with the patient.)

DISCUSSION.

Dr. WILLIAM HILL expressed surprise that Mr. French had appended to the title of his case a query as to whether the condition might be due to spasm. The suggestion that a stricture near the cardia which was so tight as to admit only of the passage, with great difficulty, of small bougies, and in association with one of the largest dilatations of the œsophagus on record, carried its own refutation with it. The obvious answer to Mr. French's query was an emphatic negative. Purely functional tonic spasm of the gullet not associated with inflammatory tumefaction was a condition often heard of but never seen.

Functional dysphagia was sometimes due to spasm and want of co-ordination in the pharynx, but purely functional tonic spasm in the gullet was unknown to the œsophagoscopist for the simple reason that endoscopy was carried out under anæsthesia, either general or local, and anæsthesia necessarily abolished spasm. Chevalier Jackson and many others failed to appreciate the weakness of the spasm theory, but even Jackson admitted that general anæsthesia abolished spasm, although he unconvincingly maintained that he had actually seen, under cocaine anæsthesia alone, marked chronic stricture due to purely tonic phreno-spasm and cardio-spasm. Functional paresis might possibly produce slight intermittent functional dysphagia near the phrenic narrowing and lower end of the gullet, but it was quite unlikely that any functional neurosis in unstriped muscle would produce a narrowing of the nature of spastic paralysis and leading to marked dysphagia and to dilatation of the gullet. When the X-ray test with bismuth porridge showed a definite arrest, evidences of either inflammatory tumefaction or else cicatricial changes were always found on œsophagoscopic examination in his not inconsiderable experience. The case was obviously one of marked inflammatory stricture at the phrenic constriction and of long duration. Mr. Roughton had a similar case under his care in hospital a year or so ago, and the speaker had recently seen a boy aged 17, at Brighton, under Dr. Curwen, in whom the condition had been greatly relieved for many years by the employment of a Symonds's funnel. He recommended Mr. French to try some form of intubation, though a funnel might not long be retained, perhaps on account of the dilatation and atony of the œsophagus above the stricture. In conclusion, the statement that purely functional spasm in the gullet did not lead to organic changes such as stricture and dilatation might appear to be too dogmatic, but he believed it would be found to be substantially correct.

Dr. DUNDAS GRANT remarked that Dr. Hill's experience differed from that of Dr. Guisez, of Paris. He had been hoping to find such cases as the latter had described, but had not yet seen one. Dr. Guisez's description of cardio-spasm was very definite, and his recent book on diseases of the œsophagus embodied many of the monographs which had appeared in the *Journal of Laryngology*. Dr. Guisez used a dilator, which he introduced into the cardiac orifice of the stomach, apparently with the result of causing complete disappearance of the dilatation of the œsophagus. He was surprised to read the description, but he believed it was accurately drawn from nature.

Sir FELIX SEMON said that when listening to Dr. Hill's speech he was reminded of the saying, "There were men before Agamemnon." He did not think it justifiable to speak so dogmatically as to the non-existence of a disease because he had not seen it. He (Sir Felix) had seen and successfully treated several cases which could not be other than functional spasm of the œsophagus, without any evidence of organic obstruction or active inflammation. They were examples of pure spasm of the œsophagus. He did not see why the œsophagus should not be affected by spasm as well as any other canal of the body. Accurate observations were made even before the introduction of the new instruments, which undoubtedly formed a most valuable addition to the surgeon's armamentarium. He would only relate one such case. A young man came to him who had several short attacks of complete inability to swallow, lasting for a few minutes. They occurred at long intervals, but one day he quite suddenly had a more serious attack, which was still present at the time of the consultation. That was long before the days of the œsophagoscope, and he introduced a bougie, which was arrested two or three inches below the lower edge of the cricoid plate. He held the instrument for a moment *in situ*, then tried to pass it gently down, but without success. He then removed it and introduced a larger one, with the same result. Then he tried the largest he had, and by gentle pressure he got the instrument through suddenly and introduced it into the stomach. He withdrew it, and the patient was able to drink water. In a week's time the condition had disappeared, and there had been, so far as he knew, no recurrence. Such cases could not be otherwise registered than as functional spasm of the œsophagus.

Dr. FITZGERALD POWELL said that he thought there was something to be said for both sides of the argument. At the present time these strictures were looked upon more as congestive or inflammatory, plus concurrent spasm of the muscular coats of the tube, and not true spasmodic stricture alone. The same thing applied in the urethral structure. They were not permanent, and were fleeting in character. He did not think for a moment that Mr. French's case was one of these. Nor did he think it was malignant, but was inclined to regard it as a fibrous stricture similar to those found at the pyloric end of the stomach. It could not be satisfactorily treated through the œsophagus, and should be approached through the stomach.

Mr. HERBERT TILLEY said that two years ago he went to South Wales to see a lady who for months was supposed to have had malignant disease of the gullet. She had been living on slop food, because she was convinced she could not swallow solid food. After examination with the bougie had established the presence of a stricture, it was regarded by the doctor as an organic obstruction: her age was 62. Mr. Tilley had seen the lady in London eighteen months before, with symptoms which suggested organic disease. He passed a bougie, and afterwards she ate a solid lunch. From that date until a few weeks before he saw her again in South Wales she had been in good health. On this second occasion he examined her with the œsophagoscope, and found the gullet quite healthy. The patient was persuaded to eat solid food, and did so, and, as far as the speaker knew, she was still quite well, although two years had elapsed since the last examination. He thought that "spasm" in a neurotic patient was the most probable explanation.

The PRESIDENT said he saw the case just referred to before Mr. Tilley had seen it. There had been difficulty in getting her to swallow, and it was feared she might lose her strength. He failed to pass a small bougie when he saw her in consultation with her doctor, not liking to use force. From the history it seemed likely that the condition was benign, probably spasmodic. But at her age such a difficulty would probably be associated with malignant disease, and, although he definitely stated he could not say there was a malignant growth, he considered there was reason to fear that it might prove to be so. Further than that he did not commit himself to a diagnosis. He heard afterwards that Mr. Tilley had seen the patient, and been able to pass a large bougie.

Mr. GAY FRENCH, in reply, said that there was no question as to the entity of spasms in the musculature of the intestinal canal. Neurospasms were quite well known, and congenital pyloric stenosis occurred at the other end of the stomach. That was held to start by spasms, which led to the subsequent hypertrophy. He had seen a congenital pyloric stenosis through which a probe could not be passed. The girl had never had pain, which was not in accordance with the idea of inflammation. He welcomed the suggestions which had been offered, and would use a Symonds's funnel. Since sending in the notes he had been able to get a No. 7 Mackenzie bougie through, but it was so tightly gripped that he had difficulty in withdrawing it. Directly afterwards she was given some water to drink, but could not get any through. If he did not succeed when trying the courses now suggested, the only thing to do would be gastrotomy and dilatation from below.

A Case of Early Myasthenia Gravis with Laryngeal Symptoms.

By SOMERVILLE HASTINGS, M.S.

THE patient, a boy aged 14, was first brought to the Middlesex Hospital at the end of July, 1910, complaining of difficulty in breathing and snoring at night. His mother, who brought him, told us for the last two months he had regurgitated fluids through his nose when drinking, and that she had found an increasing difficulty in understanding what he had to say, especially towards night. He had not suffered from sore throat. When examined about this time the vocal cords were seen in the cadaveric position, and were almost stationary. The boy was unable to cough, and articulation was defective. The soft palate moved very slightly, and there was some congestion of the mucous membrane of the nose, but nothing in the nasopharynx. The knee-jerks were normal.

On October 4, 1910, he was sent into the hospital and seen by Dr. Voelcker, who diagnosed myasthenia gravis. It was then noted that the palate became completely immovable after the patient had said

" Ah " some half-a-dozen times. Lateral nystagmus also appeared after repeated movements of the eyes. The boy was unable to screw up his eyes or wrinkle his forehead, and the facial muscles generally were very weak and showed the " myasthenic reaction " to electrical stimulation. The muscles of mastication and those of the trunk and limbs were quite normal. The thymus was not enlarged.

The patient improved a great deal while in the hospital, and when last seen (November 25) was much better. The laryngeal condition had especially improved, and the cords came together fairly well. He was still unable to whistle and could not completely close his eyes. The soft palate hardly moved at all.[1]

Mr. HASTINGS said that he had brought the patient before the Section because it was a rare condition and difficult to diagnose; indeed, he had failed to recognize it himself. At first sight the case resembled one of diphtheritic paralysis, but it was quite evident that it could not be this affection. A medical colleague saw the case for him, and first suggested the correct diagnosis.

Cases illustrating Results of Removing Nodules from the Cords by means of Galvano-cautery (by the indirect method).

By E. A. PETERS, M.D.

A. L., MUSIC-HALL artiste, aged 25: Loss of upper notes and occasional total loss of voice due to laryngitis and unilateral nodule on cord. Removal of nasal obstruction and rest gave some improvement, but the galvano-cautery removed the nodule completely, and she now sings with a good voice.

Mrs. W., aged 31, complained of loss of voice for one year, particularly affecting the upper notes of her contralto voice. The nodule was removed by galvano-cautery. The voice is better than it ever was.

H. S., aged 41, foreman fitter, shown here last session with nodule on left cord and impaired voice. The removal by galvano-cautery and treatment for nasal obstruction have restored his voice.

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[1] The boy began to suffer from severe dyspnœa on the evening of December 6, and died from failure of respiration within a few hours. No naked-eye changes were observed at the necropsy.

DISCUSSION.

Mr. HORSFORD said removing singer's nodules by any operation was very difficult; but of the various methods he would regard the galvano-cautery as the most risky. He asked whether Dr. Peters touched lightly and repeatedly, or whether he went sufficiently deep at one sitting.

Dr. STCLAIR THOMSON said he would ask the same question as Dr. Horsford. At a recent discussion at the Section the cautery was banned, as it was said the singing voice might readily be injured thereby. He would have thought the galvano-cautery was very suitable for the trifling nodule which obstinately persisted in many cases in spite of rest, treatment, and vocal re-education; but he had been put off employing it on account of the danger reported by others.

Dr. PEGLER said he did not think that, if the galvano-cautery was used with care, there was any particular difficulty or risk. He preferred it, of the two, to nitrate of silver fused at the end of a laryngeal probe, which had to be used more often, and one might easily go too far even with it. Only after the slough had become detached could one judge how much of the nodule remained to be treated. In using the cautery he thought very much depended on the method and degree of cocainization, which should be complete enough to insure complete control of the cords. The patient might have to be trained through several sittings before the cautery was applied, and even then much of the success must depend upon the proper selection of the case.

Dr. FITZGERALD POWELL, while recognizing the very good results in these cases, would very strongly advise members not to resort to treatment of "singer's nodes" by the galvanic cautery, in the case of professional vocalists, whose living depended on the perfection of their voice, as the slightest destruction of tissue might have dire results for their voice. It was quite evident in these cases which cords had been cauterized—the ones thus treated were more tense and thinner than the opposite fellow. Then, why run such a risk, when a few months' rest, and suitable treatment of the nose and pharynx with local astringent applications, such as iron and paint, would restore the voice and cure the nodes?

Mr. CHICHELE NOURSE said he was glad to hear Dr. Powell allude to the treatment of such cases without surgical measures. It had to be borne in mind that in a certain proportion of cases singer's nodes would entirely disappear if disorders of the nose and the pharynx were attended to, and care were taken to change the mode of voice-production by means of suitable exercises.

The PRESIDENT congratulated Dr. Peters on his excellent results. But he asked how long an interval had elapsed between the treatment and the result. If a nodule was recently removed by the galvano-cautery there might be a less successful result as the condition cicatrized. It was better to do too little than too much.

Dr. PETERS replied that one case was treated six months ago and the other about a year. The voice of the lady shown that day was better than before, as her friends testified. The larynx was anæsthetized by 5 per cent. cocaine, both sides of the epiglottis being treated. The probe was then passed until the patient permitted detailed examination and manipulation. The man required only one such application and the lady five. He used a flat-ended cautery, bending it so that he could see the point of contact. He did not turn on the heat until he was certain it was in position. In both cases there was one application, as he was anxious not to overdo it. He felt so certain of the treatment that he used it for a girl who earned her living by singing. For a small singer's node the proper treatment was to attend to the nose and see that the patient carried out proper breathing. Four years ago two patients came to him, and he suggested that he should first treat the nose and then the cord; they both went to somebody else. Therefore it was better to restore the voice first and treat the nose afterwards.

Case of Aphonia.

By T. JEFFERSON FAULDER, F.R.C.S.

H. H., LABOURER, aged 33. Hoarseness of voice began in 1906; ever since then he has suffered from attacks of hoarseness with slight intermissions. Since November, 1909, the patient has frequently been unable to speak except in a feeble whisper. Previous illness in 1906, syphilis; treated for eighteen months. He has been treated also for a considerable time lately with mercurial inunctions and iodides with only temporary occasional improvement. Patient's general condition is very good and the lungs seem healthy. There is general chronic laryngitis with very considerable inter-arytænoid thickening.

Dr. PEGLER said the condition was the same as when he last saw the patient in his own clinic. He had done everything he could to ameliorate the state of the cords up to the time the patient ceased attending, with but little success. He thought the phonation was remarkably good under the circumstances.

Laryngeal Tuberculosis—Cure?

By Cyril Horsford, F.R.C.S.

Woman, aged 37, after suffering from hoarseness and some difficulty in swallowing for two years, was first seen by exhibitor in July, when the epiglottis was greatly swollen, somewhat nodular, and ulcerated. The right vocal cords swollen and a small ulcer on its surface. The right ventricular band and the anterior surfaces of both arytænoids were also ulcerated. At that time she had a slight evening temperature (99° F.), and there was evidence of tuberculous deposits in both apices. The case was treated by immediate amputation of the diseased portion of the epiglottis, and under cocaine pure carbolic acid was carefully applied (four times at intervals of a fortnight) to the ulcerated areas.

There is now no sign of infiltration or ulceration in the larynx.

A section (cut by Dr. Wyatt Wingrave) of the diseased epiglottis was shown under the microscope.

DISCUSSION.

The President congratulated Mr. Horsford on his result; there seemed to be no tuberculous deposit.

Mr. Horsford replied that he would have liked to have heard if any of the members had tried pure carbolic in such cases. He had used it in this case, and the ulcers healed rapidly.

Carcinoma of Œsophagus. ? Temporary Cure under Radium Treatment.

By William Hill, M.D.

Male, aged 45, first sought advice at hospital in December of last year for dysphagia; had been unable to swallow meat for six months, and soft food with difficulty. On œsophagoscopic examination, hard, nodular tumour of anterior and left lateral wall of lower cervical œsophagus seen, with œdema above and also below, extending down to about the bronchial constriction; bleeding easily when touched. Cancer diagnosed without hesitation from appearance of strictured area. Under six prolonged applications of radium salt objective evidence of malignancy has disappeared; the lumen in lower cervical and upper thoracic region, which was very small and crescentic, is now normal. Progressive improvement in swallowing, which, however, is not quite normal. Increase in weight and vigour.

Carcinoma of Deep Pharynx with Extension to Gullet and to Posterior Margin of Larynx.

By WILLIAM HILL, M.D.

FEMALE, aged 43. The largest development of the growth is on the pharyngeal aspect of the pharyngo-laryngeal party wall. A surgeon recently advised Glück's operation on this case as justifiable. The exhibitor has found, however, that there is a second extensive focus of sprouting growth half-way down the thoracic œsophagus. ? Extension "per contiguitatem" (Semon). Radium to be tried as a palliative.

Laryngological Section.

January 6, 1911.

Dr. P. Watson-Williams, President of the Section, in the Chair.

Demonstration of Instruments for the Intravenous Injection of Salvarsan ("606").

By W. A. Lieven, M.D. (Aix-la-Chapelle).

Dr. Lieven said intravenous injection was the most useful method, and gave very satisfactory results. It obviated abscesses, feverish reactions, and necrosis. The idea of the apparatus now exhibited was to enable the practitioner to make injections with great certainty and without harm. In making an injection into a vein, the great thing was to avoid putting any of the substance into the hypodermic tissue. If "606" got into the tissues, there were painful infiltrations, fevers, &c. The apparatus consisted of two tubes, one of them filled with physiological saline solution and the other containing salvarsan. The operator need not pay any attention to the tubes, leaving that to an assistant, his effort being to get the needle directly into the vein, after causing it to protrude by means of a bandage; taking care not to pass the needle through the vein beyond its farther side. The fact of being in the vein would be shown by the blood appearing in the glass. At that moment the assistant must remove the india-rubber bandage, let a little saline go through, and then pass the salvarsan into the vein. It should be the practice to allow some saline to enter afterwards, because of the liability of a little salvarsan getting into the tissues on withdrawing the needle. The apparatus was recommended by Frank, of Berlin, and he (Dr. Lieven) highly recommended it, as it simplified the procedure very much.

When the substance was first introduced it was claimed that it would always cure the disease after one injection. But those who had followed the history of the introduction of the arseniates had learned to be somewhat suspicious about such statements. In sleeping sickness, for instance, Koch had marvellous results, but when he returned a year afterwards and wanted to see his patients again, they were either dead or could not be moved. He (Dr. Lieven) felt that the use of "606" by injection would be the method of choice, but never the routine method. One reason was that it compelled the patient to lie up for some time, and everybody could not afford to do that. After hypodermic injections the reactions did not always appear on the first day; not till the third or fourth day did the patient become feverish, and there might be recurrence a fortnight later, so that the patient had to be kept under clinical observation. It did not seem a method suitable for general practice. No doubt these substances had a marvellous effect on certain forms of syphilis, and many of the cases got better very soon, but most of them no more quickly than with the usual remedies properly given. It must not be forgotten that injection of calomel often cured a lesion in five or six days, and "606" was no quicker. He had been all over Berlin, and found that there were 30 to 40 per cent. of relapses in chancre cases in the first three, four, and five months (roseola, mucous patches, &c.). One could only judge as to relapses by the early cases, because in the later stages of syphilis relapses did not appear at such short intervals as in the first year of the disease. The roseola took a long time to disappear. The results were not, so far, encouraging as regards lasting cure. But where there was an idiosyncrasy against mercury and a quick result was desired in malignant syphilis, salvarsan seemed to act very well. It could not yet be decided whether it would be wise to combine the administration of salvarsan with that of mercury; and ten or twenty years' experience would be required before it could be said that it had prevented the occurrence of such diseases as general paralysis of the insane and locomotor ataxy. At present he did not feel justified in treating a patient in general practice by means of salvarsan only, and would do so only at the particular request of a patient.

In Berlin a good many cases of necrosis had been observed after hypodermic injection, appearing about two months afterwards, and showing very little tendency to heal. It was reported even that claims for damages had been made in consequence of those sequelæ.

Ectopia of the Pituitary, with other Congenital Anomalies of the Nose, Palate, and Upper Lip.

By ALEXANDER R. TWEEDIE, F.R.C.S., and ARTHUR KEITH, F.R.C.S.

THE subject of this paper was a male child, aged 6 months, admitted to the Nottingham Children's Hospital on July 31, 1910.

The condition noted at the time by one of us (A. R. Tweedie) was the following: Median harelip, with a groove on the columella of the nose continuing the line of the cleft; a mesial cleft in the soft palate which was occupied by a cystic swelling springing from the roof of the roof of the nasopharynx. No treatment was considered advisable. The child died of inanition on August 6, 1910. The head was preserved and afterwards examined by one of us (A. Keith). The specimen was presented to the Museum of the Royal College of Surgeons of England, where it is now shown in the Teratological series. The main result of the examination was to show that the cystic swelling seen in the cleft of the palate contained the pituitary body. Ectopia of the pituitary body is an uncommon condition; Haberfeld [2], who has investigated the matter recently, was able to find records of only five cases, and all of them in literature of a past period. A full description of the condition of parts is therefore desirable.

In fig. 1 the median cleft of the upper lip is shown, the two halves being joined at the base of the columella by a thin fold of mucous membrane; under the fold there is a pit or depression, into which a probe can be pushed backwards for 5 mm. In the floor of the labial cleft the alveolus is exposed, separated into right and left halves by a shallow fissure. A small pedunculated fibroma is attached to the left half. The columella is wider than normal and grooved; the top of the nose shows a median depression. There is evidence here of the bilateral origin of the septum nasi, which will be more evident when the condition of the internal parts comes to be described. The soft palate is cleft, and through the cleft a tumour is seen. The tumour, as was proved by further dissection, is really situated in the septum nasi.

Suspecting that the tumour might be connected with the pituitary body, a dissection was made by Mr. William Pearson to expose the infundibular parts of the brain and the adjoining basal region of the skull. Unfortunately, the condition of the brain did not permit an exact examination of the cerebral parts, but the relationships of the third ventricle were quite clear. It will be seen that the basi-occipital and basi-sphenoid up to the dorsum sellæ are intact; the parts of the cranial base formed round the notochord are thus normal. The floor of the sella turcica is absent—or, putting the matter in another way, the cranio-pharyngeal canal, in the region of which the buccal (glandular) part of pituitary is developed, is widely patent—wide enough to take the little finger. The deformity at the base of the skull concerns the region formed from the right and left trabeculæ cranii. It will be remembered these bars of cartilage are formed on each side of the pituitary, and out of them are developed the following parts: (1) The small wings of the sphenoid, which, in the specimen, are separated by a wide depression, filled by a mass of tissue; (2) the septum of the nose, the two halves of which are separated in the specimen, enclosing a V-shaped trough which becomes narrower and shallower as the septum passes towards the columella; (3) the lateral masses of the ethmoid and alar cartilages. The base of the skull in this child presented a median depression extending from the dorsum sellæ to the position of the crista galli. The floor of the depression is formed by the open out septum nasi. The free or lower border of the septum extends from the basi-sphenoid to the anterior part of the hard palate with which it effects a normal union. The right and left halves of the nasal septum thus enter into the formation of the cranial floor. The posterior part of the lower or free border of the septum is thick and tumour-like. It lay directly above, almost in, the cleft of the soft palate.

Into the posterior part of the nasal trough, directly in front of the basi-sphenoid (fig. 2), descended a funnel-shaped prolongation of the floor of the third ventricle; it represented the infundibulum, but was thick-walled and composed chiefly of neuroglial tissue. The anterior part of the trough was filled with soft, friable, semi-fibrous tissue of a pale grey colour. Microscopically it was found to be composed chiefly of non-striated muscle tissue.

Part of the tumour-like mass on the posterior part of the septum was removed for microscopical examination by Mr. H. Wilson and stained with Van Gieson's reagent. Fig. 3 represents a sagittal section near the middle line, corresponding to the cut surface indicated in fig. 2. The lower border of the septum is seen to be covered by stratified mucous epithelium similar to that of the mouth, but, as the section passes up towards the base of the skull, the epithelium suddenly changes

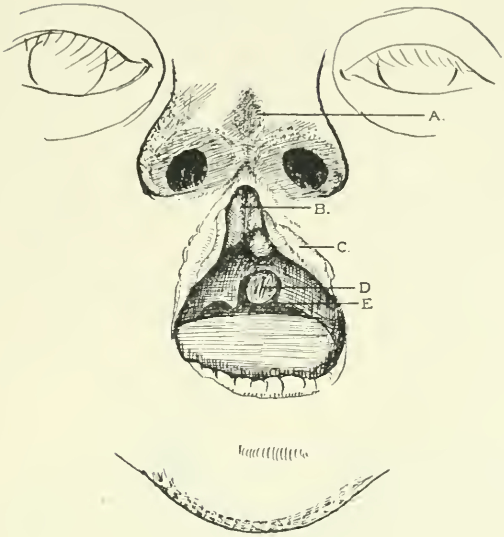

FIG. 1.

Sketch of the upper lip, nose and palate of Mr. Tweedie's case. A, median groove on nose; B, median depression in alveolus; C, lip; D, tumour; E, cleft of palate.

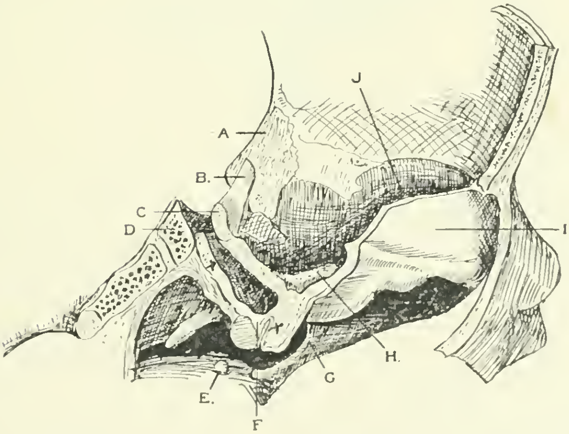

FIG. 2.

A deep dissection of Mr. Tweedie's case, viewed from the right side. The right side of the head has been cut away, exposing the pharynx, nose, and left base of the skull. A, lesser wing; B, optic nerve; C, chiasma; D, basi-sphenoid; E, tumour in foramen cæcum of tongue; F, infundibulum; G, cleft in palate; H, tumour mass in septum; I, nasal septum.

its character and becomes mucous columnar epithelium. The line of transition probably corresponds to the bucco-pharyngeal junction, for it will be remembered that the primitive mouth is of epidermal, the primitive pharynx of hypodermal origin. From the stratified area, just before it passes into the columnar, a solid column of epithelium enters the tumour-like mass in the septum, and, passing upwards and backwards (*see* fig. 3), expands, and within the epithelium appears a cleft, evidently the primary cavity of the buccal pituitary. The cavity is 8 mm. long; its width is uncertain. On the upper side of the cavity—the side directed towards and applied to the expanded infundibular part of the brain—there is an extensive formation of typical glandular pituitary tissue, small islands and columns of cells separated by fine trabeculæ. One estimates that if the tissues were aggregated in the usual shape it would form a normally sized gland. The pituitary tissue is closely applied to the infundibular extension of the brain, but there is no separation into anterior and intermediate parts as in the normally formed gland. The side of the pituitary diverticulum directed towards the nasopharynx—it is applied really to the submucous coat of this region—shows no formation

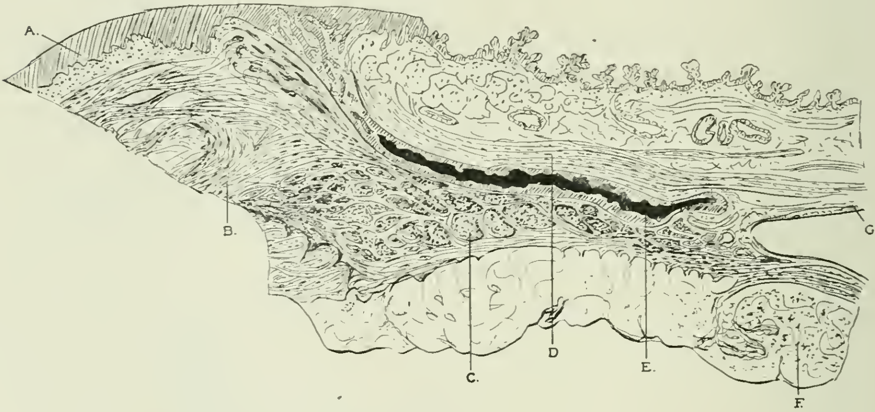

Fig. 3.

Drawing of a microscopic section removed from the tumour-like mass on the posterior part of the septum nasi. A, stratified epithelium on lower border of nasal septum; further along it becomes continuous with the columnar mucosa of the roof of the nasopharynx; B, non-striated muscle; C, pituitary tissue; D, non-striated muscle; E, primary cavity of pituitary; F, neural layer; G, periosteum. (The position of the section is shown by X X in fig. 2.)[1]

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[1] The section is shown reversed, the nasopharyngeal border being above, the infundibular border below.

of pituitary tissue. In the normal development of the pituitary, one can see that the part of the primitive pituitary, directed towards the floor of the sella turcica, is arrested in its development, so that at one point the primary cavity almost reaches the surface of the gland.

This specimen is especially interesting in connexion with investigations which have been carried out recently by Erdheim [1] and Haberfeld [3]. In fig. 4 the track of the developing buccal pituitary is shown; the lower part lies in the submucous tissue at the root of the septum nasi. Haberfeld has shown that there is always a remnant of pituitary tissue in the submucous tissue of the human nasopharynx at this point; it is often the size of a grain of boiled rice; he named it the pharyngeal hypophysis. Erdheim found, in a case of acromegaly, that a tumour mass had developed from a remnant in the sphenoid below the sella turcica. As early as 1888, Killian had observed a pituitary remnant in the submucous tissue of the nasopharynx. The pharyngeal remnant lies in the anterior part of the pharyngeal tonsil, close to the septum.

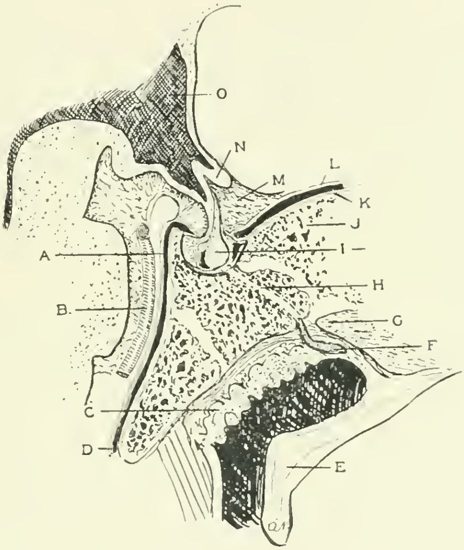

FIG. 4.

Diagrammatic section to show where pituitary remnants may occur. (From a preparation presented to the Museum of the Royal College of Surgeons by Professor Symington.) A, arachnoid; B, basilar artery; C, pharyngeal tonsil; D, arachnoid; E, palate; F, pituitary of pharynx; G, vomer; H, pituitary track; I, anterior lobe; J, pre-sphenoid; K, dura; L, arachnoid; M, sub-arachnoid; N. chiasma; O, third ventricle.

Often a vein escapes from the lower part of the pituitary track in the sphenoid ; the track may persist as a crani-pharyngeal canal ; it does so in about 1 per cent. of human skulls. It can be seen in 10 per cent. of crania of newly born children ; it can be seen in the crania of 50 per cent. of anthropoids ; in sharks it remains open. The track of the pituitary fills up during the third month of fœtal life by the fusion of the right and left halves of the basi-sphenoid. Mr. J. E. Frazer, of King's College, London, has lately shown that the mouth of the pituitary invagination in the human fœtus is placed on the posterior border of the septum nasi and carried downwards on that border during further development. All of these observations help to make clear the condition in this case. We have here an arrest of the ascent of the pituitary—it remains in the position corresponding to its point of outgrowth ; there is further a non-fusion of the right and left trabeculæ cranii across the mid-line of the basis cranii ; we see that the septum of the nose is primarily a part of the base of the skull ; there is a prolongation and enlargement of the infundibular region of the third ventricle into the ethmoidal area of the base, and it is possible that this is the primary lesion.

One very remarkable and inexplicable feature remains to be mentioned. In fig. 3 it will be seen that the infundibular wall becomes continuous with a stratum of tissue which shows all the characters of non-striated muscular tissue. There was a massive formation of non-striated muscle continuous with the lamina in the anterior part of the septal trough. Mr. Shattock suggested to us that it may be continuous with the non-striated muscle tissue of the orbit, out of which Müller's muscle is formed. It may belong to this formation, but why a mass of this tissue should occur in connexion with the infundibular region of the brain and within the septum of the nose one cannot explain. The appearance neither supports nor confutes Gaskell's theory of the infundibulum being primarily an œsophagus leading to a stomach represented by the third ventricle. Our sections do not show a communication between the pituitary and cavity and infundibulum, but the series is not complete enough to deny that there may have been a communication.

REFERENCES.

[1] Erdheim, von. *Beitr. z. Path. Anat.* (Ziegler). 1909, xlvi, p. 233.
[2] Haberfeld, W. "Die Rachendachhypophyse," *Beitr. z. Path. Anat.* (Ziegler). 1909, xlvi, p. 133.
[3] *Idem.* Loc. cit.

DISCUSSION.

Sir FELIX SEMON asked whether what Professor Keith called the mouth of the pituitary body was identical with what was known as Tornwaldt's bursa. That author had drawn attention to this body, which was situated near the posterior border of the septum, at the upper part of the roof of the naso-pharyngeal cavity. From Professor Keith's description it would almost appear as if the two structures were identical.

The PRESIDENT (Dr. P. Watson-Williams) thanked Professor Keith for his demonstration, and said that all the members must be very interested in the specimen because the morphological points touched upon seemed to afford some explanation of many anomalous cases which were constantly cropping up in practice. He (the President) had taken the view that in Tornwaldt's disease the pocket or recess was the remains of Ratke's pouch or the obliterated fœtal process which, carried up, formed the pharyngeal portion of the pituitary body. In his work on the nose he gave illustrations showing how they arose and offered it as an explanation of Tornwaldt's disease. But there were many other points of interest arising from the specimen—e.g., the split septum, which rhinologists came across, such cases as Mr. Parker had illustrated.

Dr. SCANES SPICER commented on the extreme importance of the subject, particularly to the members of that Section, so that they were specially indebted to Dr. Keith for bringing it forward. He had himself brought the subject of arrested development in the basis cranii before the Society in connexion with the physiognomy of adenoids and the physiognomy of achondroplasia. Some years ago he showed here a child which presented the signs of achondroplasia. He did not now think it should be classified thus, though it evidently was related to some abnormality in the area of the pituitary hypophysis. No doubt the arrest of development about the basi-sphenoid produced a similar physiognomy to that of a marked case of adenoids coming on early in life. His patient was sent to him from the country by a doctor to be operated upon for adenoids, but there were practically no adenoids; it was a primary arrest of development at the base of the skull. With regard to the point raised by Sir Felix Semon as to whether the pharyngeal hypophysis now discussed was identical with Tornwaldt's bursa, Sir Morell Mackenzie asked him (Dr. Spicer) to write an article for the *Journal of Laryngology* in 1887. The evidence pointed to the conclusion that the bursa in Tornwaldt's disease was not a pharyngeal diverticulum, but part of the pharyngeal tonsil pathologically altered. Further research seemed to support the view that Tornwaldt's disease consisted in pathological states located in a different position from the pharyngeal diverticulum, which opened in the posterior border of the septum nasi. He had seen the bursa discharging yellow matter, and his view was that such states were due to adhesion of the lips of the median groove of the pharyngeal tonsil, and very much posterior to the pharyngeal diverticulum.

Sir Felix Semon, in further comment, said he had seen several cases agreeing with Professor Keith's description, in which a structure at the roof of the nasopharyngeal cavity was discharging. It was much nearer the septum than the position indicated by Dr. Hill. He had seen it absolutely close to the septum, and that was why he asked as to the identity with Tornwaldt's bursa. He did not claim that this proximity was a universal rule, but could vouch for it that he had seen the discharge close to the septum.

Dr. Jobson Horne desired to add his thanks to Professor Keith for his demonstration of a specimen of great scientific importance to those following rhinology. He had observed on the posterior edge of the septum of the nose the condition which Dr. Keith had described. In his experience the position of that vestigium appeared to be relatively far distant from that which is commonly regarded as the situation of Tornwaldt's bursa.

Dr. H. J. Davis said he remembered a case of Mr. Waggett's at the London Throat Hospital ten years ago. The site of the disease was at the highest part of the pharyngeal roof, and just to the side of the posterior edge and base of the vomer; it was very difficult to see it.

The President said it was a congenital or developmental condition, and it was now recognized that the position of Luschka's tonsil altered somewhat in the course of development, for, as the basi-sphenoid developed, the relative position of the mass of lymphoid tissue altered, and came more towards the posterior wall. That would account for the fact that Tornwaldt's cleft, as seen in the adult, appeared to be further back than would be indicated by the diagram shown of the relations of the parts at birth.

Denture removed from the Œsophagus.

By Norman Patterson, F.R.C.S.

Patient, a woman, aged 56, one morning, ten minutes after eating a piece of bread and butter, was astonished to find that the tooth-plate had disappeared from the roof of her mouth. She felt her neck, and it appeared to be larger than usual. Shortly after this she suffered much inconvenience from saliva collecting in her throat. There was also some pain. Laryngoscopic examination showed swelling and congestion of the arytænoid region, but no portion of the tooth-plate was visible. A skiagram was taken, and shortly afterwards, under a general anæsthetic, and with the aid of the bronchoscope, no difficulty was experienced in catching hold of the plate and dragging it out of the œsophagus. The posterior edges of the plate and the hooks attached thereto, however, got caught in the anterior pillars of the fauces, and moderate traction failed to move it any further. It was therefore split up with bone forceps and then removal was easy. One interesting point about the case is that the plate lay in the œsophagus with the front or incisor portion pointing downwards. The patient made a good recovery.

DISCUSSION.

Mr. HOWARTH asked what was the exact situation when seen with the tube; it seemed to him to be in the hypopharynx judging from the skiagram. Most dentures were in the hypopharynx and did not reach the œsophagus at all.

Mr. PATTERSON replied that so far he agreed with Mr. Howarth that the upper part was in the hypopharynx, but the lower part he considered was in the œsophagus. It was invisible by ordinary laryngoscopic examination.

Party-wall Pharyngeal Cancer.

By CECIL GRAHAM, F.R.C.S.

THIS patient was shown at the Society's meeting on May 6, 1910, by Dr. Hill.[1]

Complete laryngectomy was performed on May 14, 1910. In cutting clear of the ulcer, which involved the posterior aspect of the cricoid and part of the adjacent left pyriform fossa, the mucous membrane of the pharynx was divided transversely. The line of suture can be seen as a transverse scar. With the exception of one deep suture immediately above the trachea the wound was left completely open, being lightly filled with gauze packing. Subsequently two plastic operations were performed before there was complete healing by the end of August, 1910. A small fistula at the upper end of the wound was the last to close.

DISCUSSION.

The PRESIDENT congratulated Mr. Graham on the result. The patient was not now displeased with life. She appeared happy, and as time went on she might get more adapted to her new conditions.

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[1] *Proc. Roy. Soc. Med.*, 1910. iii (Laryng. Sect.), p. 142.

Dr. DUNDAS GRANT said that he had procured for one of his cases of total laryngectomy an instrument devised by Gottstein. This consisted of a flexible tube which fitted into the tracheotomy cannula, and which had above it a curved silver tube which was placed in the patient's mouth and was fitted with a reed which could be set in vibration by the breath. To set it in vibration it had to be blown through with considerable force, and his patient, though a vigorous adult, was unable to set it in vibration. The force of air required was much greater than the fitting of the tracheotomy tube would permit of, and the force required was also too great for the lung power. Mr. Robert H. Woods, of Dublin, invented a bellows like the bag of a bagpipe for the patient to carry under his arm, so that when he squeezed it there was enough pressure of air to produce a tone and set the reed in vibration. He had exhibited a patient before the British Laryngological Association with such an instrument, and apparently with a satisfactory result. If there was a fistula a small tube with a reed could be passed through it.

Chronic Ulcer of Nasal Septum.

By CECIL GRAHAM, F.R.C.S.

THE patient was a bricklayer, aged 60. History: Four attacks of nose-bleeding; first one in 1908, second in 1909, third in July, 1910, and fourth on December 20, 1910. First seen on December 22, and galvano-cautery was applied to a small ulcer on the right side of the septal cartilage immediately behind the vestibule. At the present time the ulcer is seen to be triangular, and measures about ½ in. by ¼ in., which is double the size when first seen. This increase in size is probably due to the cautery. The septal cartilage is exposed, but is not perforated.

Case for Diagnosis.

By A. J. WRIGHT, F.R.C.S.

MRS. C., aged 34. History of "soreness" in throat for about fifteen months. Slight cough, but no sputum. Swelling of arytænoids, left ventricular band, and interarytænoid space. No abnormal physical signs in chest. Pirquet reaction twice negative. No pyrexia or loss of weight. No improvement with iodides. Four applications of galvano-cautery in last four months, with slight improvement.

DISCUSSION.

Dr. DUNDAS GRANT regarded it as a case of lupus of the larynx. That would accord with the appearance, with the result of treatment, and particularly with the history in which absence of signs of tuberculosis of the lungs was so definite.

Dr. DAN MCKENZIE agreed with Dr. Grant, and said there seemed to be a suspicious spot in the nose on the right side, fairly far back. Possibly there was a patch of lupus there.

Dr. JOBSON HORNE said the appearance of the larynx in no way suggested to his eye the disease of lupus, but undoubtedly that of tuberculosis.

The PRESIDENT said the patient was seen by him in July last, and he was puzzled by some aspects of the case. She then had some redness and swelling of the arytænoids, and slight superficial ulceration on the surface of the right arytænoid. Neither the epiglottis nor the vocal cords were involved. She complained of a pricking sensation, and was losing flesh. The pulse was 124, and there were night-sweats. He found increased vocal resonance on the right side of the chest, but no moist sounds. He thought it was probably tuberculous, and he suggested trying a von Pirquet reaction. These were the conditions in July. He would be glad if Mr. Wright would give the subsequent history.

Mr. WRIGHT, in reply, said that when he saw the patient first, four months ago, he thought it a case of tuberculosis of the larynx. But as the von Pirquet reaction was twice negative, he put her upon potassium iodide, and the result was very astonishing. After only one week's treatment, 8 gr. three times a day, the larynx swelled up greatly; this swelling involving the epiglottis, which had previously seemed unaffected.

Sarcomatous Growth of both Tonsils, with Multiple Sarcomata in and beneath the Skin.

By WILLIAM HILL, M.D.

MALE, aged 28. Sarcomatous nodules in and beneath the skin of head, neck, shoulders, and thorax, about fifty in number, and of three months' duration. Involvement of both tonsils first noticed two months ago. The patient has been energetically treated by Dr. Graham Little with antisyphilitic remedies, including arsenic (soamin) for six weeks, but the disease has steadily progressed. Two Fleming-Wassermann tests have been made, with negative results. Portions removed from a subcutaneous nodule and from one tonsil for microscopy were identical in structure—large, round-celled sarcoma. [The patient has died since the meeting.]

A New Pattern Expanding Œsophagoscope.

By William Hill, M.D.

DISCUSSION.

Mr. Howarth asked whether the instrument differed materially from the instrument on the same principle invented by Brünings. It expanded in one plane only and was called the dilatation œsophagoscope. It had been on the market for about three years.

The President said Dr. Hill's instrument would be especially useful with flat bodies. If a dilating instrument were expanded in one plane much wider expansion was possible in an œsophagus than if expansion were tried in three planes.

Dr. Jobson Horne said nobody living could establish a claim to the originality of an expanding instrument for œsophagoscopy. All credit for such an instrument was due to Sir Morell Mackenzie. A reference to his work would establish his claim to have devised such an instrument as far back as the year 1880.

Dr. Hill, in reply, said his instrument was constructed on the principle of the glove-stretcher, and essentially differed from the expanding instrument of Brünings, which was on the crocodile-jaw principle: the latter was less efficient as regards illumination and length of the expanding limbs. Guisez, of Paris, had anticipated Brünings in inventing an expanding œsophagoscope which was, however, constructed on the tube-forceps principle, and was less efficient than either his or Brünings' instruments. All three expanded in one plane only.

An Improved Œsophago-gastroscope, combining the Methods of Direct and Indirect Vision.

By William Hill, M.D.

The apparatus, which has been designed by the exhibitor in association with Dr. Herschell, consists essentially of two parts: (1) The exhibitor's direct œsophago-gastroscope, 55 cm. long, which is passed entirely under direct vision through the pharynx and œsophagus into the stomach by means of proximal illumination. This part of the instrument differs from a long Killian endoscopic tube, in that an inflation apparatus, with a removable plane-glass slanting window is attached to the proximal end, as in Canney Ryall's type of sigmoidoscope. (2) Of a rod-like, straight, indirect-vision optical apparatus, 65 cm. long, which is passed into the stomach through the lumen of the outer direct vision tube previously inserted, and after the window of the latter and the Brünings' hand-lamp have been removed. This indirect optical part of the apparatus is constructed on the principle of Trouvé's polyscope as in the cytoscope, and is almost a replica of the optical part of Loening's indirect gastroscope; it is, however, 2 mm. smaller in diameter than Loening's instrument and 5 cm. longer, so as to be long enough to endeavour to explore the upper part of the duodenum in certain cases.

The element of risk inherent in the use of all purely indirect gastroscopes—e.g., the Mikulicz-Souttar, Loewenstein and Loening instruments, which are necessarily inserted blindly under the guidance of touch and not of vision, is thus avoided in the employment of this apparatus; and an efficient, and at the same time really safe method of gastroscopy in accordance with sound endoscopic principles has now, it is believed, for the first time been rendered possible and worthy of general adoption.

[Some thirty members examined an exsected œsophagus and stomach with the apparatus and obtained an excellent view of the interior of the latter organ, and were all able to find a foreign body present.]

Cystic Adenoma of Facial Parotid Gland.

By W. Stuart-Low, F.R.C.S.

Female, aged 24, came to the hospital complaining of disfigurement and pain due to a swelling in the left pre-auricular region. The enlargement has existed for many years, and has been becoming gradually more prominent during the last three years. The pain has only been present for three weeks.

The case was shown to draw attention to what the exhibitor believed to be an important rule in treatment—viz., that seemingly innocent tumours in the parotid region should always be excised as early as possible, especially if indications such as pain and increased growth arise, since malignant changes are well known to be liable to supervene. This tendency to become malignant he considers to be accounted for by the hypomyxiatous conditions normally present in the parotid gland. This is a striking contrast with the submaxillary and sublingual salivary glands, in which malignant disease primarily originating is almost unknown.

Epithelioma on the Right Palato-glossal Fold in a Man aged 48.

By W. STUART-LOW, F.R.C.S.

THIS patient came to the hospital six days ago complaining of pain in the region of the right side of the base of the tongue and at the angle of the jaw on the same side. The pain had been felt for some weeks, and was increasing. There was a hard gland, painful on pressure, and with deep attachments at the angle of the jaw on the right side. There was no visible ulceration at the seat of pain on the palato-glossal fold, but on palpation and comparison with the other side severe pain was experienced and some induration detected. The exhibitor believes that this is an early stage of malignancy, and proposes to excise the gland for diagnosis.

DISCUSSION.

Dr. JOBSON HORNE, in discussing the case, said he could see no evidence of malignant disease.

Dr. FITZGERALD POWELL said there was a calculus in the submaxillary gland which, he thought, was causing the trouble.

Mr. STUART-LOW, in reply, said that he had intended to put an interrogation mark in the title of the case, as he was by no means sure that it was malignant, and really sent it for diagnosis. The symptoms were not typical of of anything, and certainly not of salivary calculus.

Case of Nasopharyngeal Fibrous Polypus in a Man aged 21.

By W. H. KELSON, M.D.

GROWTH was removed two and a half years ago by Dr. Kelson; recurrence first noticed twelve months ago, but the patient only returned to hospital three weeks ago; several profuse hæmorrhages have taken place. The patient is somewhat deaf on both sides. Transillumination shows the right side dark, the left clear. Evidences of sarcoma were not found when examined microscopically after previous removal.

DISCUSSION.

Dr. H. J. DAVIS said he supposed the condition was malignant.

Dr. JOBSON HORNE said that he did not consider that such growths, speaking generally, should be regarded as malignant in the commonly accepted meaning of the term. Under the microscope it was quite true that sections from such growths presented the appearances of sarcoma. Nevertheless, in his experience, they might be divided into two groups: (1) those that killed by extension and erosion of adjacent parts, and (2) those that killed by metastases. The all-important point in the removal of such growths by operative measures was to get away the mother part of the tumour, in spite of the alarming bleeding which such a measure entailed. There was nothing to be gained by removing the parts which were presenting and obstructing.

Mr. STUART-LOW said that he considered the growth was sarcomatous. He had had a number of those cases during the last three years, and had reported them in the *Lancet* from time to time. He recommended operation and thorough removal of every trace of the tumour, and had found that preliminary laryngotomy was a great assistance. The pharynx being then firmly packed with gauze and the palate split in the middle line, the tumour could be removed from the vault of the nasopharynx, and the site of origin well scraped.

Dr. H. J. DAVIS asked whether there was any advantage when dividing the palate in splitting the uvula. He had a similar case to operate on, and he wondered whether there was any advantage in going to the side of the uvula, and, if so, whether there was more liability for the stitches to tear through if this were done.

Mr. ROSE asked whether Dr. Kelson had been able to make out the exact point of origin of the fibroma. On two occasions he had tried to find the point of origin, and in each case it had been in the nose, just where the posterior part of the nose joins the nasopharynx, and not the roof of the nasopharynx.

Dr. FITZGERALD POWELL said that with regard to splitting the palate he had divided the palate on the side and also in the centre, but it made no difference in the stitching. It was not always wise in such cases to stitch the wound. It was better to leave it open for some future time to get at the malignant growth if it recurred. In the case of simple fibroma, the splitting of the palate and going to one side of the uvula did very well, and in these cases the wound should be closed. He did not think it mattered whether it was split in the centre or at the side.

The PRESIDENT said he had a similar case, which had been operated upon by somebody else. The spot of origin was at the upper part of the choana on the right side and adjoining portion of the roof of the nasopharynx. He did a Rouge's operation, and then divided and turned the septum to one side, and approached through the nose and the palate. That gave good access. He hoped the patient would be brought again before the Section.

Dr. KELSON, in reply, said the case probably was not malignant. On examining the specimen after removal two and a half years ago it was found to have no sarcomatous elements. The actual point of origin seemed to have been close to the opening of the sphenoidal sinus. The growths seemed to grow and press and set up inflammation and adhesions. Previously the tumour had grown into the antrum, and it had probably done that again, because the antrum was dull on that side; but it did not take origin in the antrum.

Papilloma of Pharynx in a Male aged 42.

By G. C. CATHCART, M.B.

PATIENT came to hospital last week complaining of hoarseness and a feeling of something in the throat, which he had noticed for some months. On examination a growth is seen to be springing from behind the posterior pillar of the fauces on the right side, about 1 in. long, club-shaped, and rather paler than the surrounding mucous membrane.

DISCUSSION.

Mr. CLAYTON FOX said the case had much more the aspect of a fibroma; it did not resemble a papilloma. He advised removal of it with the galvano-cautery snare.

Dr. FITZGERALD POWELL suggested the possibility that it was tonsillar tissue. The appearance was that of fibroma, and it would be interesting to hear the result of the microscopical section.

Dr. JOBSON HORNE agreed that the case did not present the appearances which were commonly regarded as those of a papilloma, but presented more the appearances of a fibroma. He had seen a similar growth projecting from the inner canthus of the eye, and such a growth under the microscope had exhibited a structure similar to that met with in the nasopharyngeal tumours in young subjects. He agreed with the previous speaker that the growth should be removed by means of the snare; he would go further, and point out that such a snare should be applied cold, but be connected with an electric current.

Experience had taught him that such a tumour bled freely, and, if it were snared off with a hot wire, that the bleeding would be avoided. Furthermore, the tumour was tough, and it required a hot snare to sever it.

Ulceration of the Left Vocal Cord in a Male aged 67. Case for Diagnosis.

By G. C. CATHCART, M.B.

PATIENT came to hospital this week complaining of hoarseness, which interfered with his singing voice. Two years ago, he says, he had the same kind of hoarseness, but it disappeared completely. The present attack has been noticed for two months. There is no pain, no enlargement of glands, and no specific history. The laryngoscopic examination shows a small ulcer with everted edges situated at the junction of the vocal process with the true cord on the left side.

DISCUSSION.

Mr. CLAYTON FOX regarded the condition as pachydermia laryngis. The left cord was on a higher plane than on the right. There appeared to be no symptoms, so the condition probably was not urgent. The patient was an amateur vocalist, but there seemed to be no history of syphilis or alcohol to account for the condition.

Dr. JOBSON HORNE expressed his thanks to Dr. Cathcart for having brought forward this case. It illustrated a clinical point to which he (Dr. Jobson Horne) had drawn attention as far back as 1899. The case was one of pachydermia laryngis. The clinical point to which he had drawn attention had already independently been referred to by the previous speaker—namely, an inequality of the planes of the vocal cords. Formerly it was alleged that the excrescence of one cord brought about by means of attrition the indentation of the opposing cord. This he had established by his researches to be erroneous. The excrescences and the corresponding indentations occurred by the adhesion and the excrescence of the mucous membrane on both cords. He had shown in pathological specimens that these lesions which would lead to a loss of voice were overcome by a vertical adaptation of the cords so that the excrescences, being placed on one cord on a higher level than on the other cord, were digitated into the furrows of the opposing cord, and in this way the remarkable preservation of voice in pachydermia laryngis was to be accounted for. It was gratifying to him to hear that the previous speaker had observed this clinical phenomenon in this case absolutely independently of any remarks from himself.

Malignant Disease of Pharynx under Treatment by Radium.

By R. H. Scanes Spicer, M.D.

Miss F., aged 52; 105 mgm. applied for thirty-five hours in all. Great improvement in the appearances and symptoms occurred within forty-eight hours, as shown by (1) lessened swelling and redness of neoplasm, and (2) disappearance of dysphagia, choking, and mucous pharyngorrhœa.

Three skiagrams and a microscopical section of excised portion of neoplasm were exhibited. The former showed osteophytic excrescences from the fifth and sixth cervical vertebræ under the malignant mass and cartilaginous outgrowths from the intervening intervertebral disk. Histologically, the growth is an epithelioma.

Dr. Finzi said section of the growth showed it to be very early epithelioma. Apparently the hard mass in the pharynx had nothing to do with the growth over it. It seemed to him a particularly suitable case for radium; 105 mg. of radium-bromide were used, the filtration being through 1½ mm. of platinum or lead, and it was applied to the growth for fourteen hours. The glands on each side were similarly treated, for ten and a half hours each. Until about nine months ago the application of radium to the pharynx was one of the most difficult one had to do. Then Dr. Hill suggested a little device which simplified the matter extraordinarily. It was used originally for treating cancer in the upper part of the larynx, and consisted in having the radium tubes tied on to the middle of a bougie and passing the bougie down the œsophagus, when the bougie would hold the radium tubes in whatever position they were required. For treating the upper part of the pharynx he had a little contrivance of his own, namely, to pass a Belloc sound with silk through the nose and then draw the silk through, attach the tubes to it, and draw them up behind the palate with a silk tied on to each end of them. The silks were then fixed outside the nose and the tubes remained suspended in the proper position.

Case of Chronic Tuberculosis of the Nose, Larynx, and Lungs.

By C. A. Parker, F.R.C.S.Ed.

Male, aged 31, first seen in May, 1909. He complained that for three or four years he had suffered from discomfort and slight discharge from the nose, and that for two months he had been hoarse.

On examination the right inferior turbinal was found to be generally enlarged and its middle third was infiltrated and superficially ulcerated; the epiglottis was infiltrated and ulcerated, and showed some loss of substance; in the interarytænoid there was well-marked irregular thickening, and the cords were infiltrated and thickened. The lungs were examined by Dr. Briscoe who reported thus: "Probably a healed patch of tuberculous disease at right apex; a few scattered crepitations at left apex; some doubtful signs of tuberculosis in left lower lobe." The sputum and some tissue removed from the right inferior turbinal were sent to the pathologist for examination. On the former he reported that tubercle bacilli were present in small numbers, mostly within the leucocytes, whilst as regards the latter he stated that the section showed structure usually found in lupus. The patient was at first treated with increasing doses of arsenic, and the local application of lactic acid, but did not make much progress, so in July, 1909, it was decided, in consultation with Dr. Briscoe, to try tuberculin injections. This part of the treatment Dr. Briscoe kindly carried out. At first he was given $\frac{1}{1500}$ mg. of Koch's T.R. tuberculin once a fortnight, and later every alternate fortnight the dose was reduced to $\frac{1}{3000}$ mg. From the first injection the patient began to improve, and after six months' treatment the epiglottis was quite healed, the interarytænoid infiltration had disappeared, and the cords, except for some injection, were nearly normal. After eight months' treatment, Dr. Briscoe reported that there were no signs of tuberculosis to be found in the lungs, and that there were no tubercle bacilli in the sputum.

In July, 1910, there was a slight recurrence on the right side of the septum. This was curetted, and lactic acid was applied, and quickly healed. Since then there has been no active manifestation of the disease. The patient is still having occasional injections of tuberculin.

This case is shown because it is typical neither of lupus nor of the ordinary complications of phthisis as seen in the upper respiratory tract. In ordinary lupus it is extremely rare to get pulmonary symptoms, at any rate whilst the lupus is in active progress; on the other hand, it is very uncommon to get nasal complications of pulmonary tuberculosis except in far-advanced and rapidly advancing cases. The history, the appearances and the physical signs in the lungs suggest that this case should be looked upon as chronic tuberculosis coming somewhere between lupus and phthisis.

The case is also shown so as to suggest that it is these chronic forms of tuberculosis which respond best to tuberculin injections.

Tuberculosis of Soft Palate, Pharynx, and Larynx in a Girl of 18.[1]

By W. G. Howarth, F.R.C.S.

The patient has complained of some soreness of the throat for the last two or three months and soreness of the roof of the mouth for one month. There is no loss of voice or difficulty in swallowing. She has been treated by her doctor with simple inhalations. The lungs have not been examined. The teeth are in a septic condition. Almost the whole of the soft palate is superficially ulcerated. The ulceration is shallow, with irregular edges, and here and there small patches of healthy tissue remain. The ulceration spreads downwards on to the fauces and pharynx. The tonsils are ulcerated and show, in several places, protuberant flabby granulations. The epiglottis is swollen and infiltrated, and part has been destroyed by ulceration. The arytænoids are also infiltrated, and the ulceration appears to be extending to the left ventricular band. The cords are not involved.

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[1] Shown at the Meeting held on December 2, 1910.

Laryngological Section.

February 3, 1911.

Dr. P. Watson-Williams, President of the Section, in the Chair.

Case of Tuberculous Tonsillitis.

By Frank Rose, F.R.C.S.

Male, aged 21, noticed some swelling in the neck and slight soreness of the throat at the end of November, 1910. Both tonsils enlarged and granular. Upper group of cervical lymph glands enlarged. Von Pirquet reaction negative. Chest said to be healthy. Section of piece of tonsil removed for examination, and section containing tubercle bacilli. Tubercle bacilli found in sputum.

Bilateral Tuberculous Disease of Tonsils with Tuberculous Laryngitis.

By Cecil Graham, F.R.C.S.

Female, aged 29. Pain in swallowing and enlarged glands in left side of neck for eight years. Pain in swallowing, hoarseness, and tenderness of glands more noticeable for two months. Both tonsils are large and ragged, the left being covered by whitish-yellow secretion. There is infiltration of fauces on either side, which is spreading across the middle line, and involving the uvula. There is œdema of the left arytenoid. Sir John Broadbent has examined the chest, and has found no definite evidence of tuberculous disease of the lungs. The inference is that the case is one of primary disease of the tonsil.

DISCUSSION.

Mr. SECCOMBE HETT reminded members that Mr. Wylie had shown a case of tuberculosis of the tonsil with ulceration in a patient who had also signs in the chest.[1] Mr. Hett then said he had seen half-a-dozen cases of tuberculous tonsillitis in phthisical out-patients. Dr. StClair Thomson, speaking at the same meeting, said that he (Mr. Hett) had been fortunate, for in twenty-five years he had not seen tuberculous ulceration of the tonsils in a patient sufficiently well to attend an out-patient department. Since then Mr. Hett had seen several more cases of tuberculous tonsils at Mount Vernon Hospital and elsewhere, the patients being the subjects of phthisis. In these cases tubercle bacilli had been demonstrated in sections from the diseased tonsils. One case was that of a man aged 26, with signs at both apices; the temperature was normal, and the patient could get about. There were deep excavating ulcers of both tonsils which had destroyed lymphoid tissue down to the capsule. He did not think it was as rare as had been supposed to find a tuberculous ulcer of the tonsils in cases of phthisis. A fortnight previously Mr. Rose had kindly given him the opportunity of seeing his case, and he could confirm Mr. Rose's observation that there was then no swelling of the arytenoid. Mr. Rose's cases had been found to have signs at both apices, and he thought that Mr. Graham's case also would prove to have phthisis.

Dr. DAN McKENZIE said it would appear that there were two types of tuberculosis of the tonsil: one in children, the disease being in the substance of the tonsil itself, and unassociated with ulceration, but associated with enlarged cervical glands; and the other in adults, in which there was obvious disease and ulceration, but without the cervical glands being involved; in this second type phthisis was usually present.

Dr. A. BRONNER desired to know whether tuberculous disease of the tonsil was common as a primary affection, or whether it must be assumed that the lungs were always involved in such cases.

Mr. SECCOMBE HETT believed that in such cases the chest was usually primarily affected. In children tuberculosis of the tonsil occurred as a local infection. The tonsils were not ulcerated, but tubercle bacilli could be demonstrated in them, and they acted as sources of infection and caused tuberculous adenitis if left untreated. In adults tuberculous tonsils ulcerated and were usually associated with phthisis. They were sometimes found in cases of chronic phthisis as slowly progressive ulcerations, but he would add third clinical variety to those mentioned by Dr. Dan McKenzie, namely, advanced or acute cases in which the tonsils ulcerated as a terminal infection. In such cases the ulceration rapidly broke down and invaded the soft palate if the patient survived long enough.

Dr. E. A. PETERS reminded members of the work done some time ago by Dr. Walsham, who showed that a large percentage of patients dying of phthisis exhibited tuberculous deposits in the tonsils, while ulceration of these organs was rarely seen.

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[1] *Proceedings*, 1910, iii (Laryng. Sect.), p. 58.

The PRESIDENT (Dr. Watson-Williams) said it was difficult to decide, and impossible to generalize, as to whether there was primary tuberculous disease of the tonsil apart from disease of the lung, or whether the disease was primarily in the lung. There were many opportunities of secondary infection.

Mr. FRANK ROSE replied that tubercle bacilli had been found in the sputum since he last saw the patient. Also during the same fortnight œdema of the arytenoids had appeared. The case belonged to the third group mentioned by Mr. Hett. Though there was disease in the lungs, the von Pirquet reaction was negative, a result which was very misleading. It was very important to know whether the disease was primary or secondary, because on that the decision as to operation must depend. He did not propose to operate.

Mr. GRAHAM replied that tubercle bacilli had been found in the sputum, but the œdema of the arytenoid, which was present a week ago, had since disappeared. He believed his case to be primary disease of the tonsil: there were no definite signs in the lung at all. With a history of eight years one would expect the lung condition to be quite advanced if the primary disease was in the lungs. He would remove the tonsils and have them microscoped.

Demonstration on a Living Subject of the Hill-Herschell Method of Combined Direct and Indirect Gastroscopy by Means of a New Type of Œsophago-Gastroscope.

By WILLIAM HILL, M.D.

THE patient, whose stomach was endoscopically inspected under cocaine anæsthesia alone, was under Dr. Hill for laryngitis, and under treatment by Dr. Herschell for gastritis, &c. He had fasted for the last fourteen hours, and his stomach had been washed out by Dr. Herschell one hour previously to remove excess of mucus with which he was troubled, and which tended to obscure the view even in the absence of food. The principle of this new type of gastroscopy was to pass an open endoscopic tube of Killian's pattern through the mouth, pharynx, and œsophagus, under direct vision throughout, into the stomach. A cap with a tap for inflation at its side and a round aperture in its upper surface was then attached by means of a bayonet joint to the proximal extremity of the direct tube. Through the round aperture, which had a rubber lining, an indirect periscopic tube (which accurately fitted the aperture), with a terminal lamp on the cystoscopic principle, was passed into the stomach through the lumen of the direct vision outer tube.

Inflating was commenced before the inner optical tube reached the stomach. The members were shown the upper part of the pyloric antrum or vestibule and the pyloric orifice, and the adjacent portion of the lesser curvature all in the field at the same time. Examination of the cardiac orifice was best done with the outer inflating portion of the œsophago-gastric apparatus, with a slanting plane glass window at the proximal end to prevent escape of air when inflating; this was Dr. Hill's form of direct inflating gastroscope. The rest of the stomach, more especially the lesser curvature and pyloric vestibule, the most frequent sites of election of morbid lesions, were more efficiently examined by the indirect vision periscope, on the cystoscopic principle, inserted through the outer tube. All *purely indirect* methods of gastroscopy which had been used hitherto had necessarily involved the *blind* introduction of either a rigid or a flexible apparatus through the œsophagus into the stomach. Such shortcomings in endoscopic practice could not, of course, be acceptable to careful œsophagoscopists. If the lower end of the gullet was readily accessible to inspection by a straight direct vision endoscope, so of course was the stomach (though Mikulicz denied this long ago, and Souttar had followed Mikulicz more recently), and its practicability was proved long ago by Killian and Chevalier Jackson, and that was obviously the only correct and safe way to approach the stomach, the indirect apparatus being afterwards inserted with perfect safety through the protecting outer direct tube. Mikulicz's practice thirty years ago was to pass blindly an angular, rigid, indirect vision periscope through the gullet into the stomach with such occasional casualities as might have been expected, and on that account the method had for many years been regarded as obsolete until recently resuscitated by Souttar with slight modifications (which included the revolving nozzle of Kelling and the rubber tip of Rosenheim), and, as might again have been anticipated, history was repeating itself as regards perforations. This element of risk and want of conformity to sound endoscopic principles had been pointed out to Mr. Souttar and Dr. Theodore Thompson by both Dr. Hill and Mr. Waggett in the *British Medical Journal*[1] more than a year ago, but Mr. Souttar still employed and invited others to employ this blind and discredited method of traversing the œsophagus and cardiac orifice regardless of the possible presence of unsuspected lesions. Less objectionable than the Mikulicz angular rigid gastroscope were the straight, indirect gastroscopes of Rosenheim and the flexible instrument of Kelling. These instruments,

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[1] *Brit. Med. Journ.*, 1909, ii, pp. 1096, 1195.

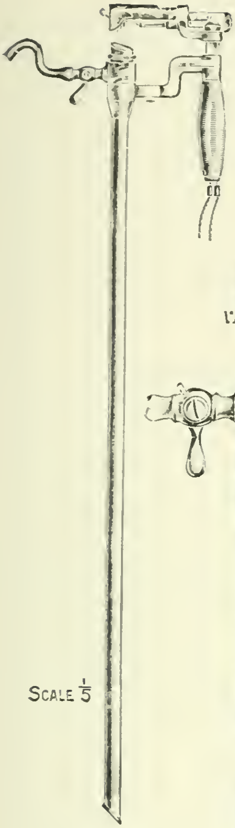

FIG. 1.

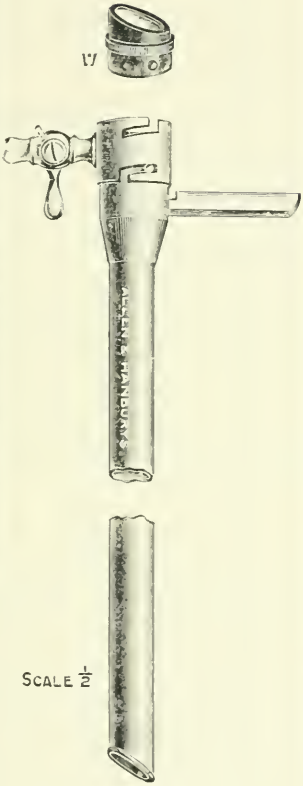

FIG. 2.

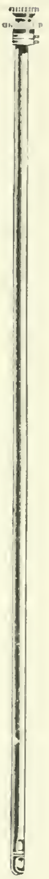

FIG. 3.

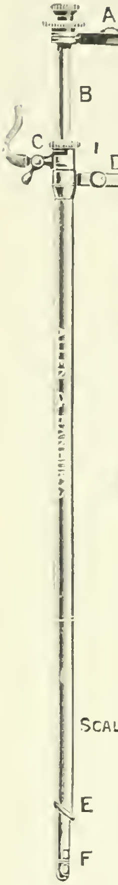

FIG. 4.

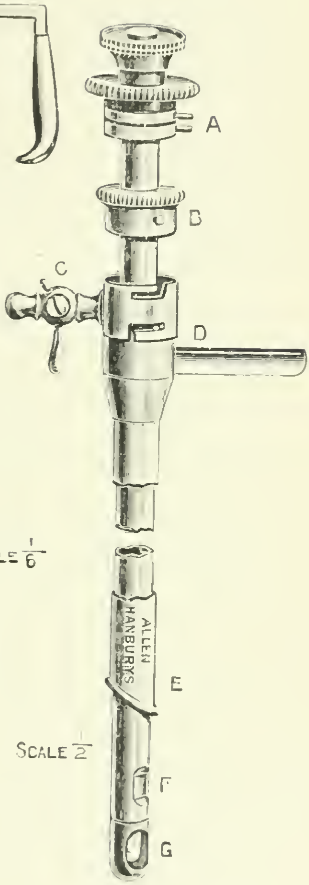

FIG. 5.

Figs. 1 and 2.—Hill's direct vision inflating œsophago-gastroscope with Brünings' hand-lamp attached. W, window; below this is a tap and nozzle to which can be attached an inflating bag. After the instrument has been passed under direct vision into the stomach, the hand-lamp is removed and the handle D in fig. 4 substituted. The window, W, is also removed, and the perforated cap, B, in fig. 5 is substituted.

Fig. 3.—Indirect vision optical (periscopic) tube on same principle as cystoscope.

Figs. 4 and 5.—Hill and Herschell's combined direct and indirect œsophago-gastroscope. The indirect vision periscopic tube, fig. 3, is shown within Hill's direct inflating œsophago-gastroscope. A, switch and detachment for illumination. F, window of periscope. G, electric lamp.

again, were passed once or twice too often in diseased conditions near the œsophago-gastric junction, and they in their turn became obsolete. They were succeeded by the adoption on the Continent during the last few years of a relatively much safer, but far from absolutely safe and sound, method of gastroscopy which involved the principle of first passing a half-rigid, half-flexible tube of gum-elastic or of soft rubber into the stomach, which acted as a guide for the subsequent insertion of a straight optical (periscopic) apparatus. Lœwenstein's and Lœning's gastroscopes were on this principle, and, although less objectionable than their predecessors, were far from conforming to the endoscopic standard demanded by responsible œsophagoscopists, who resented the gross manner in which the gastroscopists had abused their privilege of "right of way" through œsophageal territory which the laryngologists had but recently annexed. The result of all this bungling on the part of the gastrologists was that an œsophagoscopist had at last been impelled, in the interests of humanity, to associate himself with a progressive gastrologist, Dr. Herschell, in order to put gastroscopy on a sound basis. It followed as a natural consequence that the œsophagoscopists, having got below the diaphragm, would regard the cardiac end of the stomach as within their sphere of influence equally with the gastrologists, and it depended very much on the amount of enterprise shown by the latter whether they were left in undisputed possession of even the pyloric region of the viscus.

For serious gastroscopic work general anæsthesia was usually necessary. Although the stomach was fairly anæsthetic, it was rather intolerant to over-distension by forcible inflation, so that under cocaine anæsthesia the patient might have vomiting movements set up, and an effort made to eject the œsophago-gastroscope before the stomach was sufficiently over-distended to permit of a thorough examination. For this reason gastroscopy under local anæsthesia would and could be undertaken only exceptionally in selected tolerant cases. The man who had just been twice gastroscoped for ten minutes each time, to enable all present to look at the region of the pylorus, had only had the œsophago-gastroscope passed once previously to ascertain if he were tolerant or not; he had, however, been œsophagoscoped on several occasions. Dr. Hill had no hesitation in confidently recommending this method of œsophago-gastroscopy as worthy of adoption by all who had become really expert in œsophagoscopy. The method of œsophago-gastroscopy was so simple and obvious from an endoscopic point of view that it was astonishing that it had been left to him and Dr. Herschell first to put it in practice.

The straight periscope had been obtained from Heyneman, of Breslau, and was similar in many respects to that in Lœning's, though smaller in diameter; and the rest of the apparatus had been constructed by Messrs. Allen and Hanbury to the order and specifications of Dr. Herschell on the principle at first outlined by Dr. Hill, in controversy with Mr. Souttar, in October, 1909 (*British Medical Journal*[1]). The Mikulicz-Souttar, the Lœwenstein, and the Lœning instruments were exhibited for purposes of comparison.

DISCUSSION.

The PRESIDENT congratulated Dr. Hill and Dr. Herschell on the ease with which the instrument was passed. He was surprised to see how readily the patient tolerated this endoscopic method, and was much gratified by the excellent view obtained of the pyloric end of the stomach.

Mr. HERBERT TILLEY asked what position the patient was placed in when the instrument was passed under general anæsthesia. For examinations of the larynx, trachea, and œsophagus, he adopted the left lateral position, because it brought those parts nearer to the surgeon. It was much preferable to the dorsal method, with the head extended over a table.

Dr. PETERS asked what was the method of preparation of the stomach previous to the examination.

Dr. StCLAIR THOMSON congratulated Dr. Hill on his most convincing gastroscopic demonstration and inquired if he or Dr. Herschell had as yet had an opportunity of diagnosing morbid lesions in the pyloric region by this new method.

The PRESIDENT confirmed Dr. Tilley's remark as to the convenience of the left lateral position. For the more recently introduced instruments he thought the right lateral had certain drawbacks.

Dr. HERSCHELL, answering Dr. Peter's question, said that in gastroscopy it was essential to secure an empty stomach by as long a period of abstention from food as possible, say, twelve hours, and not to perform preliminary lavage unless there was known retention of food residues. For this reason the examination was most conveniently carried out before the patient broke his fast in the morning. If, however, it was seen after the introduction of the gastroscope that the stomach was not quite empty of food or contained frothy mucus, the optical (indirect vision) rod should be temporarily removed and a rubber stomach-tube passed through the outer (direct vision) tube, and the stomach washed out in the usual way. The residuum of wash-water should be evacuated to the last drop by means of an aspirator, the most suitable one for this purpose, in his experience, being Sonoran's, made by Gentille, of Paris, and used in conjunction with the red rubber stomach-tube of the same maker. As a matter of fact, any kind of ordinary rubber tubing which would pass through the open outer tube of the gastroscope might be used, as the difficulties attending the passage of an open-ended rubber tube down the œsophagus were, of course, absent. If any fluid at all were left in the stomach, it was liable to be churned up into a froth when inflation was commenced, and this obscured both the direct and indirect vision picture.

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[1] *Brit. Med. Journ.*, 1909, ii, p. 1096.

Dr. HILL, in reply, said that, after reading Mosher's paper three years ago, he had tried the Mosher position, to which Dr. Tilley alluded, and for some cases it was very good—for example, right bronchoscopy. In gastroscopy, however, the left lateral position was the least desirable of any. Under general anæsthesia it was almost essential to employ the dorsal position with the gastroscope at the right angle of the mouth, because the stomach was chiefly on the left side of the mid-line. The various antero-posterior curves of the spine had to be manipulated according as we desired to move the tip of the instrument near to the anterior or to the posterior wall of the stomach, and lateral bending of the spine was necessary for lateral movement of the tip, hence the dorsal position was the best, so that we could more accurately judge of the position of the spine. As yet he did not undertake to investigate and interpret morbid lesions of the pylorus, though when he examined the cardiac end of the stomach which was within his "sphere of influence" he liked, out of curiosity, to have a look at the pyloric end. The method he had just demonstrated was in every way safe for practised œsophagoscopists—but only for those. He did not as yet venture to recommend it for acute ulcer, or acute hæmorrhage of the stomach, for fear inflations and manipulations might do damage; but in chronic ulcer it should be useful to ascertain whether a condition was malignant or not. It probably would be of real service where the gastroscopist and the surgeon co-operated at the operating table, as Dr. Hertz had pointed out. It was, he claimed, a substantial advance in gastroscopic technique.

A Case of Ulceration on the Left Tonsil.

By SOMERVILLE HASTINGS, M.S.

THE patient, an engine fitter, aged 56, came to the Middlesex Hospital at the end of November, 1910, complaining of a pain in the left side of his throat shooting to the left ear, which had lasted for two months. He gave no syphilitic history, but told us that he had had a rash on his body about three months previously. A note that he brought from his doctor recorded the fact that Klebs-Löffler bacilli had been searched for, but had not been found. The right tonsil was in no way abnormal, but the left was swollen and ulcerated. The ulceration extended to the anterior pillar of the fauces, and the surface of the ulcer was covered to a large extent by a whitish membrane. There was no induration to be felt, nor was any enlargement of the glands in the neck detected. Pyorrhœa alveolaris was severe.

Mercury and iodides were ordered and a mouth-wash given. Cultures were taken from the tonsil and a small piece was removed for microscopical examination.

The condition of the left tonsil is to-day very much the same as when first seen, in spite of careful treatment. On December 9 the man complained of feeling ill and called my attention to some well-marked examples of erythema nodosum on his legs. These speedily cleared up under aspirin. Streptococci and staphylococci have been grown from the tonsillar region and numerous bacilli were found in film preparations. Some of these were Gram-positive and resembled Hoffmann's bacilli. The first piece of tonsil removed showed thickening of the epithelium on microscopical examination, which rather suggested malignancy, but a second, removed a few weeks later, gave evidence of inflammatory changes only, the epithelium being infiltrated with polymorphonuclear leucocytes. Wassermann's reaction was negative.

DISCUSSION.

Mr. H. BETHAM ROBINSON felt no doubt that it was a carcinoma. The patient had enlarged glands in the neck, with considerable infiltration around, and also some free glands. It was not satisfactory from an operative point of view.

Dr. KELSON pointed out the close resemblance between the white deposit and that seen in certain forms of intralaryngeal carcinoma.

Mr. HASTINGS, in reply, remarked that against malignancy was the fact that on two occasions he had snipped out pieces of the growth and microscoped them carefully. The first specimen was suggestive, but he did not think it was malignant. The second piece was clearly not. Moreover, there was no induration; the tonsil was soft to the touch, and had not changed for twelve weeks; he had watched the patient during that time. It would be best to make a fairly free excision, he thought. He would bring the case again, or show specimens of it.

Plaster Casts of Unusual Malformation of Upper Jaw.

By HERBERT TILLEY, F.R.C.S.

FROM a man, aged 32, who was brought for "congenital cleft palate, constant cough, inability to breathe through the nose, swallowing the tongue at night, and excessive expectoration of mucus in the morning." Examination showed that the roof of the hard palate was intact and V-shaped, the narrow end of the V forming the roof of a deep median cleft, the sides of which consist of the hypertrophied alveolar borders of the upper jaw. The width of the median cleft between the alveoli is less than ½ in., and the breadth of the right alveolus is ¾ in. The lower molar teeth point inwards against the side of the tongue, and the lower incisors project beyond the upper ones. There was well-marked alar collapse and much septal irregularity.

DISCUSSION.

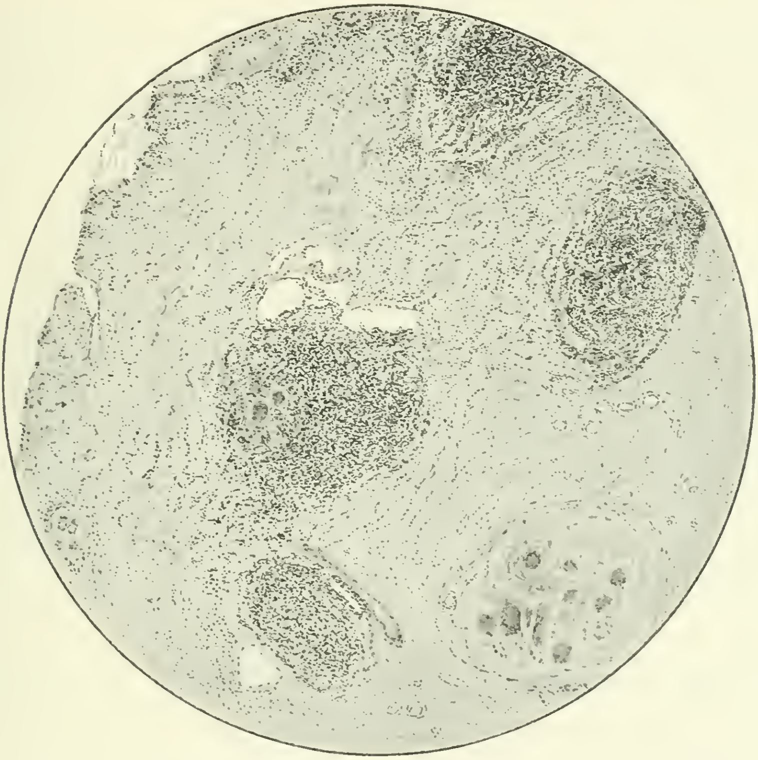
Dr. DONELAN thought the case would be better described as malformation of the alveolar border. He believed such cases were largely due to long-standing pyorrhœa, dental experience being that the thickening was largely fibrous, not bony. The patient from whom these casts were taken was remarkable on account of the large deposit of true bone. He hoped Mr. Tilley would be able to show him on some other occasion.

Mr. HERBERT TILLEY, in reply, called attention also to the displacement of the molar teeth in the lower jaw. There was extensive pyorrhœa of all the teeth, and this condition was being attended to. He wondered whether the breadth of the alveolus was due to the sepsis being absorbed. The patient was an intelligent man. He had narrow nostrils and a deviated septum, a lantern jaw, and a very narrow face. No treatment was advised beyond that of the pyorrhœa, and the suggestion that the patient should wear wool balls inside the nostrils during sleep to expand the vestibule.

Case of Tuberculosis of the Vocal Cords in a Young Man.

By L. H. PEGLER, M.D.

PATIENT, aged 21, sorter in a fur-dyer's warehouse, came to the hospital in April, 1910, complaining of loss of voice, a harsh whisper having gradually replaced phonation. Examination showed the cords, when cleansed of secretion, to be much ulcerated, the right one also showing a characteristic longitudinal splitting. They approximated very imperfectly, though the vocal bands approached a good deal on attempted phonation. There has been very little alteration since. There was a ridged deflection of the septum to the left, which was corrected submucously, and an adenoid mass was noted, though not removed at that time. Last November much more secretion was seen to be dropping down upon the glottis, and then it was found that the adenoid mass was much ulcerated. At this period the secretion was examined for tubercle bacilli and the chest again stethoscopically, both with negative result. The adenoid mass was curetted, and sections (of which one is shown under the microscope) displayed many giant-cells in the ulcerated part, but no tubercle bacilli could be found (*see* fig.). The nasopharynx healed slowly, and from the granulating surface a fragment was removed with the curette at Mr. Somerville Hastings's clinic for purposes of investigation. Two guinea-pigs were inoculated with the material and soon became infected with tubercle, which invaded the inguinal and popliteal glands, the lungs, and spleen. A preparation of one of the animals is shown on the table. Under the microscope is placed a slide of stained scrapings from one of the glands, showing tubercle bacilli.



Section of tuberculous adenoid. On the left of the drawing is seen the ulcerated surface; the invasion has taken place here. The groups of cells are lymphoid follicles, one is completely replaced by giant-cell systems; a small group of giant-cells is seen below on the left, they were very plentiful in this section. Though the tubercle bacilli are not visible, they were abundant in preparations made from guinea-pigs inoculated with this material.

The diagnosis of the case has thus been settled, but the lad, who is now installed as clerk in the same firm, has no constitutional symptoms, and declares that he never felt better in his life.

DISCUSSION.

The President thought there would be benefit from the use of tuberculin. He suggested a bacillary emulsion, $\frac{1}{5000}$ grm., and, in the absence of marked reaction, rapidly increasing the dose.

Dr. Pegler, in reply, said he had no recollection of a specimen of tuberculous adenoid growth being shown to either the old or new Society, but he had been asked that afternoon to allow a drawing of a section to be published with the case, and that he proposed to do. As regards treatment, he would be glad to carry out the President's suggestion of tuberculin injections.

Tuberculous Ulceration of the Larynx.

By L. H. Pegler, M.D.

H. G., male, aged 42, has complained of hoarseness on and off for a year, worse the last three months. No pain, no difficulty in swallowing, thinks he is losing weight: denies syphilis. Examination shows a large excavated ulcer involving the base of the left ventricular band, vocal cord, and arytenoid region; small punched-out ulcer with regular border on right ventricular band at a spot one-third of its length from the anterior commissure; fixation of left side of the larynx. No enlarged glands in the neck. Has taken iodide of potassium for three weeks without any benefit.

Extensive Malignant Ulceration of Pharynx and Tonsils.

By L. H. Pegler, M.D.

G. W., male, aged 58, has complained of pain in the right side of the throat and in the right ear (worse on swallowing) for three months. He is losing weight. Examination shows extensive ulceration involving the right tonsil, anterior pillar of the fauces, and posterior part of the soft palate extending to the middle line and also encroaching upon the base and right side of the tongue. There is a deeply excavated ulcer in the region of the supra-tonsillar fossa. The edge of the growth is raised, hard to the touch, and nodular. Some deficiency in movement of right half of soft palate; slight dextral deviation of tongue on protrusion. No enlarged glands in the neck. Patient has been taking 45 gr. of the iodide per diem since December 15, 1910.

The PRESIDENT regarded the case as malignant, and Mr. Robinson was of the same opinion.

Primary Carcinoma of Tonsil.

By E. A. PETERS, M.D.

S. A. H., AGED 38, private patient, came to me for enlarged glands and slight sore throat in November, 1910. In 1907, at the advice of a surgeon, an abscess in the jugular region had been opened, but the glands had steadily enlarged since, and in November, 1910, were felt as a nodular, hard mass in the region of the deep cervical glands usually enlarged in tonsillar affections, and extending upwards, so that palpation of the tonsil revealed this organ as a thin layer covering, apparently, a hard gland. The tonsil itself measured about 1¼ in. across, was dark red, with slightly indefinite margin, with absence of crypts; there was no ulcerated surface.

The tonsil was dissected out on November 24. It was more friable than usual, but the capsule was definite and only on microscopical examination were the squamous cells apparent. The microscopical section shows that practically the whole tonsil is affected, though there is no ulceration. The deep cervical and submaxillary glands, with typical degenerative cysts, were removed on December 13, and also the mass on the outer side of the superior constrictor. This mass had extended up to the base of the skull and round the vessels.

Dr. Ellis, who has had charge of the patient, has now placed him under X-ray treatment. It is suggested that the growth of the tonsil was primary, though not obvious to the naked eye, and that these may be the usual clinical features in such cases of glands of the neck where the primary source is not apparent.

Microscopical section shows typical squamous carcinoma.

Dr. PETERS added that he showed the case to call attention to an important series of cases of malignant glands of the neck in which the fauces were generally swollen. The tonsil was dark red and not ulcerated. In the books, unless an ulcer was detected it was laid down that the primary source was unknown. In this particular case enucleation left a' smooth capsule. It was only histological examination which determined the tonsil to be a mass of growth. It was possible that histological examination early in suitable cases would lead to recognition of more primary carcinomata of the tonsil.

Swelling at Junction of Right Vocal Cord and Apex of Vocal Process.

By JAMES DONELAN, M.B.

MAN, aged 52, professional singer, had influenza during first weeks of November, 1910. Seen on December 8. Had hoarseness, marked hyperæmia of right vocal cord and vocal process. This subsided under rest and sedative inhalations, and the hyperæmia had disappeared save for a slight areola surrounding a clear herpetic-looking vesicle on the apex of the vocal process. This ruptured, leaving a circular divided surface which in a few days covered over with epithelium. On December 22 there appeared to be more of this epithelium than one would expect in a normal process of cicatrization. Patient continued silent until January 14, 1911, when, as the surface appeared healed over and very healthy, he was allowed to speak, and to take part in some quartets, singing very little, and only for a few minutes on three or four occasions. On January 25, when he was next seen, hyperæmia of the right vocal cord had returned, and the epithelium at the point referred to had rapidly increased, assuming the size and shape of half a lentil. Patient complains of pain on swallowing in the right side of neck at the cricoid level. On this date there was no apparent impairment of mobility in the vocal cord.

DISCUSSION.

The PRESIDENT thought the case should be closely watched, as it might turn out to be malignant, although he considered it was most probably a simple inflammatory lesion.

Dr. FITZGERALD POWELL said he regarded this case as of "inflammatory origin." The larynx presented the appearance of a chronic posterior laryngitis. He thought attention should be paid to the nasal condition, the larynx painted with solution of chloride of zinc, and the patient have six months' rest for his voice.

Papilloma of Larynx in a Child.

By DAN MCKENZIE, M.D.

A LITTLE girl, aged 8, came to hospital six months ago with hoarseness and stridor. Inspection of the larynx gave a glimpse of a rough growth, the exact seat of which was uncertain. The obstruction to breathing being very great, tracheotomy had to be performed before the growths in the larynx were attacked. After tracheotomy several papillomata were removed by the direct method. On one or two subsequent occasions other small portions have been removed, chiefly from the subglottic region in the front. Dr. Wyatt Wingrave reports the growths to be papillomata. Several of the growths were exhibited, including one fairly large in size, the first to be removed.

DISCUSSION.

Dr. BRONNER asked whether Dr. McKenzie had used a formalin spray in these cases. He started with ¼ per cent. strength, working up to 1 per cent. When the growths were removed by the direct method he applied 1 to 10 per cent. formalin on a probe.

Dr. McKENZIE, in reply, said that, like Mr. Tilley, in removing the papilloma by the direct method he found it more easy if he turned the child over on the side. The best tube to use was a large one, as it greatly facilitated the removal.

Lingual Thyroid.

By GEORGE W. BADGEROW, F.R.C.S.Ed.

FEMALE, aged 17, patient came to the Throat Hospital complaining of hoarseness and thickness in speech which had been gradually coming on this last two years, adenoids removed recently with some improvement in speech. The vocal cords are seen with difficulty owing to overhanging epiglottis. Hemispherical swelling over the base of the tongue which is hard to the feel.

DISCUSSION.

Mr. STUART-LOW said the case was similar to one which he showed two years ago, though the tumour was larger, and had existed eight years. That woman had very similar symptoms. The diagnosis turned upon either dermoid cyst or thyro-lingual cyst. Probably there was a small thyroid gland present in the neck, and therefore a very hard-working one, as existed in his case. He presumed Dr. Badgerow would operate upon his case. The procedure he (Mr. Stuart-Low) followed was laryngotomy first, then to split the tongue in the middle, and stitch it up after taking away the growth. There was not much hæmorrhage. He plugged the larynx, and rubbed the side of the tumour well with chloride of zinc solution. There had been no recurrence, and the patient did well.

Dr. FITZGERALD POWELL remarked that the girl did not suffer inconvenience, and there were practically no symptoms. He agreed with what he believed was Dr. Badgerow's opinion, that unless the thyroid got larger and caused disagreeable symptoms, it would be better not to interfere at present. He assumed that Mr. Stuart-Low operated in his case because the tumour was large and was causing trouble in breathing or swallowing, and would have done so whether or not she had a normal thyroid gland.

Mr. ROBINSON did not think the diagnosis of lingual thyroid was absolutely certain. On examination the tumour seemed to be more inclined to the left side of the root of the tongue, though it did extend over the middle line. It was very hard and near the surface. She had also a suggestive hard lymphatic gland on the left side of the neck. Possibly it was a lymphosarcoma.

Recurring Aphonia in a Woman aged 36.

By G. C. CATHCART, M.D.

Loss of voice first came on in 1905 after a severe attack of influenza. Patient was treated at a hospital, and after two months, being no better, went to the country, where she stayed three months. From there she went into Mount Vernon Hospital, and, after being kept in bed for six weeks, the voice came back with several applications of the inter-laryngeal electrode. She was kept in the hospital for six weeks longer, undergoing the open-air cure. The voice has been lost three or four times since, but each time it has been brought back with electricity. Last year the patient was confined to her bed three times with attacks of rheumatism. The aphonia came on last September and has continued ever since. The case is an interesting one, as, apparently from her treatment at Mount Vernon, she must have been considered at one time tuberculous. She complains of great pain in the throat and constant cough, but there is no sweating or loss of weight, and von Pirquet's test is negative. On laryngological examination it is seen that the mucous membrane is swollen over the arytenoid and in the inter-arytenoid space, and the cords do not meet properly. The throat has been treated locally, and the anti-rheumatic remedies have been pushed, but the aphonia, pain and cough still continue.

The PRESIDENT agreed with Dr. Jobson Horne that there was a danger of overlooking latent commencing tuberculous disease.

Case of Malignant Stricture of the Œsophagus.

By P. WATSON-WILLIAMS, M.D.

A. P., MALE, aged 64, had noticed increasing difficulty in swallowing for three or four months, but much more markedly during the previous six weeks, with consequent loss of flesh. Was unable to swallow even fluid flood when seen on January 10. Subsequently, œsophagoscopy revealed an infiltrating growth 10½ in. from the incisor teeth. Œsophagoscopy in the left lateral position under general anæsthesia rendered it possible to pass a bougie, size 8 (5 mm.), into the stomach, after which the patient could swallow fluid food freely.

DISCUSSION.

Dr. W. HILL said that he had just œsophagoscoped the man at the President's suggestion. There appeared to be every evidence of malignancy, but he suggested examination under a general anæsthetic, the stricture being dilated up, and if possible the lower limits of it ascertained, and after swallowing bismuth emulsion, an X-ray picture taken. If the strictured area was not too long, the dysphagia would probably be much relieved by adequate radium treatment. The longest time during which radium had been applied continuously in the cancerous gullet in his practice was twenty-eight hours, but there could be two applications of fifteen hours each on consecutive days with advantage in some cases; after a six weeks' interval the treatment was repeated and appeared to do good. He would arrange for the admission of the patient into St. Mary's Hospital for the application of radium.

The PRESIDENT, in reply, said that when the patient was put under a general anæsthetic two days ago, no larger bougie than No. 8 (5 mm.) would pass the stricture. If Dr. Hill could use radium in this case, the patient and he would be thankful. Nothing was so likely to prolong the patient's life.

Epithelioma of the Epiglottis.

By NORMAN PATTERSON, F.R.C.S.

THE patient, a man, aged 40, has noticed pain on swallowing solids for the last two months, and during the last month pain just after the swallowing of saliva and other fluids. The pain shoots from the region of the tip of the great cornu of the hyoid on the left side towards the back of the ear. There is a tendency to the accumulation of saliva in the throat. The condition is seen to affect the anterior and to a less extent the posterior surface of the left portion of the epiglottis. It is just spreading on to the back of the tongue and corresponding aryteno-epiglottic fold. The lower portion of the larynx appears healthy. There are some palpable glands in the neck, especially on the left side, which are probably to be accounted for by the septic condition of the teeth.

Opinions are asked as to the best operative procedure to be adopted in this case.

DISCUSSION.

Mr. ROBINSON agreed with Dr. Horne that there was considerable glandular involvement in the neck, and it would require a big operation to clear the disease out. Probably also the lymphatics were involved to the base of the skull.

Mr. PATTERSON, in reply, said he did not think the disease extended far down into the larynx; the left cord moved equally as well as the right. Possibly the enlarged glands in the neck were due to the bad condition of the teeth: they were present on both sides.

Primary Sore of the Vestibule of the Nose and Adjoining Skin.

By NORMAN PATTERSON, F.R.C.S.

FEMALE, aged 38. The patient came to hospital on January 6 with a hard, indurated area, irregular, on the surface in the above-named region (*see* Plate). The lesion gradually increased in size and was accompanied by some enlargement of the glands in the submaxillary region. There has been a great deal of pain extending on to the face. I am much indebted to Dr. Sequeira, to whom I referred the case, for the trouble he has taken in its investigation. Spirochætes have been found in the serum from the lesion and a positive Wassermann reaction has been obtained.[1]

Large Growth in the Larynx.

By H. W. FITZGERALD POWELL, M.D.

MALE, aged 48. Six months' history of difficulty in breathing, coming on suddenly. Examination shows large mass of growth involving left side of larynx, filling up the cavity of that organ, involving tongue and aryepiglottic fold. The patient has been a soldier and there is a history of syphilis. The Wassermann reaction is negative.

Suggestions as to treatment were invited.

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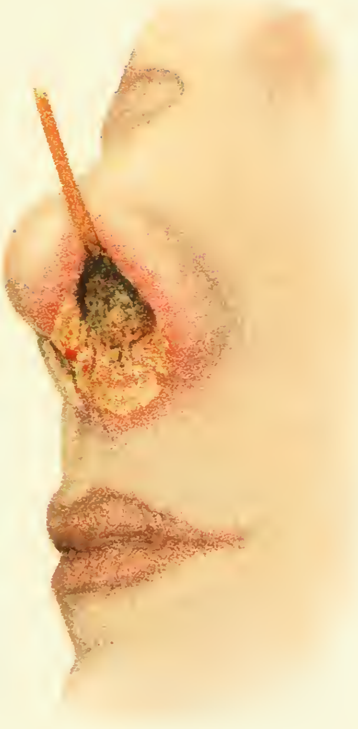
[1] NOTE.—Seven hours after treatment with "606" by Dr. Sequeira the spirochætes had disappeared. The lesion cleared up with extraordinary rapidity and there were no secondaries.

PROC. ROY. SOC. MED.
Vol. IV. No. 5.
Laryngological Section.



NORMAN PATTERSON: *Primary Sore of the Vestibule of the Nose.*

DISCUSSION.

Dr. H. J. DAVIS regarded it as malignant; there was enlargement of glands at the angle of the jaw, and the base of the tongue was involved. The man would probably live longer if only tracheotomy were done.

Mr. ROBINSON regarded it as extensive malignant disease, and there was involvement of the lymphatic glands as far as the clavicle on the left side. He would advise that nothing but tracheotomy should be done.

Dr. FITZGERALD POWELL, in reply, thanked the members for their opinion, with which he was in agreement. He thought that the growth was much more extensive than it appeared to be on cursory examination. He thought it involved the whole side of the larynx, the arytenoids, and aryepiglottic fold on left side, and extended into the pyriform fossa and on to the tongue. The patient was a fairly strong middle-aged man, and, if it had been thought at all possible to eradicate the growth by excision of the larynx and removal of the glands, he would have made an effort to do so. He proposed introducing a tracheotomy tube and removing a portion of the growth for microscopical examination.

Tuberculosis of the Nose and Cheek.

By WILLIAM HILL, M.D.

BOY, aged 4, had been operated on for tuberculous disease in the malar region and bone removed by Mr. Waugh at Great Ormond Street Hospital a year ago. Since then he had been attending the therapeutic inoculation department at St. Mary's Hospital. Dr. Hill had been asked to examine him the previous day, and a tuberculomatous condition occupying the position of the septal cartilage was seen; the cheek was swollen, and there was a sinus above the first bicuspid tooth passing up to carious bone in the region of the canine fossa. It was proposed to perform a modified Rouge's operation so as to remove the disease in the nose and maxilla, but any advice would be appreciated. The case resembled, as far as the nose was concerned, cases of nasal tuberculosis shown by Dr. Pegler.

Dr. PEGLER said he was particularly interested in this case, which resembled one very closely that he had shown to the Section in 1909[1]—a primary tuberculoma of the septum. He thought by far the most simple and effective treatment was excision of the growth with the portion of triangular cartilage from which it arose. Even if curettage were had recourse to now, the former more radical method would, according to his experience, only be deferred.

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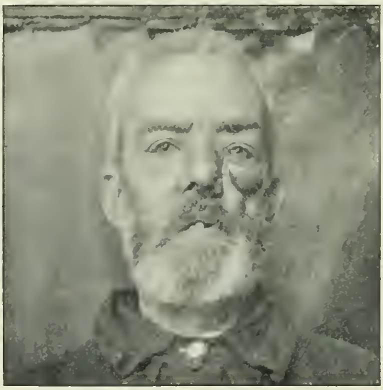
[1] *Proceedings*, 1909, ii, pp. 39, 131.

Removal of the Upper Jaw in a Man aged 71.

By H. J. Davis, M.B.

The upper jaw was excised on November 30, 1910, for malignant disease of the antrum following polypi of many years' duration and antral empyema. The notes and history of the case were published in December, 1910,[1] and a specimen of the growth invading the antral walls was also exhibited. The patient is now shown: it will be seen that œdema of the lower eyelid persists. Mr. Bishop Harman excised the lachrymal sac three weeks ago and inserted setons subcutaneously to drain the lid. The growth involving the antrum was a sarcoma; an obturator remains to be fitted to fill the gap. The patient is well, and has rapidly put on weight.



Removal of upper jaw for sarcoma of antrum; showing persistent œdema of the lower eyelid.

The President congratulated Dr. Davis on the successful result.

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[1] *Proceedings*, p. 30.

Laryngological Section.

March 3, 1911.

Dr. P. Watson-Williams, President of the Section, in the Chair.

International Congress of Laryngology, Berlin.

The President, as their representative on the Executive Council of the Congress, desired to draw the attention of all Members of the Section to the exhibition of instruments for œsophagoscopy and bronchoscopy that was being formed, and stated that the Committee would be glad to receive at once descriptions of such instruments devised by English workers, together with clichés for illustrating them in the catalogue, and specimens of bodies removed, &c., which should be forwarded to Professor Hegmann, Lützowstr. 60, Berlin.

Specimens of Equine Larynx, and Records of Cases illustrating the Results of the "Ventricle-stripping" Operation for the Relief of the Conditions known as "Whistling" and "Roaring" in Horses. Demonstrations of Instruments specially designed for this Operation.

By Frederick Hobday, F.R.C.V.S.

The respective conditions known as "whistling" or "roaring" in horses are due in the majority of cases (fully 95 per cent. and probably 98 per cent.) to partial or complete paralysis of the left vocal cord. Occasionally both cords appear motionless when the interior of the larynx is exposed, but in no instance out of 257 individual patients operated upon by the author since September, 1910, has the right vocal cord alone been observed to be paralysed.

The nerve supply is from the recurrent laryngeal, and in a bad roarer the muscles of the left side will be found to be atrophied to such an extent that often there will scarcely be a vestige of muscle-tissue discernible. Numerous attempts in the way of nerve-grafting and the application of massage and electricity along the course of the nerve have been tried, but have always been unsuccessful, and in the way of surgery many operative measures have been used. Total excision of the vocal cord, arytænoidectomy, pinning back the paralysed cord, and various other means for getting the offending organ out of the way, have been tried, sometimes with temporary success, but scarcely ever with permanent good result. The wall of the larynx has collapsed afterwards or granulation tissue has appeared, and the patient has made more noise, and been in more distress, than ever.

The "ventricle-stripping" operation offers none of these disadvantages as, although requiring a very delicate method of procedure, it is simple, and gives no need for injury to cartilage or other of the stronger and supporting structures of the larynx. It simply consists in removing the mucous membrane completely from the pouch or ventricle at the back of the vocal cord, with the idea that two raw surfaces will thus be made, and that the two sides will adhere together and thus completely obliterate it. If this follows, the paralysed vocal cord, which forms one boundary of the ventricle, must of necessity be retracted out of the lumen of the larynx and become firmly adherent to the wall of this organ. When permanently fixed out of the way the vocal cord no longer acts as a foreign body, and the increased space in the lumen of the larynx enables the horse to breathe freely and without distress, even when submitted to violent exertion.

Originally suggested by some experimental work done by Professor Günther of the veterinary school at Hanover in 1834, the matter was unfavourably reported upon, probably through some fault in the technique then employed. Taken up again in 1906 by Professor Williams, the Professor of Surgery in the Veterinary Faculty of Cornell University, U.S.A., the operation was again performed several times with varying success, and in September, 1910, this gentleman demonstrated it upon two patients in England. Since then the author of this communication has operated upon more than 250 cases with most gratifying results. A paper recording some 112 satisfactory cases was recorded in the *Veterinary Journal* for January of this year, and, to the author's knowledge, more than 200 horses (many of them absolutely useless for fast work or work involving great exertion) have been restored to usefulness and are now working satisfactorily and without distress. The breeds operated upon include racehorses, hunters, carriage and cart horses, and even ponies.

Originally the thyroid cartilage was sawn through when making the incision into the larynx, but it is now proved that the operation can be done without the slightest injury to cartilage by merely incising the cricothyroid membrane. Another improvement is also claimed by the author in that he now strips both ventricles at one and the same time, thus endeavouring to widen the lumen of the larynx to its fullest extent by obtaining adhesion of each cord to its respective side of the interior of the larynx. The operation is performed under the anæsthetic influence of chloroform, aided by the use of suprarenin and cocaine, and in several instances has been done with the aid of the local anæsthetic only.

The object of bringing it before the notice of the members of the Laryngological Section is that it may be discussed from a comparative aspect with a view to applying the operation, or some modification of it, to human patients, if this is not already done.

DISCUSSION.

The PRESIDENT (Dr. P. Watson-Williams) said it was very interesting to hear of the new method of treating "roaring" in horses, and it of course appealed to the members of the Section. Professor Hobday had brought the matter forward with the idea that it might possibly be applicable to the human being.

Sir FELIX SEMON expressed his gratitude to Professor Hobday for bringing the subject forward. He had taken an interest in it for many years, and had hoped that Professor Hobday would have been able to throw some light on two points—viz., the ætiology of the disease and the muscles which were first and principally paralysed. With regard to ætiology, up to a recent date there had been much discrepancy among veterinary surgeons themselves as to whether that paralysis was of myopathic or neuropathic origin. Almost always it was the left vocal cord which was paralysed ; and Colonel Fleming, late of the Army Veterinary Department (with whom he had had many talks on the subject), thought the main cause was probably an enlargment of the glands where the recurrent laryngeal nerve winds around the aorta. Colonel Fleming showed him (Sir Felix) a specimen which appeared to support that contention. But it was probably not the only cause. With regard to heredity, which the Professor had mentioned, it was well known that the celebrated horse "Prince Charlie" gave the disease to all his progeny. Stallions, especially those with long necks, suffered more frequently than did mares; climate had considerable influence, and ponies were but rarely affected. The information available was but patchy, and as the cause was not definitely known he would like to urge Professor Hobday, who evidently had good opportunities of seeing large numbers of animals, to direct attention to that point. In reference to the second matter, as to which muscles were first and principally paralysed, it had almost always been found that the posterior crico-arytænoid muscle on the left side was the one principally affected. It was an illustration in the animal of what he had tried to show in reference to man, that in all progressive organic paralyses which affected apparently the whole of the roots or trunks of the motor nerves of the larynx, it was the posterior crico-arytænoid which suffered first, or, it might be, even exclusively. He was interested to know whether Professor Hobday would corroborate that, so far as the disease in the horse was concerned. Even in total paralysis, it must not be believed that the paralysis was, *ab initio*, complete; because Mr. Clarke, whose work the Professor had referred to, laryngoscoped several horses, and found in two cases that what was originally abductor paralysis passed into complete paralysis of the recurrent laryngeal nerve, just as was seen in men. As the paper was principally surgical, and the Professor had thrown out the suggestion that the operations might be useful in the human subject instead of tracheotomy, he had already privately told the exhibitor that he must throw some cold water on the idea, because the suggestion was based on the assumption that the respiratory need was the same in all classes of mammals. But that was not so. Legallais had shown already, one hundred years ago, that in some species of animals, and especially the horse, and in the cat, the respiratory need was much greater than in other classes of animals—for example, dogs; so that horses were much more easily asphyxiated than dogs by the same degree of interference with the calibre of the respiratory tubes. Man was much more allied to the dog in that respect. Members of the Section knew well that unilateral abductor paralysis in man—which corresponded to "roaring" in horses—often enough was quite accidentally discovered, no respiratory or vocal symptoms having directed attention to the larynx at all. If, in such cases, distress in breathing was met with, it was due to a second stenosis by compression further down, such as that exerted by aneurysm or goitre, or by enlarged mediastinal glands. But unilateral abductor paralysis, and less still unilateral complete recurrent paralysis, did not cause dyspnœa, and so there would be no need for operation. If that very rare event, complete double recurrent paralysis, should occur, the patient was so ill—e.g., with advanced œsophageal carcinoma—that he could not move, and as he was in bed he did not suffer from dyspnœa, and so the question of operation would not come in. The only kind of case in which laryngologists had to perform operation for the relief of stenosis in connexion with paralytic trouble, was double complete abductor paralysis. But then the operation suggested by Professor Hobday would in man be only a choice of evils. In the horse, anything which relieved stenosis was of great benefit, but in man that relief would be very dearly bought, as it condemned the patient to total aphonia for the rest of his life. Personally, he would be inclined to advise the patient to tolerate the tube rather than have the voice reduced to merely a toneless whisper.

Dr. DUNDAS GRANT said there was still another reason for not expecting any great result from the operation in the human subject—namely, that, as had been so often shown, there was an extreme tendency for the regeneration of a membrane of a more or less cicatricial character, which was of much the same appearance as a vocal cord, and which afforded an obstacle to the free inspiration which Professor Hobday desired.

Dr. SCANES SPICER said Sir Felix Semon had expressed what would be the general opinion of human laryngologists on the problem raised—namely, that the fact of this operation destroying the voice put it out of court for man. In a paper in the *Lancet* some months ago, Professor Hobday had referred to the same as a cure for "grunting" in horses, so that the Professor was well aware that in the horse the operation he mentioned did destroy voice.

Dr. JOBSON HORNE said the important point in the ætiology of the disease was whether the paralysis was neuropathic or myopathic in origin. In the specimen exhibited the atrophy of the posterior crico-arytænoid muscle was well marked and out of proportion to the atrophy of the cord itself, which favoured the myopathic theory. The myopathic condition, he thought, was attributable to the shape of the neck. He did not think the operation which had been described would be of service to the human subject.

Dr. H. J. DAVIS asked what was the use of the vocal cords to the horse. Also, was it true that if one closed the nostrils of the horse it died of suffocation rather than open its mouth to breathe? There was a popular superstition to this effect.

Mr. CLAYTON FOX also thought the suggested operation would be inapplicable to the human larynx. There might be occlusion of the saccule, which would result in mucocele, and there would be as much trouble as before. In the case of bilateral paralysis, he would prefer to divide both recurrent nerves, to induce the cadaveric position, or do a thyrotomy, excise the thyro-arytænoid muscles, and seal up with sutures. In that way a breathing passage would be secured. The vocal functions would be a secondary matter.

Dr. BRONNER said he thought that in bilateral paralysis the operation suggested might be done on one side only.

The PRESIDENT added his thanks to Professor Hobday for bringing the subject forward. He would like to know whether the Professor had had any cases due to poisoning, for instance, by lead. In the Mendips there were districts in which were old disused lead-mines, in which water collected: cattle and horses drank from that water and had lead poisoning symptoms. If so, he asked whether those toxic cases showed the particular vulnerability of the left cord as compared with the right.

Professor HOBDAY, in reply, thanked the Section for the opportunity to show the specimens. He knew the difficulties in operating on the human larynx were greater than in the case of the horse, and the work was much more delicate; but in the smaller animals the veterinary surgeon also had delicate operations, and he was much indebted to surgeons who operated upon the human subject for hints in technique. In the case of canine pets, the veterinary surgeon now frequently supplied false legs, false eyes, and false teeth. If those animals were given back to their owners looking well, they were satisfied. But with a horse, except in the case of a valuable brood mare or a valuable stud animal, unless it could be returned to be of utility, the veterinary surgeon was not thanked. In the case of the horse it did not matter very much if there were granulations around the tube, so long as it was not a sufficient obstruction to block the breathing. But in the case of a hunter which was a roarer, the insertion of the tube meant the "beginning of the end," for, on the average, it was not of use for more than two seasons afterwards. It was the custom to take out the tube at the end of the hunting season and re-insert it before the beginning of the next. Another difficulty in regard to the horse with a tube was that it could not be taken through a stream. There was also the danger that the horse would, if turned out to pasture, rub its neck against a fence and break off the flange, so that some of the tube would go down the trachea. The tube had to be cleaned at least once a day, but it was difficult to get a groom to do it, as it was so offensive and objectionable. The crico-arytænoid joint was sometimes ankylosed, and the larynx in the horse was converted from a cartilaginous substance into something which was harder. The functions of the cords in the horse were for neighing; the loss of that capacity did not matter, so long as the other condition was cured. He had not met with cases of lead-poisoning which had caused roaring amongst horses. He could not be positive as to the origin of the condition, but he inclined to the view that it was neuropathic. Two points required consideration: the hereditary one, and, secondly, to try to do away with the disease known as "strangles," which caused enlargement of the glands in the region of the larynx. Stallions were more commonly roarers than geldings, and this condition was said to be met with in geldings more than mares. Statistics also showed that ponies rarely became roarers. It was said to be due to the fact that thoroughbred stallions were kept indoors the greater part of their lives. In some countries, where the stallion had greater freedom than in England (for instance, Australia and South America), roaring was comparatively seldom found.

Case of Carcinoma of the Larynx.[1]

By P. Watson-Williams, M.D.

L. T., female, aged 40. On November 7, 1910, was admitted to the Bristol Royal Infirmary with a view to operation, but on November 8 urgent dyspnœa supervened, and immediate low tracheotomy was performed. On November 9, under general anæsthesia, the larynx was opened in the mid-line, and the growth found to extend from the aryepiglottic fold downwards to midway between the lower margin of the thyroid ala and upper border of the cricoid cartilage, while posteriorly and anteriorly it reached almost to the mid-line. Its upper portion extended into the pyriform fossa, causing the swelling on the outer side of the aryepiglottic fold seen on laryngoscopy, here extending forwards to the edge of the epiglottis. The growth was removed by an incision about $\frac{1}{4}$ in. to $\frac{1}{2}$ in. beyond the apparent margin, therefore extending beyond the mid-line in front and behind, and laying bare the upper part of the cricoid cartilage on its left inner side, while above the arytænoid cartilage was removed entire, and a portion of the left side of the epiglottis.

The tracheotomy tube was removed as soon as the patient had recovered from the anæsthetic, and not replaced. Recovery was continuous and uneventful. There is considerable narrowing of the glottic lumen, due to the fact that the removed portion of the mucosa, &c., extended beyond the mid-line, and that a portion of the epiglottis, as well as the left arytænoid and aryepiglottic fold, had been removed.

DISCUSSION.

Dr. Scanes Spicer said he thought there was some indication of recurrence, not because of the large granulation, but because the thickening was so general on the affected side. He asked whether in the growth, &c., that had been removed the arytænoid was necrotic or ankylosed to the cricoid, and was it calcified? Had Dr. Williams removed any part of the cricoid cartilage at all, or of the thyroid? Had the larynx been X-rayed to see if and to what extent the cartilages were calcified? In a patient of his, one of cancer of the throat, shown here in January, the amount of calcification of the posterior arch of the cricoid cartilage and of the thyroid and arytænoid, was extraordinary—a circumstance which he considered to conduce to abnormal intrinsic irritation and neoplasia.

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[1] Shown at the November meeting, *Proceedings*, p. 16.

The PRESIDENT, in reply, said the arytænoid cartilage was not specially examined, but it did not appear to be necrotic. He would show the specimens, probably at the Bristol meeting. No portion of thyroid or cricoid alæ were removed, but he split the thyroid and cricoid. The growth extended down over the surface of the cricothyroid membrane on the left side, and he had to carry his incision down to the inner surface of the left half of the cricoid cartilage and forwards and backwards up to and beyond the middle line. He removed the arytænoid and the whole aryepiglottic fold and the left edge of the epiglottis, and removed the portion of growth which seemed to extend into the pyriform fossa. He was now carefully watching the granulations. The point about the granulations in the centre was that that was the spot least likely for recurrence.

Endothelioma of the Palate, with Metastatic Growth in the Neck, treated by Radium.

By J. GAY FRENCH, M.S.

THE patient, a woman, aged 63, came to the Great Northern Hospital early in May, 1910, giving a history of having first noticed a swelling in the roof of her mouth twenty-one months previously; this gradually increased in size, but gave rise to no pain or hæmorrhage. Three months after the appearance of this swelling she noticed another in the right side of her neck, which also continued to become larger.

On examination she was found to have a large ulcerating mass involving the whole of the right half of the soft palate. There was also a tense elastic swelling about the size of a pigeon's egg situated at the apex of the right posterior triangle of the neck, freely movable and not attached to the skin. Wassermann's reaction proved negative.

The patient was admitted into the hospital, and the tumour in the neck removed under a local anæsthetic. It was found to be situated in the fibres of the right sternomastoid muscle, these being spread out over the growth which was encapsuled and was easily enucleated. At the same time a small portion of the mass in the palate was removed for examination. Dr. Shaw's report was as follows: "Palatal growth, endothelioma. Cervical growth shows, macroscopically, soft breaking-down tissue within a fibrous capsule; microscopically, it is an endothelioma with a thin layer of lymphatic tissue at the periphery."

The patient had radium applied by Dr. Knox as follows: To the palate—May 28, four and three-quarter hours; May 29, seven hours;

May 30, seven hours. To the neck—June 3-4, twenty-four hours; June 9, three hours; June 10, eight hours; June 11, two hours. In July a twenty-four hours' application was given to both the palate and neck. Since then she has had a number of one-hour applications to the palate. The amount of radium used was 50 mgm., with a screen of 1 mm. of platinum.

It will be seen that all that now remains of the original growth is a small, pale, shallow ulcer situated on the right side at the junction of the hard and soft palates.

Two microscopical slides were shown.

DISCUSSION.

The PRESIDENT congratulated Mr. French on the great improvement.

Mr. HERBERT TILLEY stated that great improvement was often brought about by radium when applied in sufficient, i.e., large, quantities. He had a patient, aged 72, who had a squamous epithelioma of the right tonsil, invading the soft palate and side of pharynx. There was also slight glycosuria. The patient was seen by three eminent surgeons, all of whom considered the condition inoperable. As a last resort Dr. Finzi applied radium on two occasions, for seven and a half hours each sitting, and three weeks after the growth had practically disappeared, leaving a large excavation in its place. The base of the ulcer was soft, except at two points on the edge of the ulcer, which probably another application of radium would cause to disappear.

Dr. SCANES SPICER said it was marvellous what a sufficiently large quantity of radium would effect in such cases as this. A case of his own at present was being treated with Dr. Finzi's 205 mgm. of radium, and was doing very well; four-fifths of the mass had vanished within three weeks of the first application for thirty hours in all, and the ulceration left was difficult to make out. Perhaps the free discharge of mucus from the pharynx for twenty hours accounted for an immediate reduction of swelling, cleansing, and pallor. The point of greatest and remaining swelling and ulceration in Mr. French's case was at the junction of the soft and the hard palate, the point at which the chief stress of muscular action of the soft palate was resisted at its attachment to the bone.

Mr. DEMPSEY (Dublin) said he recently had a case of what was considered to be inoperable cancer of the larynx. The patient subsequently came to London, and was treated by Dr. Hill, and the difference in the appearance on his return to Dublin was very marked, though it was only a fortnight later. The tumour was much diminished in size, and the general condition was better. The patient subsequently succumbed. It seemed to be moderately easy to get improvement with radium, but very difficult to obtain cure. He asked whether the treatment in the present case had ceased, as he did not regard the case as cured.

Mr. DE SANTI said he had at present under his care two cases of malignant disease of the tonsil and the adjacent parts, with metastatic growths in the neck, which he proposed to exhibit before the Section, then to treat with radium, and later show the patients again. At present they might be considered as inoperable. His experience had been the same as Mr. Dempsey's—namely, that radium caused sometimes a great change, but not a cure.

Mr. GAY FRENCH, in reply, said the total number of applications to the palate in the first instance amounted to eighteen hours, and there was much diminution afterwards, though not complete disappearance of ulceration. Therefore further treatment was given. Dr. Knox gave an X-ray application, to try to close up the ulceration, and there was still a very shallow ulcer. He would be glad to show the case again in six months' time.

Portion of Rabbit-bone removed from Right Bronchus, wherein it had been Impacted for more than Three Years, and had caused Symptoms of Bronchiectasis.

By HERBERT TILLEY, F.R.C.S.

W. G., MALE, aged 46, was admitted to University College Hospital on January 9, 1911, complaining of "feeling ill and coughing up blood."

History: Three years ago patient "swallowed a rabbit-bone, which seemed to stop at the bottom of the throat and to cause a tickling feeling in the back between the shoulder-blades." The patient then began to suffer from a cough which had lasted ever since. Fourteen days after swallowing the bone he was removed to an infirmary, where he "was treated for pain in the back, chest, and stomach." Eighteen months ago he coughed up some streaks of blood for the first time. There was shortness of breath on exertion; he complained of a dry throat, and he has wasted very considerably during the past six months.

Present state (January 9, 1910): The patient is a delicate-looking man. The breathing is rapid and a troublesome cough is accompanied by offensive expectoration tinged with blood. There is marked clubbing of fingers and toes. Pulse, 80; temperature, 100·5° F.; respiration, 36. The chief physical signs noted in the chest are: Left lung—Some diminution of respiratory movement at the base where the breath sounds are also weak. Percussion dullness, diminished breath sounds and vocal resonance over lower portion of scapula. Percussion note impaired over front of apex to level of third rib. Right lung—Vocal resonance and expiration increased over apex in front; breath sounds absent over base of lung posteriorly; crepitations audible in axillary region with some increase of vocal resonance; breath sounds diminished over base anteriorly. No tubercle bacilli found in expectoration, but Gram-positive staphylococci and streptococci. Skiagraphy did not reveal the presence of any foreign body in either lung. Urine acid: specific gravity, 1020. No albumin or casts.

Mr. Godlee deemed an operation on the bronchiectatic cavities inadvisable because of the presence of physical signs in both lungs. He kindly asked me to examine the lower air-passages by the direct method.

February 15: Morphia, $\frac{1}{6}$ gr., and atropine, $\frac{1}{200}$ gr., were injected hypodermically half an hour previous to administration of chloroform. The left lateral position was employed. On reaching the bifurcation of the trachea the contrast between the pale mucous membrane of the left bronchus and the red congested lining of the right was very noticeable. Cocaine (10 per cent. solution) and adrenalin were then applied to the right bronchus and the extension tube passed into it, when the foreign body was sighted about 2 in. from the bifurcation. The swelling and profuse secretion from the mucosa made the seizure of the foreign body difficult, but when this was overcome and traction made upon it a profuse discharge of foul, amber-coloured liquid poured from the proximal end of the bronchoscope. During the simultaneous withdrawal of bone, forceps, and tube through the larynx the bone slipped from the grasp of the forceps, and a further search discovered it in the left bronchus, from which it was successfully removed.

The case is of interest because of the length of time the foreign body had been impacted, its non-detection by the X-rays, the presence of definite physical signs in each lung, the marked contrast between the right and left bronchus when viewed through the bronchoscope, and the successful result which may be attained by the use of the direct method in otherwise obscure cases of lung affection.

DISCUSSION.

Dr. H. J. DAVIS said Mr. Tilley had to be congratulated on the success in the case. In the *Proceedings*[1] would be found the notes of the case of a man, aged 53, in whose right lung a piece of beef-bone had remained five months before it was coughed up by the patient. He was under Dr. Seymour Taylor's care with pulmonary abscess and empyema, which was drained. Recovery was complete.

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[1] *Proceedings*, 1910, iii, p. 75.

Dr. PATERSON asked for information as to the progress of the case; what was the subsequent condition of the bronchus? Members knew of a case operated upon with success by inserting a tube into the bronchus, so as to prevent its contraction.

The PRESIDENT also congratulated Mr. Tilley on the result. Bronchoscopy had completely transformed the prognosis of bronchiectasis from the presence of a foreign body. He asked whether Mr. Tilley encountered special difficulties in the way of extraction due to the length of time the body had been there, or was there cicatricial thickening round the body? Also, did he think it would remain patent?

Mr. HERBERT TILLEY, in reply, said the foreign body was only removed a fortnight ago, so it was early to speak of ultimate results in the lung. The patient remained in hospital for a week, and at the end of that time the amount of discharge was so much diminished that the Sister of the ward said it was "nothing to be compared with what it had been." But one must assume that the bronchiectatic tubes had lost some of their elastic tissue. Three days after the operation the patient was eating well, and felt better than he had done for years. He was now at a convalescent home, and if it was desired, Mr. Tilley could show him on his return. There were considerable difficulties in the operation; the amount of secretion was very large, and directly the right bronchus was touched it began to exude blood, and one had to apply mops moistened with cocaine and adrenalin. Morphia and atropine were injected half an hour before the chloroform was given in order to lessen exudation. When he got hold of the foreign body he levered the forceps as far as possible, and then drew the bone through a bunch of granulation tissue; some oozing of blood took place immediately, followed by a gush of foul-smelling fluid. The foreign body slipped from the grasp of the forceps as it was passing through the glottis and fell into the left bronchus, from which it was easily secured, and removed simultaneously with the bronchoscope.

A Tooth removed from the Floor of the Right Antrum in Patient whose corresponding Nasal Cavity was obstructed by a Nasal Polypus.

By HERBERT TILLEY, F.R.C.S.

MISS K., aged 16, complained of constant colds during the past four months and complete nasal obstruction on the right side. Examination revealed the presence of a large polypus with all the characteristics of a naso-antral polypus, and hence it was decided to operate through the canine fossa. The antrum was found to be almost filled by a thick translucent polypoid mucous membrane, on the lower surface of which the tooth (exhibited) was lying free. The nasal polypus seemed to take origin from the upper, inner surface of the posterior antral wall, and it was freely removed from this area. The inner wall of the sinus was also taken away, and the bucco-antral wound sutured. Recovery was rapid and uneventful.

Tumour of Right Side of Neck involving the Pharynx, in a Woman aged 30.

By W. H. Kelson, M.D.

Mrs. B., married four years; no children nor miscarriages. Swelling began seven years ago as a small lump at the back of the neck, which, however, was always fixed. About five years ago an incision was made into it by a surgeon, but nothing is said to have come out, and it healed up in a few days. Patient was first seen at hospital in November, 1910, when she complained of very severe pain in the right side of her head, which prevented her getting sleep. She was then found to have a mass on the right side of the neck, limited *behind* by the middle line, *in front* by the sternomastoid muscles, above by the superior curved line of the occiput, below by a horizontal line on a level with the cricoid cartilage. Tumour was firm, fixed to deeper parts, but not adherent to the skin. On examination of the pharynx, a rounded mass was to be seen behind the posterior pillars of the fauces on the right side, extending up and down as far as the finger could reach, but not adherent to the mucous membrane.

Although the case was an unfavourable one for operation, patient and her husband were exceedingly anxious to have something done. An incision was therefore made, and a mass of the growth, which appeared to take origin just in front of the transverse processes of the cervical vertebræ, removed. The wound healed well, and the pain, which previously was of a severe bursting character, disappeared after the operation.

Microscopic slides were shown.

DISCUSSION.

Mr. DE SANTI judged, from the specimen under the microscope, that the disease was probably sarcoma, and he thought it was inoperable.

Dr. PATERSON said he saw a similar case some time ago which had a like history. Four years before a surgical colleague had removed it by an extensive operation, but there was recurrence around the tonsillar region. The question was whether anything further should be done, but Mr. Butlin, who saw the case, strongly discouraged operation. He regarded it as lymphadenoma.

Mr. ROBINSON said that, judging from the first of the sections exhibited, he regarded the tumour as a myxo-chondroma, or myxo-chondrosarcoma, and that it had recently taken on more rapid growth, as shown by the more embryonic tissue.

Dr. KELSON, in reply, said he thought the case proved that a partial operation in some instances of slowly growing sarcomata was indicated. The case went to a large general hospital, and it was sent back as inoperable. That might be so in regard to complete removal, but the woman had been relieved of her agonizing pain, and the growth was now smaller than formerly. He had seen some of these patients going on for years, one for ten years, under those conditions, and he thought the fact of not being able to get away the whole of the growth need not necessarily make it inoperable.

Ulceration of the Pharynx, due to Pyorrhœa.

By L. HEMINGTON PEGLER, M.D.

C. H., AGED 64, labourer, has complained of pain at back of tongue and right side of jaw for nearly a year. There is considerable difficulty in opening the mouth widely. No loss of weight—no history of syphilis. On first examination at hospital, there was much pyorrhœa and ulceration of the gums around the last two molar teeth of the right side. The ulceration is superficial and extends on to the anterior portion of the fauces; the edge is not very well defined. There has been no enlargement of lymphatic glands. Several teeth have been extracted, since when the conditions show some improvement.

Dr. PEGLER added that there seemed to be a belief on the part of some of the members that this case was malignant. He intended having a scraping examined for Altmann's granules as a test, and later would report the result.

Case of Party-wall Cancer of Larynx and Œsophagus.

By L. Hemington Pegler, M.D.

A. M., female, aged 45, married, has complained of sore throat since Christmas, 1910, difficulty in swallowing, and cough. On examination, there is a large mass of growth obstructing the lumen of the lower part of the pharynx, and evidently springing from the posterior wall of the cricoid. There is some ulceration to be seen on the right side. The lymphatic glands of both sides of the neck are considerably enlarged. Opinions are solicited as to the suitability of the case for radium treatment.

DISCUSSION.

Mr. de Santi asked if it was considered that the case was operable. He had had a case in a young woman aged 29, with similar malignant disease of the party wall, where the whole of the larynx was removed, as well as the œsophagus from its commencement to the level of the top of the sternum, the thyroid gland, and a mass of enlarged cervical glands : gastrostomy was done at the same time. She recovered from the operation, but only lived for five or six months longer. The nature of the operation had been previously explained to the patient, but she chose to have it done. In another case a young woman had cancer of the party wall, and glands in the neck. A colleague of his did a similar operation, namely, removal of half the larynx, five rings of the trachea, part of the œsophagus, and part of the pharynx, and did gastrostomy ; since then the patient had actually got married.[1] The operation was done one and a half years ago.

The President said it would be interesting to know whether radium treatment would do any good in the cases. He asked for illustrations of the condition now to compare with that later on.

Dr. Scanes Spicer considered the case unsuitable for radium, because the growth so extensively involved the posterior arch of the cricoid. He did not think the radium tubes could be tolerated there long enough.

Dr. Pegler thanked Mr. de Santi for the narration of his cases. He had recently had in private a case very similar to this one, that had been operated upon for him by Mr. Wilfred Trotter. The diseased portion of the wall of the œsophagus was successfully removed, and the patient made an excellent recovery so as to be able to return to her home on the Kentish coast, but she was not a very good subject and in about a month's time came back to London, where she succumbed to heart failure.

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[1] *Proceedings*, 1910, iii (Clin. Sec.), pp. 44-47 ; 1911, iv (Clin. Sec.), p. 112.

Antrum Suppuration: Nasal Antrostomy followed by Blocking of the Nasal Duct.

By Dan McKenzie, M.D.

The patient had been suffering from symptoms of left antrum suppuration for two or three years, with pain under the left eye and cough and bronchitis. Nasal antrostomy was performed on December 5, 1910, the anterior end of the inferior turbinal being removed, the nasal wall of the antrum broken down and the cavity curetted. On December 25 the left eye became red and inflamed, and since then the patient has been troubled with epiphora and the occasional discharge of pus from the punctum lachrymale. In the meantime the nasal condition has very much improved, there is no pain, little or no purulent discharge from the nose, and no chest symptoms.

The probability is that the involvement of the nasal duct has been due to a wound of its lower end inflicted at the time of operation and not to a spreading infection from the nose up a healthy nasal duct. Wounding of the nasal duct in operations on the nose would seem to be uncommon, but infection of the nasal duct in nasal sinus suppuration is still more uncommon, doubtless because of the protected position of the orifice coupled with the fact that the direction of its currents is from above downwards.

DISCUSSION.

Dr. H. J. Davis said there was no doubt, from the patient's statement, that the lachrymal sac was affected. It should be excised, and he did not think the patient would get well until that was done.

Dr. Dundas Grant thought something less radical than excision of the lachrymal sac should be tried. The canaliculus might be slit up, and a style passed down. A sufficient opening for the nasal duct could be chiselled inside the nose to take its place. He had mentioned on one occasion that he injured a nasal duct when using a rectangular knife (Macdonald's), one of a set which cut in three directions. There was some temporary epiphora, but recovery soon followed. He saw Professor Passow do the chiselling operation in Berlin, and the result was very good.

Mr. Clayton Fox did not think excision of the lachrymal sac was wanted. He thought a style longer than the usual one should be passed down the nasal duct, and probably patency would be secured after wearing it for a few weeks.

Dr. PATERSON said that such cases had been reported, and had been attributed to the use of instruments which acted by cutting towards the operator. He had seen a number of cases in the last two or three years in which there was a tendency to contraction of the inner wall opening. Most of those had been done by a burr being pushed in, and the opening had contracted up afterwards. He had seen one or two which had been done by members of the Section, and in which the opening had to be enlarged afterwards. In some there was not enough of the lower turbinate taken away, so that it flapped on to the opening.

Dr. JOBSON HORNE believed that the injury to the nasal duct, although not common, was of more frequent occurrence than was generally known. Such cases usually got well without further surgical treatment. The patient had expressed herself as satisfied with her progress; therefore, he would leave it alone for the present.

Dr. BRONNER thought that the mucocele might be due to inflammation in the nose, which had spread up the lachrymal duct, and that there need not necessarily have been any lesion of the duct. Many cases of mucocele and conjunctivitis were due to intranasal disease. He suggested that the ordinary method of treating inflammation of the lachrymal sac should be tried—namely, passing a probe and syringing out the sac.

Dr. LAMBERT LACK thought such cases were fairly common, but the lachrymal obstruction was nearly always temporary, passing away in a month or two. He thought the present case would get well if left alone.

The PRESIDENT thought Dr. Bronner's suggestion a good one. His experience was that the epiphora nearly always passed off. In many cases the nasal duct was wounded at the lower end, and it was a matter of chance whether one particular operation or another caused the obstruction. It would be worth while to wait longer, and then slit the canaliculus, and, if cure did not result, remove the lachrymal sac.

Dr. MCKENZIE, in reply, said the patient was already under treatment by an ophthalmic surgeon, who was catheterizing the duct regularly. He had not so far proposed surgical treatment of the canaliculus or the lachrymal sac. During operations on the nose the duct was doubtless frequently injured, and yet interference with the functions of the lachrymal apparatus did not occur. The reason for this, he thought, was that in nasal operations the nasal duct if opened freely would not undergo stenosis; whereas if it were injured without being freely opened stenosis would probably result. In the present case he thought there had not been enough inferior turbinal removed, and that it had undergone contraction, and blocked the orifice of the duct beneath. He would await events, and if the case did not clear up he would remove the remainder of the inferior turbinal, or adopt the measure suggested by Dr. Grant.

Cicatricial Obliteration of the Nasopharynx (Hereditary Syphilis).

By Dan McKenzie, M.D.

The patient is a male, aged 27. When first seen a year ago, he said he had never been able to breathe through his nose. On examination, the soft palate was seen to be caught up and back, and to be adherent to the upper part of the posterior wall of the nasopharynx. Perhaps it would be more correct to say that there was no soft palate at all, and that the nasopharynx was blocked by a mass of scar-tissue. Through this scar-tissue a small foramen led into the nose.

The case did not look like one that would benefit from a cutting operation—either the raw edges of the wound would reunite, or, remaining open, would, in the absence of the movable velum palati, end in rhinolalia aperta and kindred discomforts. Consequently an attempt was made to dilate the small opening with a laminaria tent. The result was rather embarrassing. After the tent had been left in situ for a few hours attempts were made to remove it, but unknown to us it broke off short, and a portion remained inside. During the few weeks following ulceration set in around the opening and spread until checked by large doses of potassium iodide. The patient was now able to breathe through his nose. Five months later complaints of purulent discharge from the nose led to the discovery of a hard loose substance in the back of the left nasal cavity. Under an anæsthetic this was removed and found to be the remains of the tent.

The ultimate result of the patient's experiences is that he can blow his nose satisfactorily.

(?) Lupus Erythematosus of the Mucous Membrane of the Cheeks of over Twelve Months' Standing.

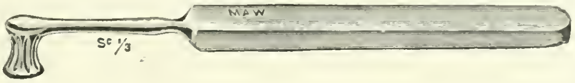
By H. Lambert Lack, F.R.C.S.

The patient, a woman, aged about 40, has two irregular patches on the insides of both cheeks opposite the molar teeth. In places there are deep cracks. The epithelium is thickened in parts, and in other parts apparently almost absent. The condition resembles lupus erythematosus of the skin. In addition there are one or two small circular white patches covered with membrane on the lower alveolus. The patient has been under various doctors for the past year. All her teeth have been removed under the impression that the condition was due to sepsis, and when this failed the patches have been cauterized frequently for nearly twelve months. The condition gives rise to no pain, and makes slow progress; it is slightly, if at all, worse than it was a year ago. The patient was not aware of her condition until it was discovered by her dentist. The exhibitor considers the case one of lupus erythematosus, and considers local treatment useless.

A new form of Burr for use in the Antral Operation by the Nasal Route.

By Somerville Hastings, M.S.

In the removal of the inner wall of the antrum, whether the operation be performed entirely through the nose or assisted by an opening into the mouth, the principal difficulty is met with below, where the lower part of the antro-nasal septum is formed by a firm ridge of bone. For the removal of the bone a burr of some kind is most convenient. But the ordinary round-headed burr, on account of its shape, is with difficulty kept in contact with the ridge during its removal. The instrument here shown, which has a concave cutting surface, has been found much easier to manipulate and more certain in its action. The head of the burr is shaped like a small bobbin, the rounded surface being roughened and the ends smooth. To one end is attached at right angles a long handle. The instrument is introduced and manipulated through the nose, and is also of use in smoothing off the rough edges of the antro-nasal opening all round, when as much bone as is necessary has been removed by forceps.



A new form of Burr for use in the Radical Antral Operation by the Intranasal Route.

By W. H. Kelson, M.D.

By means of this instrument, the cutting blade of which is flattened from above downwards, the dense bony inner wall of antrum can be cut away till the floors of the nose and antrum are flush.

Ventricular Voice in a Woman aged 28.

By F. F. Muecke, F.R.C.S.

Became hoarse when aged 3, after scarlet fever. Since about 15 years old the voice seems to have got gradually stronger. Otherwise in perfect health. The right vocal cord is seen somewhat atrophied and perfectly flaccid. The left vocal cord has disappeared, old scar tissue being seen in its place. The ventricular bands are rounded and very large, and on phonation come forcibly together, and can be seen vibrating freely. The arytænoids turn inwards, assisting the ventricular bands to close the glottis, their inner surfaces touching firmly during phonation. The voice is firm, but husky and throaty and has a somewhat falsetto quality.

Laryngological Section.

April 7, 1911.

Dr. P. Watson-Williams, President of the Section, in the Chair.

Specimen of a Pedunculated Growth on an Elongated Uvula.

By Herbert Tilley, F.R.C.S.

The specimen was removed from a male, aged 30, who said that the growth on his uvula had been present as long as he could remember, but had given rise to no irritation or coughing until the last three months. The uvula measured nearly 1 in. in length, and from its lower end hung a pedunculated growth. The stalk measured $\frac{3}{4}$ in. and the small tumour is about the size of a dried pea. In order not to spoil the specimen no section of the growth has been made, but it is probably a small papilloma.

DISCUSSION.

Dr. Jobson Horne said he had met with a similar case, which he showed before the Laryngological Society some years ago. It was similar in appearance, and there was the same absence of symptoms. That was the usual history of those cases, because if there was sufficient length of stalk it adhered to the adjacent parts, and so was not felt. He regarded the growth as a papilloma and not as a fibroma. In his own case the section went through the stalk and the entire growth. He would be pleased to show it again if desired.

Dr. Paterson said he had seen a similar case, where the uvula was longer than in this case, as it had to be hooked up with a probe to see the tip, which had a papilloma on it. He did not know whether the patient, in Mr. Tilley's case, complained of symptoms actually caused by the little growth at end of a long uvula. In his (the speaker's) case it was observed accidentally, and it produced no symptoms whatever, proving that a long uvula need not produce discomfort, and should not be sacrificed merely on account of its length.

The PRESIDENT (Dr. Watson-Williams) said that whereas an elongated uvula was at one time removed fairly frequently, it was now relatively rarely done. But it was possible to jump to an opposite conclusion, and say an elongated uvula never caused symptoms. This, however, was not true, for his own had caused trouble at one time years ago, but after it was shortened the trouble it gave rise to disappeared.

An Improved Form of Hay's Pharyngoscope.

By HERBERT TILLEY, F.R.C.S.

THIS improved pharyngoscope gives a larger image, and the lamps are prevented from touching the pharyngeal wall and so causing discomfort. If cocaine is previously applied to the posterior pharyngeal wall the instrument causes no retching, and it is useful for demonstrating lesions to novices who cannot use the posterior rhinoscopic or laryngeal mirrors.

DISCUSSION.

Mr. TILLEY, in answer to the President, said he had found it useful in post-nasal cases. In the course of hospital work he preferred the ordinary mirrors for making rapid observations.

Dr. JOBSON HORNE said there was a tendency to increase the number of such instruments, so that students were apt to omit to learn the art of laryngoscopy. He hoped that the result would not be the same to the art of laryngoscopy as had happened to the art of miniature painting by the development of photography.

Case of Chronic Laryngitis.

By WALTER HOWARTH, F.R.C.S.

PATIENT, aged 33, came to the hospital two years ago complaining of profuse nasal discharge with fœtid crusts. There was active necrosis of the nasal septum, which gradually cleared up under iodide of potassium. A year later the patient became husky, and the cords were seen to be red and thickened and did not approximate properly. The interarytænoid space was clear. The voice gradually improved, but some heaping up in the interarytænoid space became noticeable. This has steadily progressed and now has a mamillated appearance. The condition of the vocal cords and of the voice is much improved. The cleft in the left posterior faucial pillar was present when the patient was first seen. Treatment with alkaline lotion and potassium iodide has been practically continuous for the last two years.

DISCUSSION.

Dr. DUNDAS GRANT said it was a form of laryngitis which resulted from nasal suppuration, and such cases were not always easy to deal with. They might be spoken of as post-rhinitic. Although in this case there had probably been syphilitic disease of the vomer, the condition of the larynx resulted rather from the accompanying suppuration than from any specific element in the case. It was what might be called a kind of pachydermia of the interarytænoid mucous membrane, and it would be well to remove it. Such swellings did not always interfere with the voice, because sometimes they were at a higher level than the vocal cords.

Mr. HOWARTH, in reply, said that he regarded the condition as a result of chronic irritation due to the prolonged nasal suppuration. It was remarkable that the condition of the vocal cords seemed to have improved considerably, and yet the interarytænoid fold had got worse. At present the voice was not bad. He did not propose to do anything operative unless further signs developed.

Tracheo-Laryngostomy for Stenosis following Tracheo-Laryngotomy.

By WILLIAM HILL, M.D.

GIRL, aged 8. A so-called high tracheotomy was performed five years ago for diphtheria, and the child has been obliged to wear either an intubation tube or a tracheotomy tube ever since, except for an occasional few days when an attempt has been made to get on without an artificial apparatus. Two years ago the stenosed larynx was dilated with bougies after internal laryngotomy and a larger intubation tube worn for three months, but on removing it the stenosis quickly recurred and it had to be re-inserted. Tracheo-laryngostomy was performed by the exhibitor six weeks ago. The use of rubber wings for keeping the drainage tube in position and the fissure open was the invention of Mr. Charles Hope.

DISCUSSION.

MR. HERBERT TILLEY said that two years ago he had treated a child of about the same age which had had a high laryngotomy, or thyrotomy, done, in fact the tracheotomy tube had been put through the side of the thyroid cartilage and left there. The usual results followed, viz., granulations arose around it, and ultimate cicatrization occurred in the cricoid region, so that at the operation it was only possible to pass a probe upwards through the larynx from the low tracheotomy wound. He tried the method of tracheostomy and laryngostomy, and opened up the larynx and trachea. The lumen of the cricoid region was obliterated, and its place taken by thick scar tissue. It was difficult to cut out the scar tissue, but some of it was removed, and he then placed a rubber tube through the larynx and upper part of trachea. The patient was in the hospital for months, and plastic operations were tried to cover the laryngo-tracheal wound. But the cricoid region became narrower and narrower, and then became as stenosed as before. Therefore a rubber tube was kept in, being removed and replaced every fourth day. After eight months the patient was sent out with a dilatation through the original cricoid stricture. The trachea was closed in front and the child could breathe through the larynx, and through the original constricted region. But there was anxiety because stridor occurred on great exertion, and there was likelihood of trouble if the child got any laryngeal catarrh. Afterwards it was stated the child was doing fairly well. There was a great deal of trouble connected with the case, and such cases were desperate from the start. The value of intubation depended to some extent on what was the condition in the upper tracheal or cricoid region. If there were any granulations there, intubation was excellent treatment because the intubation tube would press away the granulations, and leave a fairly patent airway. But if the obstruction was fibrous, he thought there was not much benefit from intubation. He had a case at present in which ten days ago an intubation tube was put in, following high tracheotomy for diphtheria, and the child was doing well; the tube had been left out for two days and breathing was good without it. (Since the last paragraph was written, the child has died of bronchopneumonia, and the autopsy showed a fair airway through the cricoid region.)

Mr. DE SANTI said the majority of the cases of the kind he had had were in adults, and in those cases, he agreed with Mr. Tilley, intubation was of no use. He remembered one man particularly, who had extreme stenosis, and only a small tube could be got in, but it was dilated up to the largest sized Schroetter tube, and there was good breathing for the time. But the patient went back again rapidly and re-contraction took place. He had to wear a tracheotomy tube for the rest of his life. One day he got mixed up in a drunken brawl, his tube came out, and he was brought into Westminster Hospital dead. He had operated on the thyroid by splitting it. He found that even when keeping the alæ aside, though re-adhesion might be prevented, re-contraction nearly always took place. He feared that in the present patient there would be re-contraction, especially as there was a tendency for keloid to form.

Dr. FITZGERALD POWELL said that two or three cases in his practice occurred to him—in which contraction had taken place after the removal of a large portion of the larynx—and in which he had got very good results by the use of silver and ebony plugs, slipped in above the tracheotomy tube, passing through the cavity of the larynx upwards into the pharynx. One was a case of fibrosarcoma of the larynx, and the other syphilitic stenosis of the larynx. In both these cases about half the larynx had been removed. The plugs were kept in for six to twelve months, being taken out occasionally for a few hours, cleaned, and re-inserted. When it was decided to remove them the edges of the tracheotomy wound were freshened and brought together, and breathing took place through the larynx. In neither case was there any re-contraction, there was good breathing space, and in the specific case a good strong voice resulted; in the sarcoma case the voice was not so good and was more whispering. The great point about the treatment was that the plugs must be kept in for at least six months.

Dr. DUNDAS GRANT said his experience of tracheo-laryngostomy had not been very encouraging, but in one instance it was to be explained by a point which was worth remembering. A very bright little boy had a tracheotomy done on account of laryngeal diphtheria. It was done too high, and was followed by extreme narrowing. It was not merely stenosis but absolute atresia. Dr. Grant performed tracheo-laryngostomy, and the boy went on very well for some time. Unfortunately, at that time, there was a reconstruction of the wards, and it was impossible to get a reasonable temperature in them. The boy developed bronchitis and died. In a work on the subject great stress was laid on the necessity of keeping the ward at a high temperature. Recently he had another case, a child, with absolute atresia. He removed the high tracheotomy tube and introduced it low down. The child had to go home to the country, but when it returned and was ripe for operation there was, unfortunately, an outbreak of measles. The child was sent away without operation, but developed measles and bronchopneumonia, and died. Dilatation with intubation tubes was of great value if done early. He had a boy on whom high tracheotomy had been done in the country for urgent stenosis, and it was impossible to remove the tracheotomy tube. He did intubation and tried to feed him, but bronchitis set in, and the tube had to be taken out again, and a tracheotomy cannula again inserted. At the end of ten days the bronchitis had disappeared, and again intubation was done, the child being regularly fed by a tube through the nose. Intubation tubes, several sizes larger than the one appropriate to the patient's age, were forcibly introduced so as to exercise overdilatation. The patient did very well, and was now strong and hearty, ten years having expired since the treatment took place. As pointed out in

Dr. Ball's book, intubation with tubes too large for the patient's age was very valuable to obtain sufficient dilatation to obviate the need of a tracheotomy tube. He would look forward with interest to the sequel in Dr. Hill's case.

Dr. StClair Thomson said such cases were so rare that all who saw them should record their experiences. Now that tracheotomies were done lower down, they were not often seen. At the Belfast meeting[1] it was a revelation to hear what was done by intubation in America. It was of no use putting in an intubation tube for a week or two, or putting it in only at night. Cases were reported in which such tubes had been worn continuously night and day for six months, twelve months, and two years. Though there was not much credit to be made out of such cases, it was a great thing to restore a person so that he could gain his livelihood. Septic bronchitis was lurking at the back of all these cases, and sometimes the skilled physician did not diagnose it. An unfortunate little baby fell into the hands of a surgeon; as there was stridor he examined with direct laryngoscopy, and thought he saw something below the vocal cords, but a laryngo-fissure revealed nothing. The surgeon left the tracheotomy tube in, and the child got worse. Then he proposed to the relatives that he should do laryngo-tracheostomy, and it was then that the child was taken to him (Dr. Thomson.) There was evidently not enough air reaching the lungs, but he saw very little locally the matter; there seemed to be some thickening below the cords left by the laryngo-fissure. A physician reported that nothing could be made out amiss with the lung. The child died suddenly in the night, and post mortem he found there was a horrible tracheotomy tube, with such a curve in it that it had irritated the trachea below the tracheotomy wound. The lungs were simply riddled with septic bronchitis, which had not been suspected. There was no temperature or cough or abnormal sound. There was evidence that laryngo-tracheostomy was, even on the Continent, falling into disuse. Of thirty-seven cases, six were dead —a mortality of 16·3 per cent.—and only fourteen were benefited. An operation which gave only such results did not seem worth developing.

The President said he feared that Dr. Hill had not heard much encouragement. But the case seemed to give hope of further improvement. He suggested that Dr. Hill should see if the child could breathe with the external opening covered up. If the dilated process was not reduced sufficiently to last some days when the external wound was covered up, he did not suppose that any plastic operation would be likely to be successful.

Dr. Hill, in reply, said the ostium would be temporarily covered over before doing a plastic operation. The large intubation tube had been taken out after that method had been given a thorough trial. It was only in desperation that he had resorted to tracheo-laryngostomy, but he did not then realize that this operation had such a black record. He would show the case again.

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[1] *Brit. Med. Journ.*, 1909, ii, p. 1144.

Carcinoma of Œsophagus.

By WILLIAM HILL, M.D.

MALE, aged 45, was shown December, 1910, as (?) temporary local cure after six prolonged applications of radium salt to the cervical and upper thoracic œsophagus. He has had no application for four months, and there is now well-marked malignant ulceration lower down the gullet, 26 cm. from the incisor teeth; the lowest previous radium application made was 22 cm. from the incisor teeth. Further radium treatment will be carried out.

Permanent Styletted Oro-œsophago-gastric Tube which had been in situ for Thirteen Weeks.

By WILLIAM HILL, M.D.

THE tube had been inserted for the relief of marked dysphagia in a case of extensive pharyngo-œsophageal carcinoma. The patient had been enabled to swallow well-masticated solid food *by the side of the tube* after the first few days. The rubber, though sodden and inelastic, was not "perished" in spite of its long retention.

Two Cases of Malignant Disease of Tonsil, Pharynx, and Tongue.

By P. R. W. DE SANTI, F.R.C.S.

Case I.—Male, aged 51. History of sore throat, pain in the left ear, and in swallowing, also loss of flesh for two to three months. Speech thick and impeded; noticed about the same time a swelling left side of the neck. Examination reveals inability to protrude the tongue equally; the left half being fixed more or less at its base. There is a hard, raised everted mass over the left tonsil which affects the palato-glossal fold and extends to and into the tongue at its base on the left side; the growth can also be felt to involve the lateral pharyngeal wall. There is a mass of hard, fixed glands in the submaxillary and upper cervical glands on left side of neck. The case seems to the exhibitor to hold out but little hope of cure from operation.

Case II.—Male, aged 49. History of sore throat and swelling on right side of neck for four months, occupying the right anterior faucial pillar, right tonsillar region, and extending to and involving the base of the tongue. On the right side is a hard, malignant growth, ulcerated in parts. The disease extends to part of hard and soft palate on right side and passes down the pharynx laterally. The tongue is not protruded equally. There is a hard mass of infected glands in the neck. The case is of exactly the same nature as the other one shown.

The two patients are brought forward for opinions on the radium treatment, and how, if recommended, to get the treatment applied. The exhibitor's opinion is that radium will possibly improve, but not cure, the disease.

DISCUSSION.

Mr. DE SANTI said that neither of his cases were either well or happy; they both suffered much pain, the tongue was involved, there was pain on swallowing, and they were going down hill rapidly. He, like many others, wanted radium for the purposes of treatment, but could get none; there was none at the Westminster Hospital. He did not think radium would cure the condition, but it might greatly alleviate it. His object in bringing the cases forward was to ascertain if any member could tell him of anyone who had access to radium and would be willing to apply it to these two patients. If they could get radium treatment he would subsequently bring them before the Society.

The PRESIDENT said many members of the Section had felt keenly the grave difficulty there was in getting radium for the treatment of these cases and more particularly for poorer patients of the hospital class. It was a very grave want. He thanked Dr. Hill and Dr. Finzi for helping his own patient from the Bristol Royal Infirmary, who had assuredly received benefit. He hoped to show him at the Bristol meeting. It was particularly unfortunate if a poor patient came to London from a provincial town and the radium could not be applied, but perhaps more striking was the hardship for poor patients in London itself.

Functional Aphonia in a Boy aged 15.

By L. Hemington Pegler, M.D.

Aphonia, three months' duration; no dyspnœa, cough or stridor; works in an atmosphere contaminated with irritating fumes. On examination the cords fail to approximate on attempts at phonation. There is, however, an absence of the stammering of the cords, familiar in female cases; they are deeply injected, otherwise appear healthy.

The case is shown owing to the infrequency of functional aphonia in the male. It resisted treatment by application of the interrupted current to the cords, but during the last week the voice has returned under the application of liq. ferri perchlor.

DISCUSSION.

Dr. **Pegler** added that the boy had been completely aphonic, but Mr. Bain, when seeing the case after the notes had been sent to press, brought the voice back by the application of a cotton-wool holder charged with solution of perchloride of iron. There was no suggestion of hysteria in the patient. The injection of the cords was believed to be due to the boy's employment in a bird-cage factory, where he inhaled noxious fumes given off at his work.

Dr. **Jobson Horne** said he did not think the term "functional" should be used in connection with the case; though functional aphonia did occur in males. In this case one should take care to exclude the possibility of pulmonary tuberculosis.

Dr. **FitzGerald Powell** said that it must be in the experience of most of the members that laryngitis was not an uncommon cause of "Functional Aphonia," quite apart from hysterical or neurotic functional aphonia.

Dr. **StClair Thomson** said that in the *Proceedings*[1] of the old Society there was an account of the case of a soldier who had gone through the Boer War. Towards the end of the War he was thrown off his horse, and that extinguished his voice. Yet he was a robust man and courageous.

Dr. **Dundas Grant** said the association of laryngitis with functional aphonia was not very uncommon. Sometimes the occurrence of the laryngitis in a nervous person occasioned such alarm as to cause the voice to disappear; a self-suggestive or, in other words, hysterical event. Sir Morell Mackenzie recognized the association, and said that before faradizing a case of supposed functional aphonia, one should make sure there was no congestion of vocal cords or laryngitis, simple or tuberculous. From catarrhal causes there might be impairment of the action of the internal tensors, and the treatment of the laryngitis was often the most important element in the case. Recently Dr. Grant had a case of marked aphonia, with return of voice after the use of the laryngoscopic mirror. There was, however, tuberculosis in the lungs, and congestion of the vocal cords, undoubtedly an early stage of infiltration with tubercle.

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[1] *Proc. Laryng. Soc. Lond.*, 1906, xiii, p. 112.

Dr. PEGLER replied that Dr. Horne might take it as assured that this was a case of functional paresis of the adductors. When the voice returned (which it did suddenly) there was no remaining hoarseness, as might have been expected had there been intercurrent laryngitis, although from other causes the cords were congested. There was no suspicion of tuberculosis.

Laryngeal Tuberculosis in a Boy aged 10.

By W. JOBSON HORNE, M.D.

THERE had been loss of voice, coming on gradually for over two years. Latterly there had been dysphagia, due to a considerable thickening and ulceration of the epiglottis. To relieve this difficulty the exhibitor intended to operate upon the epiglottis, and showed the case prior to the operative treatment.

DISCUSSION.

The PRESIDENT said that it was a question whether it was tubercle, and it would be well to see the case again later on.

Dr. DUNDAS GRANT said the one point against it being tubercle was its unilaterality. Otherwise the ulcer looked exactly like tubercle.

Perforation of the Soft Palate due to Syphilis, but presenting Unusual Features.

By W. JOBSON HORNE, M.D.

THIS case was exhibited, not on account of any rarity of the lesion, but in view of the discussions that had arisen when cases presenting similar appearances had been brought forward, and also in regard to the unusual features the palate presented.

The history of the case, briefly stated, was as follows: The patient, a woman, five years previously had had a bad throat. This continued for a month or six weeks before she saw a doctor; she subsequently remained under his care for three or four months. The perforation involved the greater part of the right half of the soft palate. The peculiar feature of the lesion was the remarkable absence of thickening of the edges of the perforation and of adhesions. The perforation, in other words, added to the difficulty in the discussions about those attributed to scarlet fever and congenital origin.

The PRESIDENT said it seemed to him that the perforation was due to a former gumma, but there did not seem to be so much cicatricial contraction as usual.

A Case of Laryngeal Disease in a Man aged 45, probably Tuberculous, but presenting appearances of Epithelioma.

By W. JOBSON HORNE, M.D.

THE clinical instructiveness of the case rested on this one point: there was evidence of tuberculosis, and yet the disease in the larynx, although far advanced, was limited in a remarkable degree to one side, naturally raising the question whether it was tuberculosis or epithelioma.

The patient was a man, aged 45, who had experienced a harshness and dryness of the throat, together with some loss of voice for about twelve months. The right half of the larynx was considerably thickened and œdematous; the left half was free.

A Simplified Insufflator for use in the Treatment of Diseases of the Throat, Nose, and Ear.

By W. JOBSON HORNE, M.D.

THE insufflator consists of (1) glass tubes of various lengths, shapes, curves, and calibre, to meet the requirements of the regions in which they are intended to be used. The proximal end of the tubes is roughened on the inner surface for an inch or more. This roughening of the inner surface of the tube permits of the powder being retained in situ, and also of a fixed quantity of powder being used. The tubes may be used in the following way. The proximal and roughened end is dipped into the bottle of powder to be used, and when withdrawn retains the required amount of powder. If required for the larynx or the fauces, or the post-nasal space, the distal end of the tube is inserted and the lips closed or the nostril closed upon it, and the powder is aspirated on to the diseased surface. If the insufflator is not to be used in this manner by the patient but by another person, then piece (2) of the apparatus is attached; this consists of glass with rubber tubing and a rubber ball.

The advantages of this form of insufflator over those commonly used are the following: (1) Simplicity; (2) cleanliness, the instrument readily permitting of sterilization and boiling; (3) cheapness; (4) simplicity of auto-insufflation; (5) precise dosage.

It is essential that the glass tubes should be carefully made, suitably curved, and that the part other than the roughened proximal end be absolutely smooth and thoroughly dried before being used. The insufflator and the various tubes have been made for Dr. Jobson Horne by Mr. F. Rogers, of Oxford Street, London.

Microscopical Section of Cyst of Tonsil.

By W. H. Kelson, M.D.

Patient was a man, aged 43, sent up from the country with the diagnosis of sarcoma of the tonsil. He had suffered from repeated attacks of sore throat, and the left tonsil had been slowly increasing in size for some months; he had slight pain and some difficulty in swallowing. The left tonsil was found to be very much enlarged, firm, but elastic; no enlarged glands. The tonsil was partially enucleated, and then the cold wire snare applied.

Complete Stenosis of the Larynx from Epithelioma in a Man aged 23.

By StClair Thomson, M.D.

Hoarseness was said to have come on suddenly while patient was in India in July, 1909. Admitted to Netley Hospital, March, 1910. He was then very hoarse; the whole larynx was enlarged, particularly on left side. No enlarged glands. Left vocal cord was fixed in adduction, and the posterior half and the left arytænoid were involved in the growth of an irregular tumour; no ulceration.

May 14, 1910: Tumour had grown to occupy more than half of the lumen of the larynx. After a low tracheotomy a laryngo-fissure was performed at Netley, and the left cord and tumour removed. On examination the tumour was found to be fibrous in character.

All reported to be well until June, when he had more attacks of choking, and the tube was re-inserted in the neck. Sometimes the patient had noticed swellings in left side of neck, and at those periods he could not swallow solid food. When admitted to King's College Hospital in October, 1910, the patient was wearing a small tracheotomy tube in the region of the cricothyroid membrane, and apparently through the thyroid cartilage. Through this there was both inspiratory and expiratory stridor. When the tube was blocked there was no respiration through the larynx. Much cicatricial tissue was around the wound, and along the scar in middle line. Great thickness and swelling of larynx, with tenderness, especially noticed in transverse diameter. Patient spoke entirely with the pharyngeal voice. Entrance to larynx was entirely occluded by the fixation of the arytænoids against the posterior surface of the epiglottis. No movements in arytænoids. Wassermann and von Pirquet tests were both negative.

October 14, 1910: Under eudrenine, low tracheotomy was done and a large Durham's tube inserted; and, a few days later, the upper (old tube) was discontinued. Perichondritis extended in epiglottis and thyroid alæ. The old tracheotomy wound never closed, and pus continued to ooze through it. Laryngo-fissure, but nothing fresh discovered. During exhibitor's absence in January, Sir Watson Cheyne evacuated an abscess in the left side of neck, re-opened the larynx and removed a piece of necrosed cartilage. The granulation tissue removed at the same time was shown under the microscope.

This case and specimen were shown before the Medical Society of London, and several expert pathologists agree that it is malignant. Still, not only the age of the patient, but the clinical evolution of the disease, appear quite opposed to the conclusions of the microscope.

It should be noted that there are no enlarged glands, and it is unusual for the larynx to be completely occluded by a malignant growth without any of it sprouting up through the glottis.

DISCUSSION.

The PRESIDENT pointed out that the opinion of several expert pathologists was that the growth was an epithelioma. He would be glad to hear the views of members on the case, which presented many points of interest.

Dr. SCANES SPICER said the case was one of the most interesting which had been shown before the Section. It raised the question whether perichondritis was the primary condition, or whether the case was malignant either at the beginning or even at the present time. The patient stated he was talking to some companions when his voice went *suddenly*, though it had been *perfectly well* before. He thought an arytænoid might have been dislocated, for the vocal cord appeared to have been fixed in adduction when he was examined at the hospital. Apparently the perichondritis started then, and septic invasion ensued, spreading over the remaining cartilages. There had been no ulceration and no enlarged glands, points somewhat opposed to the theory of primary malignancy. He asked what was meant by saying the tumour was "fibrous" in character. What was done in this case was a thyrotomy. An interesting question was whether this should not have been done before. In a discussion which was opened by Dr. de Havilland Hall some years ago he remembered Dr. Hall's statement that Sir Duncan Gibb had recommended that as the proper treatment of perichondritis of the larynx at an early stage, and that if there were any separated dead cartilage it should be removed by opening the larynx. On another occasion, under similar circumstances, one might entertain chondrotomy and early removal of sequestrum if it were found there was much destructive perichondritis with pus coming from the parts. He was even now dubious about the case being malignant. The structure of the specimen appeared to be that of epithelioma, but such appearances occurred in other conditions, such as chronic hyperplastic processes of epithelium, covered parts, erosions of the cervix uteri, and even in growing epidermoidal cells in blood serum and on agar, &c. Numerous references to such conditions and the authorities who had described them could be found in Roger Williams's work on the "Natural History of Cancer," p. 173 *et seq.* Because a specimen looked like epithelioma it was not necessarily malignant.

Mr. DE SANTI said he was very strongly of opinion that the case was not one of epithelioma. The slide under the microscope, with all due deference to the expressed opinions of the pathologists, did not appear to bear out the diagnosis of epithelioma. Clinically the general history and appearance of the man did not suggest malignant disease either. There was an absence of any glandular enlargement, and although there has been much ulceration and breaking down in and around the larynx, yet there was a total absence of any malignant ulceration. In some of these doubtful cases one had to rely principally on clinical evidences and experience in contradistinction to the pathologist's reports. For instance, in a case under his care a patient presented a large, hard, ulcerated, cauliflower-like mass growing from the external auditory meatus. It looked and felt like a typical epithelioma. The history was, however, a very short one, and there was a total absence of glandular infection. On further examination it was found that the patient had a hard chancre and a typical secondary rash. A piece of the growth was excised and sent for pathological report. At the same time the patient was put on mercury. When seen a week later the pathologist's report had come, and was "typical epithelioma," but the cauliflower-like growth was half the size, and subsequently entirely disappeared, and a similar kind of growth occurred in the opposite meatus. The case was one of syphilitic condylomata.

Mr. TILLEY endorsed Mr. de Santi's remarks; it was very unlikely that the lesion was epitheliomatous, it looked more like chronic inflammation.

Dr. JOBSON HORNE thought the age of the man and the fact whether or not he had typhoid fever in India, or had had syphilis, should be taken into consideration. The section did not clearly show it was epithelioma. It was very important that the specimen should be cut at right angles to the cord, or to the surface of the tumour. It was very difficult to judge from an obliquely cut section.

The PRESIDENT suggested that the specimen be referred to the Morbid Growths Committee, as it was one of great interest. The clinical aspects of the case made one think it unlikely that it was epithelioma.

Dr. STCLAIR THOMSON, in reply, said he had the patient's medical sheet sent from Netley Hospital, and it made no reference to typhoid. The man denied syphilis, and the Wassermann test was negative. There was no trace of tubercle in the lungs, and the von Pirquet reaction was negative. A low tracheotomy was done before the original laryngo-fissure was carried out.

But a month after the latter he got stenosis, a hurried tracheotomy was done, and the tube was left in for three or four months. The man did not come under his care until four months later, and then he regarded it as septic perichondritis, and as such the man was treated in King's College Hospital from October to Christmas. On doing laryngo-fissure he found nothing in what had been his larynx. He had him rubbed with mercury and gave him iodide of potassium; while he (Dr. Thomson) was away at Christmas an abscess developed on the left side, and Sir Watson Cheyne opened it; it led up to the thyroid. He showed him at the Medical Society, when several pathologists and surgeons said they could not disbelieve the section. He would be glad to submit the section to the Morbid Growths Committee.

Laryngological Section.

May 5, 1911.

Dr. P. WATSON-WILLIAMS, President of the Section, in the Chair.

Cases illustrating the Treatment of Frontal Sinus Suppuration.

CASE I.—SHOWN BY DR. STCLAIR THOMSON.

MRS. M. This patient was exhibited before the Section on December 4, 1908, fourteen days after a radical Killian operation on the left frontal sinus.[1] The left antrum was operated on at the same time, and a suppurated ethmoidal region cleared away; the sphenoidal sinus was explored and found healthy. A radiograph was useful by showing that the left frontal sinus crossed the middle line, and that there was an orbito-ethmoidal gallery running outwards behind the bridge. The patient begged to have the operation done on account of persistent headache. She has not been up for inspection until the present occasion.

The case is shown as a sample of an excellent result, both in regard to the cosmetic result and the cure of all symptoms.

CASE II.—SHOWN BY DR. STCLAIR THOMSON.

Mrs. W. This patient was operated on some years ago for suppuration in all the accessory sinuses of the right side. Her head symptoms were at one time so severe that she was first admitted to the Queen's Square Hospital.

The case is exhibited to illustrate the complete cure of suppuration, and the relief of all symptoms. But the cosmetic result has been somewhat marred owing to the accidental fracture of the Killian bridge during operation. The opening into the sphenoidal sinus is well seen.

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[1] *Proceedings*, 1909, ii, p. 51.

CASE III.—SHOWN BY DR. STCLAIR THOMSON.

Mr. M. R. This man, aged 22, was exhibited to the Society on March 4, 1910,[1] six weeks after the operation for unilateral pansinusitis; all four cavities were operated on at one sitting. With the exception of a little crust in the ethmoidal region all suppuration has ceased. The cosmetic result is satisfactory, and the patient is cured of his persistent headaches. These headaches had prevented him from pursuing any occupation.

CASE IV.—SHOWN BY DR. IRWIN MOORE.

The patient was a woman, aged 50, a hospital nurse, with suppuration of the left frontal sinus, maxillary sinus, and ethmoidal cells, on whom a radical Killian operation was performed on March 11. Patient came to the London Throat Hospital complaining of constant headache, with severe pains over left frontal sinus, and left side of face for eight years, greatly aggravated since August last, and constant purulent discharge from the left nostril (patient using six handkerchiefs a day), which prevented her from attending to her professional work.

On admittance to hospital she was in bad health and was not a good subject for operation, having had four operations during the past fifteen years—viz., uterine curetting twice, operation for hæmorrhoids, hysterectomy for fibroids, also three attacks of hæmatemesis, besides a few weeks' residence at Bow Asylum.

In December last she had influenza, followed by pneumonia, from which she has only recently recovered.

A skiagram taken before operation was useful in showing opacity of the frontal sinus, but did not disclose the full size of the sinus, which, on operating, was found to be large, extending backwards over roof of orbit, and full of polypoid membrane secreting pus. There was no connexion with the right frontal sinus. The ethmoid cells were extensively diseased and were thoroughly removed. The portion of the orbital roof forming the floor of the sinus was freely removed. An intranasal operation was performed on the left maxillary sinus which was found full of fœtid pus.

There is now (two months after operation) no discharge from the nose, and the washing from the maxillary sinus is practically clear.

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[1] *Proceedings*, 1910, iii, p. 114.

With the exception of some œdema of the eyelids the patient made an uninterrupted recovery. The headaches have disappeared, and the patient is feeling very much better, and her general health has greatly improved. There is practically no disfigurement.

CASE V.—SHOWN BY DR. IRWIN MOORE.

The patient was a male, aged 18, with suppuration of both frontal sinuses, both maxillary sinuses, and both ethmoids, complicated by external fistula. A previous operation of one sinus had been performed by another surgeon; all four sinuses and both ethmoids were recently operated on by the exhibitor. Three years ago polypi were removed from the right nostril, followed two and a half years later—i.e., March, 1910—by swelling of left eyelid, and a frontal abscess. The left frontal sinus was then opened by a median forehead incision, combined with an incision through eyebrow, and a portion of the anterior wall removed.

Condition on admittance to hospital, July 1910: Complained of severe headache, difficulty of breathing through right nostril, purulent discharge from both nostrils, and discharge from forehead since previous operation. Right nares crowded with polypi and abundance of pus present. Left nares: Polypoid condition of ethmoid, and pus seen in fronto-ethmoid region. Both maxillary sinuses contained fœtid pus. There were three discharging sinuses present on left forehead: (1) Centre of upper eyelid through which a probe could be passed into frontal sinus. (2) Above supra-orbital arch, through which a probe passed into frontal sinus. (3) Centre of median incision, through which a probe also passed into frontal sinus, and through which fluid could be syringed into left nares.

Operation (September, 1910): The left frontal sinus was opened up through the old incisions and found full of degenerated membrane and pus, which on cultivation showed a nearly pure streptococcus. The anterior wall was freely removed and a large fronto-ethmoidal cell discovered and opened up. The left frontal sinus was found to communicate with the right, which was full of pus, and its anterior wall removed. Both ethmoids were cleared away with Luc's forceps (intranasally.) An intranasal operation was then performed on both maxillary sinuses. On account of the unhealthy appearance of the frontal sinuses the wound was not closed.

Patient made a good recovery, and was discharged from hospital in November in much better health. All headache has disappeared.

A Caldwell-Luc operation has recently been done on left maxillary sinus which continued to secrete pus. It was found full of polypi and has now practically cleared up. There is still some pus seen in both fronto-ethmoidal regions, also exuding from the median forehead incision, evidently coming from the right frontal sinus. The left frontal sinus appears to be cured, and the fistulæ have disappeared. The patient is considerably disfigured by the forehead scar, which is deeply puckered.

This case affords the opportunity of comparing the older Ogston-Luc operation with the more recent and complete Killian operation, as shown in Case I.

It is proposed to re-operate on the right frontal sinus, and do a plastic operation on forehead.

Cases VI and VII.—Shown by Mr. Stuart-Low.

These two cases, both in females, aged 25 and 28 respectively, were shown to illustrate successful Ogston-Luc operations. In both instances there had been fœtid discharge from the nose with severe headaches for years. On inspecting the upper regions of the nasal cavities, septic ethmoiditis was found, more marked on the left side in each case. The anterior two-thirds of the ethmoid were first removed. The Ogston-Luc operation was then performed, the partition between the two frontal sinuses broken down, and polypi cleared out of both sinuses from the one opening on the left side. The two sinuses were then drained through one rubber drainage tube passed into the left nasal cavity. The shield used to protect the wound from bandage pressure was also shown.

The result was very good in both cases, the headaches and discharge having ceased, and even now, after the three years, there was no recurrence of the symptoms.

The Present Position of the Treatment of Purulent Discharge from the Frontal Sinuses.

An Address Introductory to a Discussion on the Subject.

By P. WATSON-WILLIAMS, M.D.

THE subject for discussion falls under two main headings :—

(1) The treatment of acute cases of frontal sinusitis.
(2) The treatment of chronic cases of frontal sinusitis.

ACUTE FRONTAL SINUSITIS.

Opinions are fairly unanimous with regard to the treatment of acute frontal sinusitis, differing mainly in regard to details.

I do not propose to say much on the treatment of acute cases, the vast majority of which recover spontaneously. Here, as a rule, our object is to relieve the acute pain and to expedite the discharge of pent-up secretions. Rest in bed, or at least in a warm room, the repeated applications of hot fomentations over the affected sinus, and the exhibition of diaphoretics, phenacetin, acetanilid, or phenazone, with purgation are helpful. I have found the external application of capsicum with menthol afford relief to the pain. What may we recommend in the way of intranasal treatment? Opinion is divided as to the desirability of applying cocaine and suprarenal extract to the inflamed mucous membrane, such distinguished authorities as Hajek and von Eicken being against the use of these drugs under such conditions. Certainly I think their repeated application is distinctly detrimental; on the other hand, it sometimes happens that applications of cocaine in the region of the fronto-nasal duct suffices to enable the purulent contents of the sinus to find an exit through the duct narrowed by the swollen mucosa, and thus relieve the tension which is attended with intolerable pain. In some cases menthol inhalation causes a free flow of watery mucus, and gives some measure of relief. I should be glad to hear the experience of others from the use of Sondermann's suction apparatus, and of Bier's method. I must, however, draw attention to Brünings's "Lichtkopfbad," which is also commended from experience in his own practice by von Eicken, to whom I am indebted for drawing my attention to the bath.[1] It consists of a box with a hole in the anterior wall, through which the head of the recumbent patient passes. There is a tube passing through the wall of the box, with a mouthpiece inside, so that the patient breathes the air from the outside. The box also contains four electric lamps, which can be turned on one after the other, when the patient is lying with his head in the box, a cloth surrounding the neck, so as to keep the heated air in the box from escaping. When the lamps are lit, leather goggles are worn to protect the eyes from the great light and heat. When the lamps are turned on the temperature of the air within the box rapidly rises to 50° C., and then upwards to 80° C. or 90° C. The skin of the head and neck soon perspires freely, and the nasal mucosa becomes less swollen, so that the previously blocked passages become pervious; the pain diminishes, and often the affected sinus discharges its purulent contents. The lamps are kept burning for about twenty to thirty minutes, and after they are turned out the box is kept in situ for about a further fifteen minutes before the patient's head is uncovered.

Thus in uncomplicated cases operations are avoided; we rely on general treatment by diaphoretics, counter-irritation, Brünings's light head bath, and intranasally on lavage and local sedatives. But in certain cases we are constrained to resort to operative measures, viz:—

(1) When the aforementioned palliative measures are unsuccessful in affording relief, especially if well-marked acute ethmoiditis co-exists. In such cases anterior middle turbinectomy, with removal of the bulla ethmoidalis and opening the lower anterior ethmoid cells, may suffice to remove the obstruction of the lower end of the fronto-nasal duct.

(2) When there is concomitant cellulitis of the soft tissues externally in the region of the implicated frontal sinus, external operations are generally called for, but should consist in opening the sinus, and lavage; the mucosa being preserved not only of the sinus itself but also of the fronto-nasal duct: only exceptionally is a radical operation needed.

If external operation on the frontal sinus in the acute stage is unavoidable, I believe there is a universal consensus of opinion that the mucous membrane should be preserved, that the operation should be restricted to opening the sinus, washing it out and, intranasally, the removal of the anterior end of the middle turbinal, and opening up the bulla and anterior ethmoidal cells, and so forth.

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[1] Von Eicken's results with this bath, and many other suggestions of great value in the treatment of acute and chronic cases, are given in his paper, "Uber die Behandlung der Eiterungen der oberen Nasennebenhöhlen," *Verhandl. der Gesellsch. deutsch Naturforscher u. Aerzte*, lxxxe Versamml. Köln, 1908, Leipz., 1909, p. 445, iite Teil, 2 Hälfte.

(3) When caries of the frontal sinus walls has resulted in an external fistula, or when there is reason to fear intracranial complications.

Under these rare and exceptional circumstances a radical operation is usually essential, and it may be necessary to completely remove the whole of the mucous lining, as well as the entire anterior wall and floor of the sinus.

CHRONIC FRONTAL SINUS SUPPURATION.

Opinion is divided with regard to the treatment of chronic frontal sinus suppuration.

Intranasal treatment by lavage and local applications to the interior of the affected sinus should be carried out systematically, after the removal of any obstruction to the duct, or of co-existing pathological conditions in the middle meatal region, such as polypus.

It is my custom to treat first any other co-existing suppurating sinus, e.g., in the corresponding maxillary antrum, or sphenoidal sinus, and above all the most common co-existing suppuration in the anterior ethmoidal cells. It may not be enough to remove the anterior end of the middle turbinated body; in that case the ethmoidal bulla and anterior ethmoidal cells may require opening up, and of course any polypi removed completely, so that free drainage of these small cavities aids the beneficial effects of lavage of the frontal sinus itself. Shall we refrain from operating on a suppurating maxillary antrum or a sphenoidal sinus before operating on the frontal sinus, because these cavities may be re-infected by the frontal sinus suppuration? I have no hesitation in operating on all other involved sinuses, before resorting to an external frontal sinus operation, for when these other sinuses have been freely opened it is relatively easy to keep them freely washed out, and not only are the nasal passages in a healthier state, but the resulting improvement in the patient's health is itself a by no means negligible factor in aiding us in obtaining a good result from lavage of the frontal sinus itself.

I do not always confine myself to simple saline solutions for the frontal sinus lavage, but after the free irrigation with such solutions I have found much benefit from weak solutions of iodine or of protargol 15 per cent., and in the more inveterate cases nitrate of silver, varying in strength—40 gr. to the ounce or more. The stronger solutions of silver nitrate cause marked discomfort for a short while, but if this be too severe the effect can always be modified by irrigation with a solution of common salt. Another solution for injection after simple saline, or peroxide of hydrogen irrigation, is a solution of menthol, eucalyptol and terebene in colourless oil of paraffin.

I have had a number of permanent cures of frontal sinus suppuration by these relatively simple means, and although I strongly advocate radical external operation in a restricted class of case, I believe implicitly in the complete efficacy of these intranasal methods in a considerable percentage, and in their being more desirable methods of treatment to continue, even if not resulting in absolute cure, provided the symptoms are not such as to make more drastic operative treatment really necessary.

Vaccines of autogenous bacillary emulsions are undoubtedly helpful in some cases, but while they may afford a measure of relief, vaccine treatment rarely results in cure.

Intranasal operations with a view to enlarging the fronto-nasal passage, and so effecting drainage and cure, are practised and advocated as successful, particularly by Spiess, Halle, and Fletcher Ingals.

As I have never done these particular operations myself, and have never seen any patient on whom they have been performed, it is hardly possible to form a personal opinion worthy of much consideration. We shall hope to hear the views of those present who have had actual experience of the results to be obtained. Writing to me on February 4, 1911, Dr. Fletcher Ingals said:—

> I may say that my confidence in the operation is greater each time that I perform it, and I feel that everything you may find in the paper that I read at the International Medical Congress, Budapest, is correct.

Fletcher Ingals having performed his operation on twenty-nine cases, invited special attention to the following statements in that paper:—

> It can be done in almost 95 per cent. of all chronic cases and in the majority of acute cases, provided the anterior end of the middle turbinated body has first been removed.
>
> Experience has shown that the canal left by this operation is as large as desirable, and that the drainage is ample and remains so. This was true in all my cases, even in the one that was not materially improved by the internal and external operations.
>
> I believe this operation as safe as any other that has thus far been described for treatment of suppuration of the frontal sinus.
>
> This operation will surely establish free drainage even in some cases of empyema, in which it may not be sufficient to effect a cure. In cases that do not greatly improve within a few weeks, the frontal sinus should be opened

externally and cleaned out. The enlarged drainage canal resulting from the intranasal opening of the sinus would allow free drainage into the nose and would lessen the danger from the external operation.

Healing is apparently as rapid after this as after any other operation on the frontal sinus. Probably 95 per cent. of all suitable cases will practically recover within from two weeks to six months.

One cannot lightly set aside such authoritative statements based on experience; but while one is tempted to try the method of intranasal drainage, there are certain objections which rise up in one's mind, viz., the inherent danger in such methods of opening into a frontal sinus, although the dangers have been greatly diminished by improved technique and the aid of the X-ray screen. Further, even if we concede that a free opening into a frontal sinus can be effected without immediate risk to the patient, it may be objected that:—

(1) It lays bare and exposes a bony channel to the invasion of virulent organisms, without removing the pyogenic mucous membrane, which remains to perpetuate the flow of pus.

(2) While depending on the maintenance of an enlarged fronto-nasal passage, the mucosa of the fronto-nasal duct is largely destroyed, with consequent tendency to cicatricial stenosis.

(3) It must often prove impossible to effectually open up and remove all the involved fronto-ethmoidal cells, or irregularly developed ethmoidal cells.

In a large proportion of chronic frontal suppurations the fronto-nasal duct becomes abnormally patent from atrophy of the lining mucous membrane, hence these operations, which can only ensure free drainage for the sinus while adding fresh risks, establish a condition which, though often existing in chronic cases, does not after all result in cure.

Too much stress, it appears to me, is laid on obstruction in the fronto-nasal passage, as the essential cause of the persistence of frontal sinus suppuration, and more particularly of the recurrent headache associated with it, as though any operative method which removed such obstructions fulfilled all the conditions essential for curing the patient. Undoubtedly obstruction preventing the escape of muco-purulent secretion immensely aggravates the pain of frontal sinusitis, but many patients whose fronto-nasal passage is never obstructed suffer from recurrent exacerbations and headaches, just as a chronic bronchitic suffers from grave discomfort and tightness in his chest in the early stages of recurrent attacks, symptoms which are relieved when the bronchitic secretions become "free"; yet no one talks about the tightness of the chest being due to obstruction, and we ought to rid our minds of the fallacy of attributing chronic frontal suppuration solely to some obstruction which in many cases has long ceased to exist.

Indications for a radical operation are:—

(1) The continuance of a purulent secretion, despite intranasal and other treatment, sufficient to cause grave inconvenience or markedly impaired health.

(2) Persistence of headache, or of mental depression, or other nervous phenomena due to frontal sinus disease, especially in neurasthenic patients.

(3) Recurrent facial erysipelas, external or orbital cellulitis, caries of the sinus walls, the formation of a fistulous opening, and any symptoms suggestive of intracranial complications secondary to the sinus.

The age, occupation, and the social and financial status of patients, as well as the size and shape of the involved sinus, as revealed by skiagraphy, should be taken into consideration in coming to a decision for or against advising a radical operation.

I think there is no class of cases for which it is more difficult to formulate statistics of the results of operative treatment than those of nasal accessory sinus suppuration. It is difficult to compare one case, or one method of treatment with another, unless one is in full possession of the conditions from which the patients were suffering. Thus one meets with chronic frontal sinus suppuration, complicated only by the involvement of the frontal-ethmoidal cells; whereas another patient, in addition to a chronic purulent discharge from the frontal sinus and the fronto-ethmoidal cells, may have chronic antral disease with extensive involvement of the anterior and posterior ethmoidal cells, with a nose full of polypi, and also with suppuration in the sphenoidal sinuses. Then again, the infective organism varies so greatly in virulence, and, to put it broadly, in tenacity to life. A virulent streptococcal or pneumococcal infection is usually more difficult to subdue than staphylococcal infection, or at any rate may have a *Micrococcus catarrhalis* with or without some form of *Bacillus coli*, which gives an exceedingly offensive discharge, often even without markedly virulent properties. Furthermore, one patient will have a frontal sinus which is of the ordinary size, or perhaps even less than the average size, whereas in another it may be so extensive as to reach far across the mid-line on to the other side, measuring 2 in. or 3 in. in its transverse diameter, and extending upwards more than half-way up the surface of the frontal bone, or backwards to two-thirds of the depth of the orbital roof. One sinus may have a relatively smooth internal surface, and in another there are small channels of communication, with deep-lying frontal bullæ on the anterior or posterior wall, or other irregular developments of the ethmoidal cells. Then we have in other cases caries of the walls, and sometimes perforation, with the formation of external fistulæ. While in the first-mentioned conditions, operative treatment may be expected to yield completely successful results, the last-mentioned complications enormously handicap the operator in obtaining complete and lasting cure.

The question of operative technique is not an essential point in our discussion. What we are considering is, how far active operative interference compares favourably or otherwise with the more *laissez-faire* attitude, to which some practitioners have been driven by their failure to gain success from radical operative or other active measures of treatment. There is a considerable percentage of cases in which the symptoms are so exceedingly acute—frantic pain in the head from which they suffer, mental depression, in which the chronic suppuration sometimes results in causing even mental aberration, so that it is imperative to do more than rely on mere palliative measures; and in other cases, even where such acute symptoms are absent, the patient's means of livelihood may be so seriously interfered with, or his health impaired, that it becomes a matter of the very gravest importance for him to be restored to such measure of improved health that he can provide for himself and his family—even though one may not be able to obtain the ideal condition of affairs, or complete cure. If one enables a working man, who otherwise must involve both himself and his family in starvation and misery—to carry on his work, it is surely idle to say that the operation, by which such a happy consummation is obtained, is a failure because the nasal discharge and perhaps occasional headaches are not entirely done away with.

Turning now to the question of radical operations. The details of operative technique are hardly within the scope of our discussion, which concerns results rather than the minuter details of treatment by which we seek to obtain those results, and in any case it would be superfluous in this assembly to describe the numerous operations which have been advocated by various authorities. But we are concerned with types of operations; for instance, if a patient has a small or shallow sinus, we need not hesitate so long to obliterate it by the operations in class 4, which also have advantages in men of the working class, for whom greater likelihood of complete and lasting cure outweigh the greater liability to deforming scars.

A radical operation on the frontal sinus connotes an operation which aims at the obliteration of the sinus.

As types of the methods advocated with this object we may cite :—

(1) The Ogston-Luc operations.

(2) Killian's bridge operation, the operation which is most widely known and practised.

(3) Watson-Williams's osteo-plastic operation.

(4) Delsaux's, Riedel's and Kuhnt's operations, involving the total removal of the anterior wall and the floor of the sinus.

If a maxillary antral empyema or purulent discharge from the posterior ethmoidal cells or sphenoidal sinus co-exist, these sinuses should be opened up and treated before resorting to any external operation, unless special circumstances in the case make it unavoidable to delay radical operation, in which case these cavities should be explored, at any rate before the external operation is done, and if containing pus they should be operated on at the same time.

Presuming that a radical operation is necessary, the particular type of operation to adopt must be determined by the nature and conditions of the individual case, and no one operation can be held the best under all circumstances. The ultimate cosmetic result and the success or failure of a radical operation to cure the discharge and other symptoms (apart from the method adopted and the skill of the operator) largely depend on the anatomical conformation of the involved sinus or sinuses, the nature and character of complicating factors, and to some extent on the virulence and type of the infecting micro-organism. Hence it is difficult to compare, by statistics alone, the relative merits of minor intranasal and radical external operations.

Unless the indications for a radical operation are definite, one should rely on minor intranasal operative, palliative, and other therapeutic measures.

If a radical operation is advised, one should adopt a method which allows a free access to the whole of the involved territory and the ultimate obliteration of the suppurating cavities with the least cosmetic defect.

A complete cure, with cessation of all purulent discharge, headache and pain, cannot be assured beforehand, and although in suitable and apparently uncomplicated cases a radical operation will almost certainly afford much relief, the possibility of a fatal result from osteomyelitis is always a factor to be reckoned with; for, although so far I have had no fatal result, there are no certain means of determining what complicating conditions may be encountered, before the frontal sinus has been opened and explored.

I may perhaps be permitted to state my views on my own osteoplastic method very briefly. I usually prefer my method to Killian's, because it affords a freer access to the sinus and opens up the duct and the neighbouring fronto-ethmoidal cells to a more complete inspection, and enables one to look right down the nasal passage and attack the middle turbinal plate and the ethmoidal cells from in front instead of from the outer side. Thus one sees more easily where one is going. With Killian's operation, if we leave a broad bridge, it obstructs the view to some extent, renders access to the ethmoidal cells more difficult; whereas, if the bridge is narrow, the bone is liable to become absorbed. Twice this has happened to me from recurrence or persistence of suppuration. In one case there was cellulitis of the external tissues before I operated, and it is quite possible an osteoplastic operation would have been equally unsuccessful, though I think it less liable to such misfortune, as there is no narrow bridge covered only with a narrow periosteal ribbon. On the other hand, I prefer Killian's method if a fistula has formed, or the sinus has been opened through the floor, and several of my most successful cases have been operated on after the method of Killian. Where I have not had success, it may perhaps have been due to my failure to imitate Killian's manipulative dexterity, or to faithfully follow his technique.

In tabulating the results of the last twenty-eight cases in which I have done radical operations, I have indicated in the most superficial manner the nature of the operation undertaken, in which it will be seen that in most cases the frontal sinus disease was complicated by other sinus troubles, and I have indicated the result only after a very considerable period, as I do not think that a good result which may last for a year after operation is a certain indication that the good will persist. Although I have put down in one column the different sinuses which have been attacked, this does not imply that they were always done at the same time. In several cases considerable periods have intervened between the operations on the two frontal sinuses and in nearly all cases antral or sphenoidal sinus operation has preceded by a good while the operation done on the frontal sinus. My purpose has been simply to give a general indication of the nature of the case and a fair indication of the result obtained.

SUMMARY OF THE AUTHOR'S LAST TWENTY-EIGHT CASES OF RADICAL OPERATION FOR FRONTAL SINUS SUPPURATION.

Case	Operation		
1	Watson-Williams radical frontal sinus; left radical antral; right and left sphenoidal sinus	Still has some headaches and nasal discharge, but very much less than formerly; health greatly improved	Much relieved
2	Double Killian; double radical antral and sphenoidal sinus	No headaches, no discharge; health excellent	Cured
3	Fifteen years before, right frontal sinus had been opened by another operator; double Killian radical, floor not removed	Two years later still had some discharge and headaches	Relieved
4	Right Killian; floor not removed; packed; and double radical antral	Has little discharge and sometimes headache	Much improved
5	Right and left Killian; floor not removed; packed; double radical antral; nasal polypus	Frontal sinus cured, but some discharge from ethmoidal cells	Much improved
6	Left Killian; floor removed; left radical antral	No nasal discharge; headache slight and rare	Cured
7	Left Killian; left radical antral	No headache or discharge from frontal sinus	Cured
8	Double Watson-Williams radical frontal sinus; double antral operation; right and left sphenoidal sinus	Occasional headache and discharge	Much improved
9	Right Watson-Williams frontal sinus; double radical antral	No discharge; occasional headaches	Much improved
10	Operation fifteen years previously by another operator; right Killian; floor not removed	Some discharge and occasional pain in left frontal sinus region	Much improved
11	Left Watson-Williams radical frontal sinus, right radical antral operation	Complains of post-nasal discharge, but is very much better	Much improved
12	Double Killian; double antral	Feels depressed; no headache, and very little discharge	Much improved
13	Double Watson-Williams radical frontal sinus, left posterior ethmoidal and sphenoidal sinus operation	No headache; no discharge	Cured
14	Double Watson-Williams radical frontal sinus; recurrence, frontal sinuses opened; right and left sphenoidal operations	No discharge, but still gets headache, mainly occipital; very neurotic	Much improved

SUMMARY OF AUTHOR'S CASES—*continued.*

Case	Operation		
15	Double Watson-Williams frontal sinus; double antral	No discharge, except a little muco-pus from left posterior ethmoidal cells; very occasional headache	Much improved
16	Double Watson-Williams frontal sinus; right and left sphenoidal sinus operation	No discharge; no headache	Cured
17	Double Watson-Williams frontal sinus; double antral operation; enormous sinuses; recurrence, required re-opening and draining of outer angles of the sinuses	No headache; no nasal discharge; there is a small discharging sinus in mid-line which causes him no inconvenience	Much improved
18	Left Watson-Williams radical frontal sinus; antral and sphenoidal sinus operation	Discharge and headaches	Not improved
19	Left Watson-Williams radical frontal sinus	No headache or discharge	Cured
20	Right and left Watson-Williams frontal sinus; double radical antral	Headache and nasal discharge persist	Relieved
21	Formerly had Killian operation on right frontal sinus; left Watson-Williams frontal sinus operation; polypi; recurrence of frontal sinus discharge	Nasal discharge and some headache	Slightly improved
22	Right and left Watson-Williams radical operation (enormous sinuses); re-opened for recurrence of discharge	No discharge; no headaches	Cured
23	Left Watson-Williams frontal sinus; double antral and sphenoidal sinus; later right Killian	Headache; some discharge from right antrum and sphenoidal sinuses	Frontal sinus cured
24	Right Watson-Williams frontal sinus; double antral; both sinuses enormous; recurrence	A little discharge from right nose; headache sometimes; very much better	Much improved
25	Double Killian sinus; left sphenoidal sinus and antral operation	No discharge; no headache	Cured
26	Right and left Watson-Williams frontal sinus; double antral	No discharge; no headache	Cured
27	Double antral and sphenoidal sinus; double Watson-Williams frontal sinus; double antral	No discharge; no headache	Cured
28	Left Watson-Williams frontal sinus operation; left radical antral	No headache or discharge	Cured

Thus of my last twenty-eight cases submitted to radical operation on the frontal sinuses, eleven were cured (three Killian method, seven Watson-Williams operation, one Killian and Watson-Williams—right and left); thirteen were much improved (four Killian, nine Watson-Williams); two were improved, but were not satisfactory; two were decidedly unsatisfactory in result; both, however, were complicated cases—one had aortic and other valvular heart disease, and was intensely neurotic, and the other had very extensive ethmoidal cell suppuration as well as antral suppuration. Or expressed in percentages we get the following results: Cured, 39·28 per cent.; much improved, 46·42 per cent.; some improvement, 7·1 per cent.; result unsatisfactory, 7·1 per cent.

In this list both hospital and private patients are included: at the same time, I would add that the latter are much more satisfactory than the former, and yield a far higher percentage of complete cure, probably owing to the greater care in keeping the nasal passages clean during the stage of convalescence. Of those that are classified as "much improved" there are a considerable proportion that might be claimed as practically cured of the frontal sinus symptoms, while of others it may be justly urged that the disease was by no means confined to the frontal sinuses and ethmoidal cells. In all the discharge was of long standing, often ten to fourteen years or more, and every patient suffered from severe headaches.

DISCUSSION.

Mr. HERBERT TILLEY congratulated the President on his excellent précis on the subject, which was so clear that it left but little for other speakers to add. The interest of the discussion centred in that of chronic suppuration of the frontal sinus, either alone or associated with ethmoidal disease, or possibly an overflow into, or suppuration of, the antrum, or of the sphenoidal sinus. As the President said, one could not lay down any particular operation for all cases: the suitable operation must be selected for each case. With regard to lavage in chronic suppuration of the frontal sinus, the question arose, when did an acute case become chronic? He did not know where the line could be drawn, but for himself would set the time at six weeks to two months, after which there was not much probability of getting relief from irrigation, although one might have removed the middle turbinal and provided free intranasal drainage. During the last influenza epidemic he treated five cases by irrigation, and only one of them got quite well. He first saw the cases six or seven weeks after the initial attack of influenza; the frontal sinuses still suppurated in spite of intranasal operations, and he did not think there would be a cure unless an external operation was done, though he did not say that such operation was necessary. With regard to vaccines, his experience had been unfortunate from the point of view of curing the patient. He had had autogenous vaccines prepared, but in the majority of cases the patients had not got well. He thought vaccines would not cure many chronic cases, because when suppuration had been going on for a long time, the mucous membrane became so thick, and there was so much hyperplasia of the subepithelial connective tissue, that although vaccination might produce a secretion free from organisms, yet directly the vaccines were suspended and there was the least "cold" or infection, the suppuration returned. There might be temporary relief, but he did not think there was permanent cure. In the last eighteen years he knew of only one case which was absolutely cured by vaccines, and that was one in which the vaccine was prepared and administered by Sir Almroth Wright. Much depended on whether the infection was a pure one. If it were, a vaccine was much more likely to be curative. If a sinus had been suppurating a long time and the infection was a mixed one, he generally maintained there was very little chance of a vaccine doing good, because it could not reduce the excess of mucous membrane and the hyperplasia which he had mentioned. He agreed with the President as to the difficulties of Ingals's operation. He had no right to condemn the operation, as he had not tried it, and he had not performed it, because he could not conceive how success could be attained in those cases so common, where there existed not only frontal sinus suppuration, but where the chief trouble was suppurating fronto-ethmoidal cells which ran out under the floor of the sinus. He did not think supra-orbital neuralgia had been a prominent post-operative complication in his practice, except in a few patients for a day or two after the operation. With regard to the question of any intervention at all in chronic frontal sinus suppuration, ten years ago one operated on practically all cases in which there was suppuration of the frontal sinus, and many such cases were dealt with in a year; Dr. StClair Thomson and he generally had their hands full of such cases in the Golden Square Throat Hospital. But, as he mentioned at the last meeting of the British Medical Association, the pendulum had now swung back, and he now operated on only four or five cases a year.[1] That change, in his opinion, as to necessity for operation, had come about because patients seen fifteen years ago in the higher walks of life who refused external operation, but from whom one had removed the middle turbinal and made a freer fronto-nasal passage, and then washed them out the sinus, so that the headaches were reduced or cured—now seemed satisfied, although some suppuration still persisted. He had treated many such cases, and to-day they were in no worse condition than when he first saw them. It was, of course, true that patients in whom the suppuration persisted were swallowing noxious matter, and such might induce intercurrent complications, but he could only remember three cases of patients having died from the direct result of frontal sinus suppuration which had not been operated upon. He remembered at least a dozen, either in his own practice or those seen in the practice of others, who had died of osteomyelitis as the direct result of intervention, and these patients before operation had been well except for a discharge of pus and possibly some headache. After fifteen years of practice one arrived at general principles which guided one's action and practice. And the conclusion he had come to was, that if one freed the middle meatal region and the fronto-nasal duct so that one could enter the frontal sinus and occasionally wash it out and keep it clean, one relieved the patient of the headache and made him comfortable. It was better to act in that way than to run the risks attending an external operation. On the other hand, where the discharge was profuse, or other symptoms severe, one should advise operation. Two days ago he operated upon an elderly man who had no symptoms beyond a small displacement of the right eye, causing diplopia. There was found to be a small fluctuating swelling under the right eyebrow, and when that was cut down upon it was found to be full of pus and to communicate with the frontal sinus. The roof of the orbit and posterior wall of the sinus had been destroyed. The patient had not any symptoms except double vision, and yet was in a dangerous condition. He would like to know how Dr. Fletcher Ingals would cure such a patient by an intranasal operation.

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[1] *Brit. Med. Journ.*, 1910, ii, p. 1612.

Dr. StClair Thomson contended that there was much loose talk still about frontal sinus operations; they seemed to be spoken of as if the frontal sinus operation was to the laryngologist what the operation for appendicitis was to the general surgeon. But, as Mr. Tilley had said, those who had long operated on the sinus now did fewer, not more, operations. He had had sent him the statistics of a large American special hospital, and they recorded only two frontal sinus operations done in the year among the whole staff. It was right to remember that fatal cases had occurred, and still occurred. He had published his own fatal cases, as did Mr. Tilley and Dr. Lambert Lack; and Dr. Logan Turner summarized all published cases in a paper, giving twenty-four fatal cases from the operation. Fatalities might occur from the operation in the very best hands. He felt much less anxiety since he adopted the Killian operation, and he now held out brighter prospects to patients than formerly. Still, the Killian procedure did not exempt from danger. Killian himself had eighty-six cases in his clinic operated upon without a fatality, but before he had completed his 100 cases he had three deaths; these were due to circumstances which might happen to anybody. One was because he had overlooked a sphenoidal sinusitis, and infection spread to the brain. In another case infection had spread up through the middle turbinal. If there were any gentlemen present who had not had a death from the operation, the probabilities were that they would, in time, come to their Waterloo. He had not had fatalities since he adopted the Killian operation, but it was a serious procedure. It was not known what the prospects were if the patient was left alone. At a meeting of the Medical Society of London he had read a paper recording two deaths from untreated sphenoidal sinusitis, giving the full post-mortem notes.[1] He had also collected forty cases from literature of deaths from sphenoidal sinusitis which were not operated upon. That was valuable, because there were many cases which were not published, and in which the diagnosis had never been made; the patient had headache after influenza, and died, the death being certified as due to meningitis. Probably many of those cases were meningitis from sphenoidal sinusitis. There were no statistics to go upon in regard to the frontal sinus, and he appealed to pathologists to say in every case of death with cerebral symptoms whether every sinus in the head had been examined. He knew of only two cases. One died with abscess in the frontal lobe, and the other would have died if it had not been operated upon. A colleague in Marseilles said he had followed up ten cases of frontal sinusitis for ten years; only one of the patients was dead, and this death was due to another cause. With regard to cases which should be operated upon, he would keep before him the three indications which the President mentioned. In his own practice headache was the symptom which he placed first in agreeing to operate, because his practice now was to operate in such cases only if asked to do so. There were some cases in which the condition made life unbearable; he had brought three cases of the kind to the meeting. Sometimes the mere diminution of the discharge after operation was more disagreeable than the more plentiful discharge, because it was more inspissated. Some cases would put up with the discharge if the headache ceased and they were able to earn their living. One of the cases shown—a hospital patient—presented a very good result. Intranasal treatment had been tried, but failed. He had not seen her for a year, but she never now washed the nose out, and had remained in good health. The second showed how important was a Killian bridge. In that case he made the bridge too thin, so that it cracked and exfoliated. But it was a cure, and the patient was, perhaps, better than any of them, because there was obliteration of the "dead space" behind the bridge. He would conclude his remarks by saying that to have suppuration in the frontal sinus was not so dangerous as had been thought, but that to operate on a frontal sinus was more dangerous than many people imagined.

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[1] *Trans. Med. Soc. Lond.*, 1906, xxix, p. 14.

Dr. DONELAN said he had operated on a great many cases, but he thought on the whole more justice could be done to patients by being more conservative with regard to the radical operation. In illustration he might mention a case of double sinusitis in which he operated on the left sinus four years ago. On the right side only the middle turbinal and some polypi were removed, and by regular douching and occasional injections of nitrate of silver the result seemed quite as satisfactory as that on the left side. He was fortunate so far in not having had a fatal case. He attributed that to the selection of cases of long standing, in which middle turbinectomy had been ineffectual, and to great care in obtaining as aseptic a condition of the site of operation internally and externally as possible. He had no experience of Ingals's operation through the nose, but he had used a tube modelled on that of Ingals, and found it an excellent drain in connexion with the Ogston-Luc operation. For retention for more than two or three days he thought it should be made of rubber. With regard to acute cases he had seen a fatal case several years ago during a severe attack of influenza. The patient died of meningitis, and both the frontal and sphenoidal sinuses were found full of pus.

Mr. F. J. STEWARD said that with regard to the really acute cases—cases in which acute suppuration occurred in the frontal sinus, generally with influenza, not those which had been chronic, and then became acute from some complication—-his treatment had been very simple; and he took the opportunity of referring to one small point, which he had found most successful, and which had not so far been mentioned. When he saw a case of acute frontal sinus suppuration, the patient usually being in great pain with severe headache, much tenderness, and a rise of temperature, he had found that by simply plugging the middle meatus with a freshly prepared solution of cocaine and hemisine, free discharge and relief of the symptoms had nearly always been brought about. That measure had relieved every acute case he had seen except one, and that was the case of a man with typical symptoms after influenza. In this case he had made a small opening into the sinus, and after washing out the sinus for three days there was no further suppuration. Another point with regard to acute suppuration was, that although the washing out of the sinus appeared to be very useful at times, he was very doubtful as to whether it was often really necessary. If the sinus could drain through the natural opening, after possibly the anterior end of the middle turbinal had been removed, the majority of the acute cases got well without washing out. And when one remembered how complicated the cavity of the frontal sinus was, with its posterior extensions along the roof of the orbit, with the two walls often practically in contact, it was difficult to believe that washing out could really reach those outlying parts. As to whether the cases cleared up or not, he was uncertain, but he thought that a good many did so. In a certain number, however, though they were relieved of their symptoms, they continued to have discharge. With regard to the chronic cases, he was convinced that fewer and fewer such cases needed external operation. He thought the fact of the operation being done less often now was due to more pains being taken with the preliminary treatment, namely, removal of the anterior end of the middle turbinate, opening up the anterior ethmoidal cells, and removal of polypi. If that were done persistently, he thought the great bulk of frontal sinus cases would drain satisfactorily. That did not necessarily mean that drainage was entirely dependent on the patency of the fronto-nasal canal. In some cases he thought that might be widely open, and yet drainage would not be efficient. That seemed to be due to the fact that the cavity of the frontal sinus in such a case as that was so complicated that it could not completely drain through the fronto-nasal canal. As showing the value and potency of simple, free, nasal drainage in bringing about cure, he would mention two cases which had made a good deal of impression on him. The first was that of a woman, aged 42, who, in August, 1900, came to the out-patient department with severe headache and all the signs of frontal sinus suppuration. She also had ethmoidal disease. Numerous small polypi were removed, as was also the anterior end of the middle turbinate. But the condition did not improve, and as the headache and tenderness continued, she was advised to have an operation done, but refused. She then left London to keep house for her son in Manchester, where she remained until 1907, when she again attended, and was found to be completely cured without having had any further treatment. The other case was that of a doctor's wife, who had been under treatment for seven years, had also refused external operation, and had finally got quite well. When operation was done for the condition, he thought it should be the most thorough one possible; and unless it was absolutely necessary to avoid deformity as much as possible, he did not believe in keeping a Killian bridge, but in removing the entire floor, anterior wall, and all mucous membrane as completely as possible. This, he believed, would in the majority of cases result in a cure.

Dr. D. R. PATERSON remarked, in answer to Mr. Steward's observations, that as complete an operation as he could desire could be done and a Killian bridge still be kept. He thought the Killian operation might be improved upon in not being too sparing with regard to detaching the trochlea. He did not hesitate to strip the periosteum as far as the outer part of the orbit, so as to get at the temporal recess, where the disease was very apt to lurk, and was most difficult to reach. In his last few operations he detached the trochlea to get more room, and the subsequent diplopia was very slight and temporary. Some months ago, in a case where the temporal recess was large, he had to go so far out than it was necessary to put a drainage tube in the temporal fossa. If one confined oneself to the strict Killian operation it was usual only to go as far as the supra-orbital notch. He had seen the head bath in action in one of the Continental clinics, and the result was excellent; it was a very valuable addition to our methods of treatment for neuralgias of various descriptions. After one or two of his earlier operations there was supra-orbital neuralgia. In subsequent cases he had pulled out the nerve, as recommended by Killian. With regard to the cases which ought to be operated upon, he agreed with the President, Mr. Tilley and Dr. StClair Thomson, that the most important matter was the persistence of headache and mental depression. With regard to the results of the operation, he had a fatal result after he had operated on thirty such cases. This patient, a man, aged 30, came into hospital suffering from pansinusitis. He was in a very miserable condition, the whole of his nose being blocked, and the mucous membrane swollen and œdematous. There were no polypi. From the nostrils pus was streaming. Dr. Paterson first opened both maxillary antra, and from that operation there started osteomyelitis, which spread to the frontal sinuses. These were operated upon, as were the ethmoidal and sphenoidal cells, but an illness of eighteen months' duration terminated in death. The point about this case was that it was the operation on the antra and not on the frontal sinuses that set up the osteomyelitis. A few months afterwards he met with a very similar case, in a girl, who had a swollen, œdematous condition of the mucous membrane, with pus pouring from both nostrils, and pansinusitis. After explaining the risks to her, she declined to have anything done, and she died six months later of some intercurrent affection. He suspected that such cases were not uncommon: there were cases where the disease was such that it led to lowering of the system, so that comparatively trifling affections caused death. In judging whether a particular patient should be operated upon or not, one had to bear in mind, on the one hand, the effect which the general disease with its continuous discharge had upon the general health, and on the other, the risk of starting an osteomyelitic process by operation.

Mr. CHICHELE NOURSE thought it a matter for congratulation that the President had given such an excellent résumé, because it showed at a glance the chief points of the subject. He recalled that in the year 1899, at the meeting of the British Medical Association at Portsmouth,[1] when he reported work which he had been doing on the treatment of frontal sinus suppuration by means of injections through a cannula passed into the frontal sinus from the nose, which he then thought might prove successful, this mode of procedure was deprecated by some of the speakers as being less safe than a radical operation. Since then, time and increased experience had led to a modification of views on all sides. For his own part he found that although the use of injections might check the discharge of pus and produce an apparent cure, yet it was, as a rule, merely palliative; that the disease was rendered latent, but that the discharge was liable to recur from trivial causes. He was able to confirm the observations of the President that the fronto-nasal canal was often unusually patent in cases of chronic suppuration of the frontal sinus. It was clear also that free drainage alone was not sufficient to lead to cure of the disease when the mucous membrane was in a state of degeneration, although a want of free drainage might have been one of the factors which led to its development in the first instance. Under such circumstances nothing but a radical operation would get rid of the disease. It was yet a matter of doubt whether one would be justified in leaving the patient unoperated on, whose frontal sinus contained a vascular and œdematous lining with infective microbes, not only on its surface, but embedded in its substance. Without asserting that one should operate in all cases, he thought in the cases coming under the three headings which the President had enumerated operation should certainly be done. Persistent headache, although a sign for operation, did not seem, as far as his experience went, to be due to retention of discharge, but was caused in some other way. The Ogston-Luc operation was that with which he had had most experience. So far he had been fortunate enough not to have had a fatal case. If he had, he would have felt bound to publish it. The chief danger seemed to be re-infection of the operated area from other septic foci in the nose or other accessory cavities. In order to obviate that, he had adopted the method of passing a tightly fitting drainage tube from the sinus through the fronto-nasal canal, and leaving it projecting from the nasal orifice in such a way that the healing sinus was cut off from every source of infection, until the denuded surface of bone had become covered with granulations. The tube was left in place for ten days or more. Some force was required to introduce it: and though it caused pain in one or two cases, it was usually borne well. He had had no case of osteomyelitis after operation. After removal of the tube the fronto-nasal canal sometimes became obstructed with granulations, and the wound temporarily re-opened at the inner angle, but it soon closed again.

Dr. DAN MCKENZIE said that out of the details of the discussion would emerge one striking point—namely, that there had taken place in the last three or four years a definite subsidence of the high tide in favour of the operation which was done for the condition. That would render the present discussion historical, and it fully justified the holding of it. What had caused that change of opinion? The first reason was that the best operation under the best circumstances, and with the best operative results, did not invariably lead to the cessation of the suppuration. Secondly, that under the best circumstances, and with the best operators, a patient who had submitted to frontal sinus operation, for a comparatively trivial cause, might die—the cause of death being osteomyelitis in most cases, and not meningitis from injury of the cribriform plate. Here was a point in which there was need for further research. What was that osteomyelitis, and why was it so frequent in operations on the frontal sinus? Very little had so far been learned as to its pathology or ætiology. That day a case had been reported in which death occurred from a Killian operation, the cause of death being, it was said, infection from the sphenoidal sinus. But there was scarcely an occasion on which the frontal sinus was opened in which some lurking focus did not persist after the operation: it was impossible to make quite sure that every vestige of suppuration had been removed; yet, in spite of that, only a few cases became affected with conditions which led to death. What was the circumstance which led to osteomyelitis in these few cases? On comparing frontal sinus operations with mastoid operations in this respect there was found to be a vast difference. But it was not yet known why osteomyelitis happened more frequently after frontal sinus operation than after mastoid operation. That matter should be investigated, because it was unsatisfactory to go on washing out their patients week after week, or month after month. If osteomyelitis was inevitable in some cases, it was important to know why. But the disease might be due to something which a better acquaintance with its pathology would enable surgeons to avoid. He agreed with all that had been said in the discussion as to being conservative in frontal sinus operations, because he had had a death from osteomyelitis; and anyone who had had an experience of that kind would not readily forget it. It was an experience which would lead him to go on washing out for a long time rather than submit a patient to the risk of a fatal ending as a result not of the disease, but of the operation.

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[1] *Brit. Med. Journ.*, 1899, ii, p. 995.

Dr. IRWIN MOORE said he had had considerable experience in chronic frontal sinus suppuration, having had the opportunity of working with Dr. StClair Thomson—in fact, having assisted him with most of the cases he had already shown before this Section. He brought forward two cases to-day to enable a comparison to be made between the older operation and the complete Killian. In the latter case, which was only operated upon two months ago, the patient might be said to be completely cured. She had had persistent headache for eight years, and chronic nasal catarrh, and a very fœtid maxillary antrum on the same side. She was a nurse, and could not follow her profession except a few small cases, and she believed she would return to full work in a few months' time. The other patient had previously had one frontal sinus operated upon by the Ogston-Luc method; he found that sinus still suppurating, and three channels discharging from it; also a suppurating frontal sinus on the other side. Both ethmoids were crowded with polypi and pus, as also were both maxillary sinuses. He operated upon all four sinuses, and it had only been a partial success, because he did not do a complete Killian, but followed out the lines of the old incision. There was still more to be done on the right frontal sinus, though both maxillary sinuses were cured. In treating chronic frontal sinus suppuration, having decided to operate, it was important to do a complete and thorough operation.

Dr. ADOLPH BRONNER said he was glad to hear members conclude that frontal sinus disease should not be operated upon unless it was a matter of urgency. He had a case of osteomyelitis twenty-three years ago, and had been somewhat afraid to do the operation since. He only performed Killian's operation in urgent cases, or when there was a fistula. Patients died not only of post-operative osteomyelitis, but of broncho-pneumonia. Recently there was a well-known case of death from operation on the maxillary antrum. He asked whether members had tried placing aseptic wax over the bare bone to prevent osteomyelitis. His own opinion was that that would lead to the condition, rather than prevent it. He suggested removing the lower wall and only part of the anterior wall, leaving the wound open for a few days, and then filling up the sinus with wax, to prevent deformity. He always removed the middle turbinate, and asked the patient to come once a week, because small polypi grew quickly and prevented free drainage. In a few weeks there was generally no recurrence of the small polypi and of the swollen mucous membrane, and free drainage and cure took place without the radical operation. Patients said that menthol (10 per cent. spray) was very comforting, and that it relieved the headache, and prevented recurrence of the local pains.

Dr. DUNDAS GRANT reminded the Section that the first case brought before the Laryngological Society of London was one of chronic frontal sinus suppuration, treated by him by the Ogston-Luc method.[1] He had also from time to time brought forward cases of operation carried out by Killian's method, but from first to last he had been a very strong advocate for intranasal treatment. He was surprised that there had been no reference to the use of aspiration in the treatment of acute cases of sinusitis, as he had brought before the Society Sondermann's apparatus for effecting it.[2] It had, however, to be preceded by the application of cocaine and adrenalin, as mentioned by Mr. Stewart. Dr. Grant narrated the case of a middle-aged gentleman whose frontal pain rendered him almost maniacal. After the application of cocaine and the exercise of suction by means of Sondermann's apparatus, relief from pain and complete calmness followed. A more efficacious instrument for introducing into the nose had since been devised, but aspiration could also be practised by the patient pinching his nostrils and making a strong inspiratory effort. In another similar case, where the symptoms were extremely violent, relief was effected by the removal of the swollen anterior extremity of the middle turbinated body. In regard to chronic frontal sinus suppuration, Dr. Grant had brought several cases before the Society, and in one in which he had recommended operation and the patient refused it, cure eventually took place apparently as the result of repeated injection of mentholized liquid vaseline. In another case the relief of headache was obtained immediately when dilatation of the infundibulum by means of bougies was practised. The patient learnt the art of irrigating his own frontal sinus, and kept the discharge in check. In one case a deflection of the septum had to be removed in order to render the introduction of a catheter into the frontal sinus possible. He considered the most perfect operation to be Killian's, the whole of the anterior wall being left, and not merely the part internal to the trochlea, and that many of the cases which recovered as the result of a simple Ogston-Luc operation, were likely to have recovered without it. With regard to fatal results as the result of osteomyelitis, he had only seen two. One was a case in which he was not called in until already the pus had made its way through the anterior surface of the frontal bone: the other was the one which Dr. Dan McKenzie had narrated. Apparently every operator was sure to meet with it some time or other, but he did not think that Dr. McKenzie need be discouraged from carrying out the operation in suitable cases, as it would probably be a very long time before he would meet with such another. Dr. Grant narrated a case in which he had performed what was apparently an exceptionally successful Killian operation from the artistic point of view, but the pain still persisted, though indeed the discharge disappeared. It was temporarily relieved by resection of the supra-orbital nerve, and then again by free opening of the sphenoidal sinus; the pain centred itself in the opposite frontal region, and Dr. Grant had recently opened this other frontal sinus, finding it full of granulation tissue. He was glad to find that members of the Section were coming round to the views which he had always strongly advocated in regard to a hopeful conservatism and the efficient trial of intranasal methods of treatment; when these failed he advised a complete radical operation, and considered Killian's method the best.

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[1] *Proc. Laryng. Soc. Lond.*, 1893-94, i, p. 1.

[2] *Proc. Laryng. Soc. Lond.*, 1905-06, xiii, p. 45.

Mr. STUART-LOW said that as regards the treatment of acute frontal septic antritis, the best procedure consisted in relieving tension in the nose and sinus by reducing the œdema and swelling in the middle turbinal region by packing the nose with cotton-wool plugs soaked in adrenalin and cocaine solutions. This constituted a temporary treatment, and should be followed up with the removal of the anterior half or two-thirds of the middle turbinal, in most instances. With regard to chronic frontal septic antritis, Mr. Stuart-Low said that after a large experience of these cases treated by surgical measures, he had come to look upon thorough intranasal treatment as a preliminary step to be of the most essential importance to securing a successful result. The anterior two-thirds of the middle turbinal must first be taken away, and any swelling in the neighbourhood of the hiatus semilunaris reduced—i.e., the anterior lip or the ethmoidal bulla made of normal dimensions, if enlarged. Should these means fail to bring about efficient drainage, then the best thing to do was to perform the Ogston-Luc operation. If this were done thoroughly and efficiently, and a large-sized rubber drainage tube fixed at the lowest point of the frontal sinus and drawn down into the nose, as usually practised by Mr. Chichele Nourse, good drainage would be established. Mr. Stuart-Low had shown some of his successful cases at the meetings of this Society from time to time, and he had never seen a case in which this treatment had not been sufficient to cure the patient. He deprecated such extensive and hazardous operations as Killian's and Watson-Williams's, and did not consider that they would ever be required if thorough and intranasal, and properly performed Ogston-Luc operations were carried out.

Mr. WAGGETT stated that in his experience acute frontal sinusitis invariably cleared up under conservative treatment, an important element in which was confinement to bed for a few days. With regard to chronic empyema, in his experience the vast majority could be effectually dealt with by the intranasal methods. In certain cases the external method was undoubtedly indicated, and in such cases the Watson-Williams operation gave a remarkably free access.

Dr. KELSON thought that fatal osteomyelitis on the one hand, and deformity on the other, had combined to render external operation on the frontal sinus less popular than formerly.

Dr. H. J. DAVIS pointed out that no allusion had been made to a clinical fact, which was, that the acuter, i.e., the more recent, the sinusitis, the greater the chance of recovery, and the more chronic the sinusitis, the more difficult it was to cure. This applied to affections of the other accessory sinuses as well.

The PRESIDENT, summing up the discussion, said that the result of the discussion appeared to be to consolidate and confirm the guiding principles of treatment that he had outlined in his opening remarks, and which he believed freely represented the views of the majority of present-day rhinologists. He congratulated those members who had brought patients upon whom radical operations had been performed, on the exceedingly good results obtained both from the curative aspect and on the absence of any disfiguring cosmetic defects, and he believed that these results were unsurpassed by any other operators. He also thanked those members who had again directed attention to their fatalities from osteomyelitis. As he had already mentioned, he himself had had the good fortune to escape this dreaded complication, but he recognized that this was not due to any skill on his part, and through no controllable cause his results in future might be marred from its occurrence. But the instance mentioned by Dr. Paterson in which death was due to osteomyelitis without operative interference reminded them that patients might be exposed to the gravest risks from the disease itself, and one should not therefore unduly magnify the risks of operation. He was interested in Dr. Bronner's suggestion, and had tried packing the cavity of the sinus with an antiseptic wax after it had been cleared of all trace of mucous membrane. With regard to the examples in his own method of radical operation on the frontal sinus that were shown on the lantern screen, he wished to thank Dr. Waggett for reference to one case at which he was present, in which two enormous suppurating sinuses were dealt with, and he proposed to show the patient at the next meeting at Bristol. He reminded the members of a case recently shown by Mr. Charters Symonds, in which his method of operation had cured the patient and left no visible scarring. Dr. Lambert Lack had also adopted this operation in a case, and, he reported, with good result.

Laryngological Section.[1]

June 2, 1911.

Dr. P. Watson-Williams, President of the Section, in the Chair.

Memorial to Sir Felix Semon, K.C.V.O., M.D.

The President, on behalf of the Organizing Committee, presented a bound and engrossed address to Sir Felix Semon, containing a complete list of the subscribers to the fund for establishing a perpetual memorial of his work in Laryngology. The wording of the address was as follows:—

Dear Sir Felix Semon.—When it was understood that you intended to retire from the active exercise of your profession, we felt a desire to show our appreciation of your life's work and of your devotion to the cause of humanity.

Many of your confrères, more especially those engaged in your branch of medicine, knowing the extent to which laryngology is indebted to you, were anxious to express their recognition of the example you have set of a high professional standard, of the educational assistance you have afforded to younger men in your branch, and of the great efforts you have made and the personal influence you have exerted to promote in every direction the advancement of that speciality to which you have so successfully devoted your energies.

Numerous friends have been glad to take advantage of this opportunity of testifying to you their gratitude for the services you have rendered to them personally, and at the same time to express their feelings of esteem and affection for you as a man.

It was recognized that the most appropriate manner of commemorating your work would be some permanent memorial by which your name would be handed down to posterity. For this purpose we—whose names are inscribed below—have raised a fund with which we intend to establish a Scholarship or Lectureship in Laryngology bearing your name. We feel certain that this scheme for furthering scientific work in your branch will be to you the most acceptable form which a testimonial could take.

We trust that, in spite of your retirement from practice, your influence in furthering medical science and study will long continue to be felt, and that you will yourself witness valuable results promoted by the memorial we are establishing in your name.

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[1] Extra-Metropolitan Meeting held at Bristol.

At the same time we desire to express our esteem, regard, and affection for Lady Semon, and hope that this recognition of your great work will remain for her a happy memory throughout her whole life.

As a finishing step of this exceptional memorial of a man's life work, the book further contained the following statement:—

The members of the Organizing Committee have the gratification of recording that the wishes of the aforesaid founders have now been fulfilled by the endowment in the University of London of the Semon Lectureship in Laryngology.

(Signed)

HENRY T. BUTLIN, President of the Royal College of Surgeons;
DUNDAS GRANT, Past President of Laryngological Section, Royal Society of Medicine;
A. MOND (now Sir ALFRED MOND, M.P.) } *Hon. Treasurers;*
STCLAIR THOMSON, President-elect }
P. WATSON-WILLIAMS, President of the Laryngological Section, Royal Society of Medicine, *Hon Secretary;*
H. J. DAVIS, *Hon Secretary.*

The PRESIDENT, in handing the gift to Sir Felix, said circumstances into which he need not enter had delayed the presentation of the address until that day. In a sense he was pleased that that was so, because to him had fallen the delight of performing that little ceremony as one of the last acts of his Presidency. It was also very fitting that the presentation should take place in Bristol University, since the rules attaching to the Semon Lectureship were founded upon the ideas of the Long Fox Lectureship. He (the President) congratulated Sir Felix upon the establishment of the Lectureship bearing his name.

Sir FELIX SEMON thanked the President for the kind and graceful way in which he had handed him the gift, which it was a pleasure to him to receive because of the esteem and affection it expressed towards him. The speaker referred to the terms under which lecturers would be appointed, and said his mind had been exercised as to whether the fund should be devoted to some sort of scholarship for an educational purpose in relation to the study of affections of the throat, or whether it should be applied to the encouragement of original research by those of mature years who had had fuller experience; and he felt there was greater need for the latter. That was why he had instituted the lectureship, and why he stipulated that the lecturer should be appointed from among those who had done most for the advancement of knowledge in that special department, either by clinical or research work.

Demonstrations.

(1) *Direct Bronchoscopy on a Patient.*

By P. Watson-Williams, M.D., and A. J. M. Wright, F.R.C.S.

(2) *Œsophagoscopy of Malignant Growth and Application of Radium.*

By N. S. Finzi, M.D., and C. W. M. Hope, F.R.C.S.

Mr. Hope passed a split œsophagoscope tube of Dr. Hill's pattern down to the growth. The radium on the end of a long flexible silver rod was then passed down to the growth and slipped through the slit in the œsophagoscope tube, which was then easily withdrawn. The whole manipulation only took a few minutes, and the actual introduction of the radium only a few seconds.

(3) *The Hill-Herschell Method of Œsophago-gastroscopy on the Living Subject, with Remarks on Pathological Conditions observable by this means.*

By William Hill, M.D., and G. Herschell, M.D.

The method was described at the February meeting of the Section (see *Proceedings*, p. 69). The figures below illustrate the method (*see* figs. 1, 2, 3, p. 152).

The President congratulated Dr. Hill on the relative ease with which a view of the stomach was obtained.

Dr. Paterson said he would like to associate himself with the President's congratulations. He remembered the demonstrations of Mikulicz showing the interior of the stomach, but he thought the present method had advanced the process very materially.

(4) *Series of Foreign Bodies removed by Direct Bronchoscopy and Œsophagoscopy, and Skiagrams relating to above.*

By Herbert Tilley, F.R.C.S.

A description of these was published in the *Proceedings* (No. 8), Clinical Section, p. 152.

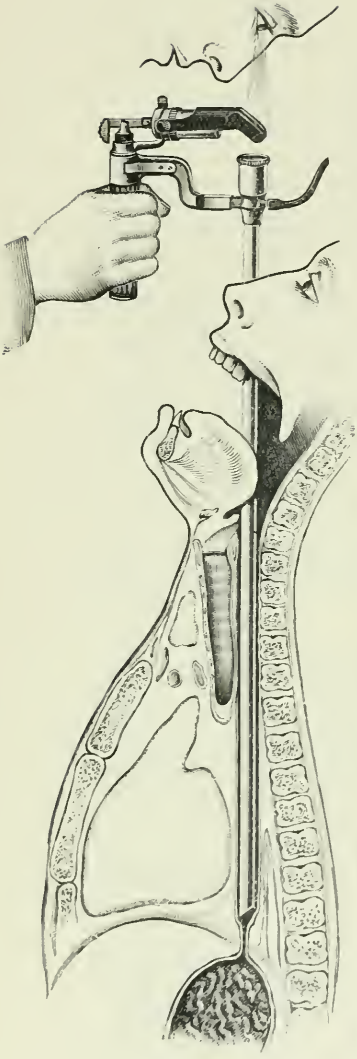


Fig. 1.

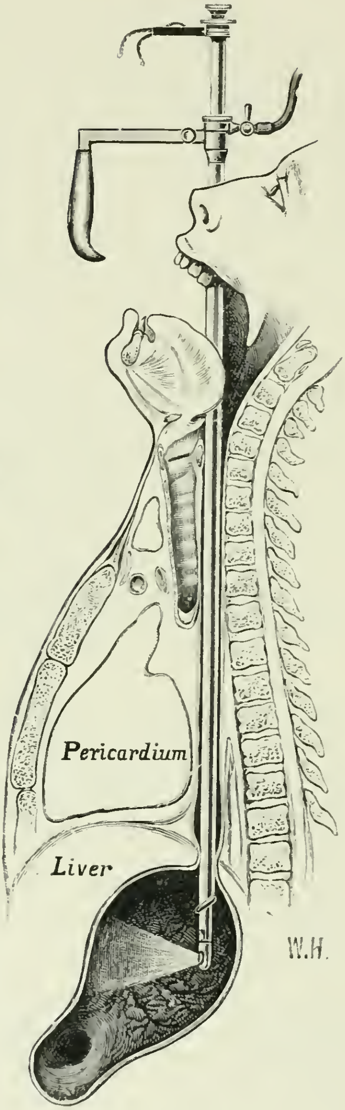


Fig. 2.

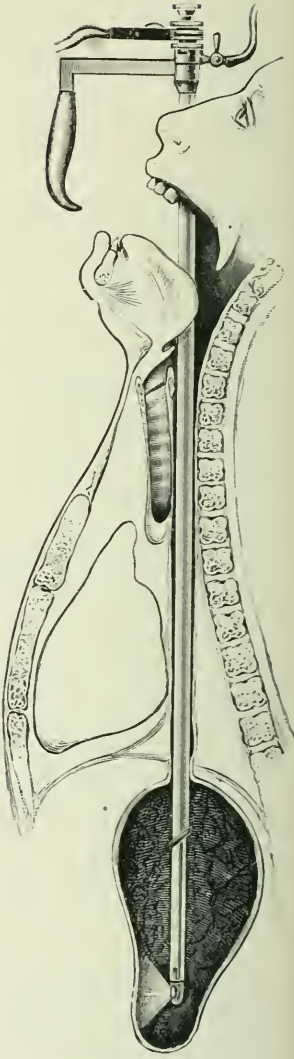


Fig. 3.

Fig. 1.—Passing Hill's direct vision inflating œsophago-gastroscope with the aid of Brünings' hand lamp.

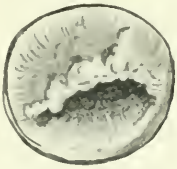
Fig. 2.—Hand lamp and window removed and periscope inserted through the outer tube into the cardiac region of the stomach.

Fig. 3.—Showing forward position of spine for inspecting the pyloric canal.

Case of Malignant Stricture of the Œsophagus.[1]

By P. Watson-Williams, M.D., Charles Hope, F.R.C.S., and N. S. Finzi, M.B.

A. P., male, aged 64, had noticed increasing difficulty in swallowing for three or four months, but could not even swallow fluids when first seen by Dr. Watson-Williams on January 10, when œsophagoscopy revealed an infiltrating growth 10½ in. from the upper incisor teeth. Dr. William Hill, in conjunction with Dr. Finzi, applied radium (150 mg. of radium salt) by means of two tubes inserted "tandem," for fourteen hours on February 4, and this so far relieved the obstruction that the patient was able to swallow meat, potatoes, and any solid food. Since then two more applications have been made by Dr. Finzi, the last time only for eight hours, as the patient drew out the radium tube too soon.



Œsophagoscopic appearance of the growth.

The growth is easily seen by œsophagoscopy (*see* figure), but the patient has been enabled to take food regularly and has greatly improved in general condition, as he was being starved before. [Section of a portion of the epitheliomatous growth shown under the microscope, and Dr. Finzi demonstrated the stricture and the method of application of radium.]

Further Notes on a Case of Epithelioma Laryngis.[2]

By P. Watson-Williams, M.D.

Miss L. T., female, aged 41. An extensive malignant growth, occupying not only the posterior two-thirds of the vocal cords, but also extending upwards and over the upper margin of the left thyroid ala so as to appear in the pyriform fossa. She was operated on on November 9, the growth being completely removed. She recovered, and was shown subsequently at the meeting in London on March 3, 1911. At that time there was a suspicious-looking granulation in the centre of the cicatrix, and this, on removal for histological examination, was reported by Professor Walker Hall to be a recurrence in an early stage. The larynx was therefore again opened, and the cicatrix freely removed, as the only alternative to a total laryngectomy. The patient has done well, and as the tissues removed at the operation were devoid of any indication of malignancy, there is reason to believe that the growth was successfully extirpated. But the size and extent of the growth at the first operation necessitated unusually free removal of the left half of the larynx, including the arytænoid and arytæno-epiglottidean fold and left margin of the epiglottis, and as a result the cicatrix is contracting so as seriously to interfere with respiration. A large intubation tube has been inserted with only temporary relief to the stenosis, and it was not well tolerated, and it is feared that a tracheotomy tube will have to be worn permanently.

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[1] Shown at the February meeting (*Proceedings*, p. 83).

[2] Previously shown at the Section's meetings, November, 1910, and March, 1911 (*Proceedings*, p. 16 and p. 93).

Patients illustrating Results of Dr. Watson-Williams's Osteoplastic Radical Frontal Sinus Operations, &c.

By P. Watson-Williams, M.D.

Case I: Radical Operation for Frontal Sinus Suppuration.—Mrs. O. Severe headaches and pain round the left eye dating from a sharp attack of influenza in 1905, from which time a copious purulent secretion persisted. The symptoms were becoming aggravated in 1906 when she first came under the exhibitor's notice. Diagnosis: Left frontal sinus and antral and bilateral sphenoidal sinus suppuration, with left nasal stenosis from septal deflection. A left radical antral operation, with removal of several polypi, and free removal of the anterior walls of the sphenoidal sinuses, afforded marked relief; subsequently lavage of left frontal sinus. In 1908 radical operation was performed on the frontal sinus by Watson-Williams's osteoplastic method: a very large left sinus was found containing pus and polypi; it extended outwards almost to the external orbital angle, and upwards of $1\frac{1}{4}$ in. above the supra-orbital margin, and was 2 in. in depth. The orbital roof and nasal wall were removed. The conformation of the vertical plate of the middle turbinal made the approach to the ethmoidal cells peculiarly narrow and difficult, but the free exposure of the osteoplastic method of splitting the nose rendered it easier to reach the deeper parts than any other method of exposure could have done. The sinus cavity was drained for forty-eight hours by a rubber tube, and the wound irrigated through the brow for several days. Healing complete, with entire cessation of all headache and discharge, and with no cosmetic defect other than some slight depression above the brow inevitable with the obliteration of an enormous sinus (*see* fig. 3).

Case II.—Miss S., aged 47. In 1907 came with symptoms of nasal discharge of ten years' duration, with severe frontal and occipital headaches, inability to do mental work properly, and with increasingly poor health. In 1908 a double radical Killian frontal sinus operation performed. In 1909, both sphenoidal sinuses freely opened and found to contain pus and polypoid granulations. The sphenoidal sinuses were also found to be partially subdivided by a horizontal septum into an upper and lower portion. In 1911 some recurrent suppuration led to the opening of the frontal sinus area, but the sinuses were found to be obliterated, the suppuration coming from some anterior ethmoidal cells.

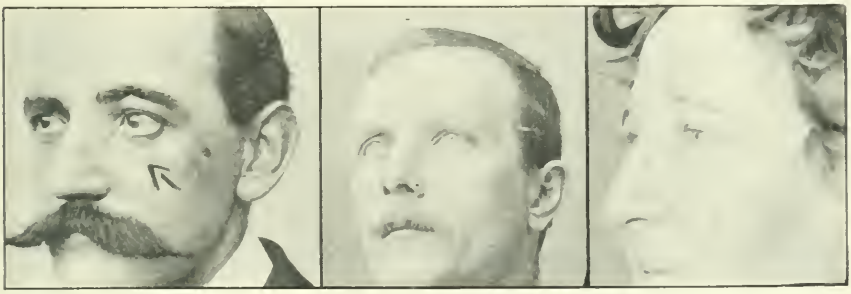

Fig. 1. Fig. 2. Fig. 3.

Three of the patients exhibited on whom Watson-Williams's osteoplastic radical operation had been performed. Fig. 1 is Case III, double frontal operation; fig. 2, Case IV, and fig. 3 is Case I described in text. Reproduced from untouched negatives.

Case III.—W. B., aged 21. In 1908 both frontal sinuses suppurating; severe headaches. Lavage of both sinuses daily for some weeks. Finally the exhibitor's double osteoplastic radical operation performed, with free removal of the anterior ethmoidal cells and the sphenoidal sinuses opened freely. Cured; he uses two or three handkerchiefs a week instead of two or three a day (*see* fig. 1).

Case IV.—S. B. Suffering from severe frontal headaches and nasal discharge for many years; had previously had an external operation on the frontal sinus by another operator, but it had left an external discharging fistula. February, 1910, the osteoplastic operation performed, Mr. Waggett and Dr. Cathcart present. The sinuses communicated and were enormous, measuring 12 cm. laterally, and reaching 7½ cm. above the supra-orbital margin. He was able to get up in the ward four days after operation. He is now free from headaches, and says he feels cured (*see* fig. 2).

Case V.—Miss C., aged 36. For two years left-sided nasal discharge and severe frontal headaches. Diagnosis: Left pansinusitis. Left osteoplastic frontal sinus operation, opening up and removal of ethmoidal cells, and radical antral operation. This was followed by angina Ludovici and cellulitis of the supra-orbital tissues, the supra-orbital wound being kept open and irrigated. She completely recovered, but in 1910 the other frontal sinus suppurated, and was operated on. Is very much improved, but occasionally gets headaches.

Case VI.—F. S., aged 34. In 1908 severe headaches and nasal discharge; two previous attacks. The osteoplastic operation performed on the left frontal sinus, the left antrum and both sphenoidal sinuses also opened freely. Now never notices any nasal discharge, and he feels well. There is a slight depression on one side of the nose, owing to a chisel being used instead of a saw in splitting the nose, a small splinter of the nasal bone coming away. Cured.

Case VII.—Miss H., aged 34, first seen in March, 1908, with history of headaches and nasal discharge for "years." Double radical antral operation performed by the canine route, and in June, 1908, the osteoplastic frontal operation on the right side. In May, 1909, both sphenoidal sinuses were opened up. There is now no nasal discharge, but the headaches are unrelieved, but apparently are not due to the nasal condition.

Case VIII.—Miss G., aged 17. Came in April, 1910, with a history of ten years' nasal discharge and severe headaches. In May, 1910, double radical antral operation performed by the canine route, and in July of the same year the exhibitor's osteoplastic operation was per formed on the left side. In February last acute symptoms developed on the right side, and a radical Killian operation was performed. Two months later swelling recurred on the right side, and the "bridge," which was found to have necrosed, was removed. The symptoms are now relieved. It is difficult to see that any external operation has been performed on the left side; there is no obvious scar or depression.

DISCUSSION.

Mr. HERBERT TILLEY said he often wished he could see the Watson-Williams operation performed and follow the details. In comparing the cosmetic results of the operation with others, he thought the result did not depend so much on the operation as on the depth of the sinus, and this could be previously determined by skiagraphy. He remembered a case where the sinus required 11 ft. of cyanized gauze 1 in. in width, and in which he considered the sinus to be the largest he had ever seen. He considered that in Killian's operation the operator got an excellent view of the sinus, and he did not think an operator could wish for a better one. If the operation was a more complicated one without giving a better view it was a serious disadvantage.

Dr. CATHCART said with regard to the merits of Dr. Watson-Williams's operation and Killian's, he had seen both performed—in Killian's own clinic and also by Dr. Watson-Williams in Bristol. The osteoplastic operation of the latter was not so complicated in practice as it seemed on paper, and it gave a far better view of the sinus than any other he knew of. Every part was exposed, and the operator was able to chip away without any fear of approaching any of the dangerous parts. He considered the cosmetic results in a large number of cases far better than were obtained with other operations, although it took a little longer to perform. The case that he had seen Dr. Watson-Williams operate on, and which was shown at the meeting to have no deforming scar, had the largest frontal sinus he had ever seen.

The PRESIDENT said that he did not claim in all cases better results by his operation. With a large deep sinus any operation which aimed at obliteration of the sinus was liable to cause some depression above the brow. For his operation he claimed that it gave the operator a more direct approach to the sinus and a better view of the parts, and there was no risk of a narrow bridge becoming absorbed as in Killian's operation. He was very glad to hear Dr. Cathcart's appreciation of the operation that he had witnessed. His (Watson-Williams's) method took a little longer in exposing the sinus and fronto-nasal passage, but the latter part of the operation was then done more quickly and with greater ease than when one had to work under a bridge as in Killian's operation. However, he had himself obtained excellent results with Killian's method, and had shown some patients at the meeting on whom he had done this operation.

Pemphigus of Pharynx and Conjunctiva.

By P. WATSON-WILLIAMS, M.D.

R. G., AGED 58. His eyes have been inflamed for six weeks, and his throat and mouth sore for about one month. There are white membranous patches scattered over the palate, pharynx and inner sides of the cheeks and under surface of the tongue. The surrounding mucous membrane is swollen and intensely red. When first seen there was a membranous condition of the conjunctiva, but this has partially cleared up with applications of 10 per cent. argyrol. Two days ago the patient attended with bullæ on the chest; this being the first evidence of skin involvement.

Bacteriological Reports by Professor Walker Hall.—May 23, 1911: Swab from conjunctiva gave a growth of diphtheroid bacilli of a suspicious type. May 29, 1911: Eye—Gram-positive diplococci and staphylococci only. Throat—Gram-positive streptobacilli and cocci only.

The patient is now taking arsenic.

Chronic Glossitis. ? Cause.

By P. WATSON-WILLIAMS, M.D.

MISS C., aged 16. Tongue has been sore and fissured for five or six years, but much more so during the last year. Some pain on swallowing, and occasional hoarseness. There is a history of discharge from the left side of the nose, but no evidence of accessory sinus suppuration. The tongue is extremely fissured with atrophy of papillæ in some parts, and hypertrophy in others. No actual ulceration. No history or evidence of syphilis.

Tertiary Specific Ulcer of Pharynx.

By P. WATSON-WILLIAMS, M.D.

G. M., AGED 10. History of syphilis in both parents, and of a rash and "snuffles" in infant shortly after birth. Treated for keratitis one year ago. Now has general glandular enlargement with a painless deep sloughing ulcer on the right lateral pharyngeal wall.

Cystic Middle Turbinal (Right).

By P. Watson-Williams, M.D.

Miss S., aged 39. Formerly suffered from "colds in the head" and has had nasal obstruction for many months. There is a large air-containing bone cyst in the anterior half of the right middle turbinal.

Results of Orbital Cellulitis following Ethmoidal Operation.

By A. J. M. Wright, F.R.C.S.

Miss S., aged 33. First attended in 1906 with history of headaches and nasal discharge for ten years. In 1906 left antrum and sphenoidal sinus were operated on. In January last the ethmoidal cells were opened on both sides. An orbital hæmatoma resulted on the left side, suppurated, and was opened. There is now some ptosis and entropion of the lower lid, owing to contraction of the scar and injury to the elevator of the upper lid, but the condition is improving under massage.

Paralysis of Right Third Nerve following Ethmoidal Operation.

By A. J. M. Wright, F.R.C.S.

R. W., aged 36. First seen in April, 1911, with history of nasal polypi for about eight years. Following an intranasal ethmoidal operation on the right side he developed an orbital hæmatoma and complete right third nerve paralysis. The pupil is now, six weeks after the operation, smaller and reacting, and the ptosis is less, but the condition is otherwise unaltered. Vision of the right eye is unaffected.

Œdema of Conjunctiva from Acute Frontal Sinusitis.

By A. J. M. Wright, F.R.C.S.

A. W. First seen on May 23 last with severe left frontal headache, and œdema of inner portion of left conjunctiva, lids and orbit, and with a conjunctival hæmorrhage. There was a history of a similar attack fifteen years ago. There is pain on extreme deviation of the eyes to the left, with slight diplopia. The septum was deviated to the left, blocking that side. No nasal discharge. The septum was resected on May 26, with relief of the headache, and the swelling is subsiding. There has been a little left nasal discharge since the septal operation.

Tumour of the Cheek.

By C. F. Walters, F.R.C.S., and N. S. Finzi, M.B.

Mrs. X., aged 47. Swelling inside cheek from the age of 7, which was of a soft, pedunculated character and painless. This became painful in May, 1910, and got larger a month or two afterwards. The growth continued to increase and was removed with the adjoining mucous membrane in November, 1910. All seemed satisfactory until December 19, 1910, when there was a sudden rapid swelling in the cheek. This increased in size and was incised a few days later, but continued to grow rapidly.

On January 3, 1911, there was an enormous swelling in the cheek about the size of a Jaffa orange, slightly movable, but with considerable œdema round it. No definite glands felt, but the œdema might have obscured any there were. The anterior part of the swelling fluctuates and appears on the point of breaking externally.

Treatment: January 5, 1911.—Radium, 105 mg., used through a filter of $1\frac{1}{2}$ mm. of platinum or lead in two tubes inside tumour sixty-nine hours, also outside skin for twelve hours in each of two places.

February 2: The tumour had very much shrunk, but there was a pre-auricular gland to be felt. This was excised, and the radium inserted into the wound, 100 mg., nine hours, February 23 and May 11. Subsequent treatments, externally and internally, with 205 mg.

Histologically the tumour was a mixed one, probably of the nature of an endothelioma or a glandular carcinoma.

The Relationship of Diseases of the Nose and Accessory Sinuses to Affections of the Eye and the Orbit.

An Address Introductory to a Discussion on the Subject.

By StClair Thomson, M.D.

I deeply appreciate the honour conferred upon me by the Council of the Section of Laryngology in inviting me to open this discussion. I will try to acknowledge the obligation by being both practical and brief. But I would crave some indulgence, for two reasons: If I speak much of the anatomy and pathology of the nose to a meeting of laryngologists, I may be classed with those who attempt to instruct their grandmothers how to extract the nourishment from the egg of the barndoor fowl; and if, in a gathering where one sees many distinguished practitioners and ophthalmologists, I venture to say much of diseases of the eye, I shall most certainly display an ignorance which may make the faithful grieve, and cause the finger of scorn to be pointed at the specialist who views diseases only through a nasal speculum. However, I will try to avoid these pitfalls by adhering closely to our text; by remembering my own conviction that the opener of a debate should be simply the pioneer who pegs out the ground, and not the miner who works it; and by recalling the repeated but friendly advice of our President, "Don't exceed fifteen minutes"!

When we glance at the illustration, which will shortly be thrown on the screen, you may all think, with Horatio, that it needs no ghost come from the grave to teach us that diseases of the eye and orbit must be affected by their proximity to the nose and the accessory cavities. A perfervid orator once exclaimed that England was an island almost entirely surrounded by water! He may have inserted the word "almost" to avoid referring to Scotland, the predominant partner who sits at, if not on, the head of England. Well, the enthusiastic rhinologist may say more truly that the orbit and its contents are almost entirely surrounded by the air-spaces of the nose, and must necessarily be as much influenced by the currents of the latter, as England is by its seaboard.

Yet many of our recent text-books of ophthalmology are strangely silent on this relationship. They still talk of orbital cellulitis as a pathological entity, coming on "spontaneously"—whatever that means—and of its being followed by periostitis, abscesses, caries, and necrosis. Now the experience of the rhinologist is, that it is not the cellulitis which results in periostitis, abscess, and so forth, but that in the large majority of cases it is a septic process in these neighbouring nasal cavities which is the primary lesion, frequently leading to suppuration, necrosis, and orbital cellulitis.

Chronic periostitis is still in many text-books on eye diseases attributed to syphilis, rheumatism, or scrofula, instead of the much more frequent cause of nasal suppuration. As for thrombosis of the cavernous sinus, with its characteristic orbital manifestations, we still read in ophthalmic text-books that it is generally due to some injury or traceable to some remote lesion in the lips, neck, teeth, or throat. These certainly may occur, but my own researches indicate that suppuration in the nose and its annexes, or in the ear and mastoid, is a much more constant source of infection. That the orbit may be invaded by distension (from any cause) in the neighbouring accessory cavities is much more generally recognized, and that affections of the tear-duct, the lachrymal apparatus and the conjunctivæ are frequently caused by diseases of the nose is well known.

The relationship of diseases of the optic nerve and the eye itself to rhinology has, in recent times, attracted a great deal of attention. Indeed, the literature on the subject is almost unwieldy, and such a gathering as the present appears to me particularly opportune for epitomizing the association of diseases of the nose and eye, and indicating how, why, when, and where this association may take place.

The anatomical relations will be glanced at presently, when we darken the room for the magic-lantern. As to the routes by which disease can extend from the nasal to the ophthalmic region we will have to-day to consider—

(*a*) Direct extension, as when a frontal mucocele bulges into the orbit, and a malignant nasal tumour invades it, or a suppurating ethmoiditis bursts into it.

(*b*) Obstruction and infection of the lachrymal passage, with consequent disturbances in the conjunctivæ.

(*c*) Infection of the lymph channels, causing congestion, discoloration, or redness of the eyelids, or many of the functional disturbances of vision.

(*d*) Infection of the blood-stream, causing thrombosis of the ophthalmic vein or the cavernous sinus, or hæmorrhagic retinitis, and so forth.

And, lastly (*e*) reflex effects.

[Illustrations were then thrown on the screen to indicate some of the anatomical relations of the nose and its accessory sinuses to the orbit, the eyeball and the optic nerve.]

DISCUSSION.

Mr. F. Richardson Cross said the importance of the relationship of disease of the eye to affections of the nose had been thoroughly recognized for a long time. He said he began his work as a general surgeon, and had operated upon many cases of frontal sinus mischief; and realizing the importance of the connexion between the work of the nasal surgeon and the ophthalmologist, he had persuaded the Committee of the Eye Hospital at Bristol to have a nasal surgeon associated with the Staff. Every practical ophthalmic surgeon would acknowledge the help that could be obtained from the rhinologist in dealing with sinus cases. Some cases of eye disease started in the nose, but his experience was that they were not very common; it was generally fairly obvious where the main centre of the disease lay, whether it was primarily a nose or an eye condition—though, no doubt, some were on the borderland, and the best line of treatment might be uncertain. All forms of sinusitis, particularly in frontal and anterior ethmoidal disease, might implicate the orbit; while inflammation of the sphenoidal or posterior ethmoidal cells might be expected to involve the optic nerve at the apex of the orbit or in the optic chiasma. But evidence of implication of the nerves of sight was usually absent—definite abnormity of the optic disk was rare, because of the distance of the disease behind the eyeball; though some haze or pallor of the papilla might be present, especially if there was extension into the orbital cavity. There might be complete absence of defective movement, or of tenderness in the eyeball. The most valuable test in these cases was by the perimeter; diminution in the temporal field, chiefly in its upper quadrant, suggested pressure on the chiasma, or intracranial portion of the optic nerve, as they lay in the sphenoid bone—while implication of the nerve at the foramen, or in its orbital portion, might cause peripheral narrowing of the field, or more probably a central defect, because the very delicate axial fibres were those most readily affected by the pressure. Mr. Cross illustrated his remarks by lantern slides showing charts of the visual fields in different cases.

Dr. TAYLOR showed slides illustrating a case of acute frontal sinus suppuration. It was originally admitted to hospital as a case of cerebral tumour. Three weeks after operation on the sinus there was great improvement, and the patient was quite well in six weeks.

The PRESIDENT and Mr. A. J. M. WRIGHT showed some slides. Mr. Wright remarked that he considered there was always a certain amount of risk to the orbit in ethmoidal operations.

Dr. MATHESON MACKAY said that after a certain amount of work, both on the eye and nose, his experience had been in accordance with that of Mr. Cross—that these cases did not occur very commonly, and were more frequently reflex than suppurating cases. Pressure in the nose might be associated with eye symptoms. In a case where Lack's operation had been performed for polypi there was excessive bleeding, and after the patient had been put to bed she complained of defect in vision, and optic atrophy was discovered: eye symptoms had not been looked for previously. He thought that the nasopharynx should be included in the examination, and also the influence of adenoid vegetations upon the eye. These he had found to be associated with follicular enlargement in the conjunctiva, and their removal had cured the symptoms.

Dr. G. F. C. WALLIS made the following remarks: In accessory sinus affections Berger and others have recorded abnormalities in the periphery of the visual fields, whilst Birch-Hirschfeld lays stress upon the presence of central scotoma. Henrici, Haffner, and Hinkel found nothing abnormal. During the past two years I have examined some forty-five cases of sinusitis, and, excepting four, all showed very definite affections. Whereas in a smaller number of cases of simple and atrophic rhinitis and deflected septa, when uncomplicated by sinusitis, no defect occurred. The large majority of these observations were made in chronic, and a few only in acute, cases. Speaking generally, two sets of symptoms are observed in chronic sinusitis; the more usual being peripheral field contraction without other ocular signs, the other variety presenting a similar train of symptoms to those occurring in acute suppuration, peripheral contraction, central scotoma, and optic neuritis.

In sixteen cases of sphenoidal and posterior ethmoidal sinusitis, eleven being bilateral, general peripheral field contraction occurred in all. In addition ten cases showed marked temporal contraction, of which seven were bitemporal and one a bitemporal hemianopsia. Of three bilateral frontal suppurations, two showed marked general peripheral contraction; the other, in which the sinus was very large, some bitemporal contraction. In ten antral suppurations, eight showed general peripheral contraction; one, a patient aged 14, had normal fields, and in the other slight temporal contraction was alone present. In twelve cases of polysinusitis, where most of the sinuses were involved on one or both sides, all showed either temporal or general contraction. Nasal contraction was not observed, but altitudinal contraction, either alone or in addition to other, was present in ten cases—three in sphenoidal and seven in polysinusitis. In these cases the field for white has been least affected, then that for red, and green most of all. I now always use white and green for test objects.

Central scotoma was observed five times, all occurring in the posterior group, of which three were acute and two were chronic suppurations. In three of these optic neuritis was present. An island field of vision developing into a ring scotoma occurred in a case of chronic sphenoidal sinusitis which Mr. Russ Wood and I recorded in the *Lancet*.[1]

Normal fields were found in the following conditions: Frontal and ethmoidal mucoceles, deflected septa, simple and atrophic rhinitis. In two cases of atrophic rhinitis associated with sphenoidal sinusitis temporal and general contraction occurred. In a case of atrophic rhinitis which appeared to be due to sphenoidal sinusitis, in spite of normal fields, I opened the sinus and found it healthy. This, together with the characters of the field defects, and only partial recovery with successful treatment of the chronic sinusitis, conclusively proves, in my opinion, that field anomalies are not reflex in origin. The cause is, I think, a mild local toxæmia. In chronic sinusitis, without obvious ocular symptoms, the toxin soaks slowly through the sinus wall and directly affects the optic nerve, with the result that the nasal part of the nerve being in closest relation to the spheno-ethmoid sinus is the most affected, and temporal contraction results. In the fronto-antro-ethmoid group the nerve is not in direct contact with any sinus wall, and the toxin becoming diffused, the circumferential fibres being equally affected, general contraction results. Central scotoma does not result in these cases, the macula bundle, being axial in position, is protected by the circumferential fibres.

In acute suppuration the toxins are carried by the vessels, larger quantities reach the nerve, causing œdema of the nerve-sheath, with consequent nerve pressure, and the field becomes contracted; in addition the more severe cases show central scotoma. In chronic suppurations with central scotoma and other acute ocular symptoms the passage of toxins is probably favoured by some anatomical variation or incomplete bony wall. For the production of bitemporal hemianopsia the optic chiasma must be involved; it is, however, uncommon, because, as pointed out by Lawrence, the optic chiasma is rarely in relation to the roof of the sphenoidal sinus. Mucoceles being sterile, are presumably free from toxins, hence the normal fields. If this view be correct, then the presence of central scotoma as observed by some authors, and of peripheral contractions by others, is explained.

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[1] *Lancet*, 1907, ii, p. 937.

To assign definite field contractions to individual sinuses is difficult owing to the frequency of polysinusitis and the varying anatomical relations. Suppuration in the posterior group gives rise to marked temporal contraction most frequently, whilst bitemporal hemianopsia, after excluding acromegaly, is diagnostic; central scotoma only occurs in this group, the result of either acute or chronic sinusitis when acute ocular symptoms supervene. General contraction, as the chief characteristic, is the rule in the anterior group, and pronounced temporal contraction the exception. When the symptoms are acute, efficient treatment of the sinus leads to rapid and complete recovery of the field anomalies. The field defects only show slight improvement, as a rule, after successful treatment of a chronic sinusitis, because the nerve has been permanently, although slightly, damaged. As optic atrophy has only resulted in two cases of this series the toxin must be of a mild character.

The presence of field contractions in a suspected sinusitis is, I think, of some help in confirmation of the diagnosis, but it is open to some fallacies. If the colour perception be weak, the colour field will be considerably contracted even with healthy sinuses; in young people neurasthenia and hysterical manifestations are apt to develop in chronic sinusitis, which cause modifications of the fields.

The PRESIDENT expressed the indebtedness of the Section to Dr. Thomson and Mr. Cross for their kindness in opening the discussion. Many interesting points had been brought out. His experience led him to the conclusion that, speaking generally, temporal contraction of the visual fields was usually due to sphenoidal sinus and posterior ethmoidal cell disease, while general contraction was more commonly associated with frontal sinus and maxillary antral suppurations. They had learnt a great deal with regard to the diagnostic significance of these signs, but he considered there still remained a good deal of work to be done on the subject before they could be on a sure basis, and views must frequently be exchanged between the rhinologist and the eye surgeon.

Dr. DUNDAS GRANT wrote as follows: Since Dr. Christian Holmes, of Cincinnati, read his famous article on this subject, with the well-known transverse vertical section of the head showing the relation of the optic and ocular nerves to the sphenoidal sinuses, there has been no lack of contributions to the illustrative pathological relations existing between the eyes and the accessory sinuses of the nose. In many instances the influence of minor affections of the nose upon the refractive and secretory mechanisms of the eye has been considerably exaggerated, but the importance of the association is unquestionable, and there is a growing tendency on the part of oculists to call the rhinologist into counsel in many instances when the purely ophthalmic methods of treatment show themselves less rapidly efficacious than usual.

I remember the case of an elderly woman sent to me at the Central London Throat and Ear Hospital on account of intractability of her recurrent iritis, which rapidly answered to treatment after the removal of the anterior extremity of a hypertrophied middle turbinal. No doubt the pressure exercised by the swelling interfered with the venous and lymphatic circulation of the parts. Many of those present may recollect two cases I brought before this Section in 1909,[1] in which retrobulbar neuritis, which was apparently not subsiding, appeared to be rapidly and beneficially affected by the removal of the hypertrophied posterior portions of the middle turbinals: this had been done for the purpose of affording access to the sphenoidal and posterior ethmoidal cells for exploration and treatment. In the same year I showed, with Dr. Dan McKenzie,[2] a case in which curetting the ethmoid had been followed by a retrobulbar neuritis interfering with the vision of the left eye. In this case I opened the left sphenoidal sinus according to Hajek's method, which includes opening the corresponding posterior ethmoidal cells. The operation was followed by rapid improvement in the vision of the eye, which ultimately became normal. The curetting had probably resulted in weakening or breaking down the barrier of bone between the posterior ethmoidal cells and the optic nerve, and the injurious effect was counteracted by the free opening the subsequent operation afforded.

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[1] *Proceedings*, 1909, ii, p. 121.

[2] *Proceedings*, 1909, ii, p. 148.

In the discussion on my cases Dr. Hawthorne raised the very interesting question as to why disease of the ethmoidal cells should affect the retrobulbar portion of the nerve and should exercise a selective action so as to confine its attack to the papillo-macular bundle. Professor Fuchs describes this behaviour of the nerve-fibres as paradoxical; he explains it by a peculiar vulnerability connected with the specially exquisite and delicate functions of this bundle, with which, perhaps, there is associated a correspondingly delicate anatomical structure. Professor Fuchs advises in regard to the treatment of retrobulbar neuritis, "above all the consideration of the causal factor (the nose)."

Another interesting problem, brought forward by Mr. Westmacott, was the explanation of the disappearance of an optic neuritis on one side after surgical opening of the suppurative antrum on the other. I suggested it as possible that the diseased antrum was in pathological continuity with the posterior ethmoidal cells, and that these cells, as often observed by Onodi, extended over the sphenoid to the opposite optic canal, the barrier being in such cases exceptionally thin and, therefore, exceptionally favourable to extension of disease.

The interference with the nasal duct by disease of the nose seems extremely natural, and it may lead to suppuration in the lachrymal sac, just as conversely a dacryocystitis may penetrate into the nose and produce symptoms resembling those of nasal sinus suppuration. I have seen several cases of epiphora markedly benefited by even very mild treatment of the inferior turbinal and inferior meatus of the nose. In some cases more active measures have been required.

Intranasal operation may at times be the cause of lachrymal trouble, and I have narrated before the Section a case in which the use of an exceptionally sharp rectangular knife for opening the antrum through the nose led to traumatism of the nasal duct, which produced epiphora of about a week's duration. I have recently had under observation a case in which this accident took place when a similar method was being practised by another surgeon, and in which extreme narrowing of the lower orifice of the duct seems to have resulted. Fortunately the nasal duct lies very far forward in relation to the antrum, and it is very rarely injured.

The grosser forms of extension of disease from the one part to the other have probably received adequate attention, and I have referred only to the less obvious instances which have come under my own observation. The reconsideration of the whole subject was well worthy of the Section, and no one was calculated to do it more justice than Dr. StClair Thomson.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FOURTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE
SESSION 1910-11

MEDICAL SECTION

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1911

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

Medical Section.

October 25, 1910.

Dr. J. MITCHELL BRUCE, President of the Section, in the Chair.

Multiple Cartilaginous Exostoses.

By A. M. GOSSAGE, M.D., and E. ROCK CARLING, F.R.C.S.

THERE is a class of exostosis which differs from all others in that the tumours appear spontaneously in connexion with cartilage and are multiple. At first cartilaginous, the tumours only become ossified later, and throughout their period of growth are covered by a layer of hyaline cartilage. They may be attached to any bone that is developed in cartilage, but are naturally never found connected with the membranous bones, such as those of the skull. In the long bones the growths take their origin from the epiphysial cartilages, though later in life, with skeletal growth, they may become detached from them and appear to arise from the diaphyses. When growing from the epiphysial cartilages they have the same structure as the adjacent long bone, having a compact outer covering which encloses cancellous bone and a central canal which communicates with the central canal of the bone. The tumours may be very numerous or few: Chiari records over a thousand in his patient, and one of ours had several score. In size they may be as large as the fœtal head, or so small as to be undiscoverable until the bone has been macerated after death. In shape they are very variable, sometimes forming a sharp spine, in other cases being hook-shaped, rounded, flattened, or irregularly tuberous. They may also lie in a depression of the parent bone, and so be larger than would be expected from external examination. They may be present before birth (Drescher), but usually first appear in early childhood and increase in size and number up to the end of the period of skeletal growth, when the epiphyses all become attached to the diaphyses about the twenty-fifth year. After this the size may diminish, and in certain cases some have even been said to have disappeared. Hartmann's photographs of a case before and after an interval of twenty years proves the possibility of retrogression. In rare cases the first appearance has been recorded as occurring at the age of 50.

As a rule, they do not cause the patient any pain or inconvenience, but sometimes pain may be produced by pressure on adjacent structures; exostoses of the feet may cause difficulty in walking and those on the pelvic bones may interfere with parturition (Drescher). It is the increasing deformity that usually leads the patient to consult a medical man. It is probable, though not perfectly certain, that the deformities are not merely a mechanical result of the pressure of the bony mass—of which the growth direction is always unilateral—but are also the expression of irregular ossification throughout the intermediary cartilage. Sometimes dislocations of bones occur. This is seen particularly at the elbow, the head of the radius being displaced backwards. Other surgical conditions arising in connexion with these outgrowths are due to pressure upon the bulb or spinal cord or peripheral nerves, and upon vessels with resulting erosion or aneurysmal formation. The growths are subject also to spontaneous necrosis.

Although the short and flat bones are affected, it is the long bones that suffer more particularly, and of these the upper end of the humerus, the distal end of the radius, and the articular ends of the femur, tibia, and fibula are most frequently concerned. Occurring usually near the epiphysial ends, the projection may sometimes be covered with synovial membrane and may even become detached and appear as a loose body in the adjacent joint.

As a general rule, the exostoses are best left alone, but where intervention is demanded by extraneous symptoms or lesions, removal is easily effected by the chisel; the periosteum and the covering cap of cartilage, where present, should be carefully excised. Difficulty may possibly arise in reaching the base of the tumour for anatomical reasons, but with the help afforded by radiography this can usually be surmounted. The compact tissue of the exostoses in some of the cases that have been operated on has been found to pass through the compact covering of the bones to which they were attached, tapering towards the medulla: hence the growth has often to be scooped out of the parent bone.

Various hypotheses have been advanced as to the ætiology of these growths; from isolated cases they have been regarded as consequences of previous rheumatism, syphilis, or rickets, but the evidence in favour of any of these views is of the flimsiest character. Virchow rather favoured the idea of an intimate relation to rickets, because in that condition he was familiar with "islets" of cartilage; but, as Pels-Leusden points out in his exhaustive study of the subject, these are always in the middle of the bone and would more properly be advanced as an explanation of enchondromata. Of course, rickets may occur in a patient afflicted with multiple exostoses, as in Young's case; but this is no argument that it is a causative agent. The greater incidence on males—calculated by Reinecke as thrice that in females—is a strong argument against regarding rickets as the cause. Similarly, there is no real evidence that congenital syphilis is a cause; yet it appears certain that the abnormality is initiated in intra-uterine life, and that the abnormal ossification of the intermediary cartilage is not limited to the plane of the exostoses. The frequent association of disturbance in symmetry of growth in the affected bone, generally in the direction of arrest, with consequent shortening, but sometimes as a prolonged or excessive activity resulting in undue length, and occasionally as a local or general excess or diminution in total bulk of a bone, indicates that it is not merely a question of accidental "deviation" of undisciplined offspring by the normal mother cartilage. If this were the case there would have been justification for the use of these tumours as examples of Cohnheim's "rest" theory of neoplasms. There are undoubtedly some relationships between cartilaginous exostoses and multiple enchondromata—e.g., Weber and Läwen found them conjoined—and of both with the peculiar disturbance of growth described by Ollier and known as dyschondroplasia. In this condition, however, only one side of the body is, as a rule, affected. Karewski did describe a case of multiple cartilaginous exostoses of the skull and face in association with facial hemiatrophy, and Kohler noted them in a case of partial gigantism. Again, they have been found sometimes (Reubsaël, Kryger) with myositis ossificans progressiva, and, where few in number, confusion of diagnosis with other conditions may arise. For example, a good deal has been written lately about exostoses or osteomata of the os calcis as a cause of painful heel. Some have been instances of true osteomata, but some certainly have been the outcome of inflammatory processes, such as gonorrhœal effusion into the long plantar and other ligaments.

From quite early times it had been noticed, especially in England, that the condition was hereditary, and we have collected records of sixty-seven families in which more than one member has been attacked. It seems clear that some inherent abnormality of the intermediate cartilage, which causes it to develop in this strange fashion, is the chief factor in the production of the tumours, and that other circumstances and diseases only influence their development to a minor extent. This abnormal constitution of the cartilage forms a definite entity, which is frequently handed down from parent to child. It must be acknowledged that in a large number of examples of this condition no history of the occurrence of similar tumours in the sufferer's relatives can be obtained, and one is obliged to consider the possibility of the abnormality arising *de novo* or of passing through several generations without manifesting itself. It should be specially noted that in some cases the tumours are few and small, and it is possible that they may not attract the notice of the bearer or his relatives, and so an abnormal individual may be reckoned in the family genealogy as a normal one. For instance, the mother of the B. family, from the Westminster Hospital, stated that her tumours had quite disappeared; but on careful examination six exostoses were found—five small ones up to the size of a pea, and a larger one as big as a walnut—and in Roberg's family one woman had only one small spinous exostosis on the lower end of the right radius.

Males are decidedly more frequently attacked than females; in the 67 families we have collected there were 199 affected males and only 89 females—i.e., a proportion of more than 2 to 1. In Tillmann's family the sex is not mentioned. It is of interest that in multiple exostoses there are examples of a skipped generation, and in nine such instances the condition was transmitted through unaffected females. In the literature we have found recorded two examples of transmission through unaffected males—(Roberg and Ziegler)—but in both instances the truth of this transmission is very doubtful, whereas the transmission through unaffected females seems quite clear. Stanley's family is very interesting in this connexion: a normal woman had a child with multiple exostoses by each of two separate normal husbands (*see* fig. 3).

An affected individual does not transmit the tendency to all of his progeny, but a certain proportion of them are quite normal, and the children of these normal individuals are normal in the majority of cases and do not hand on the family tendency to bony tumours. Unfortunately, the family histories are usually incomplete, but this point comes out in many of the genealogical tables—e.g., in the B. family here recorded. In the 67 families there were 293 affected individuals, and 139 unaffected; but the excess of the abnormal is evidently due to the incompleteness of the records, many authors omitting to mention the normal members of the family. In 35 families the records are fairly complete, especially in the later generations. Taking these, and omitting the earlier generations, we find 133 affected (83 male, 45 female, and 5 doubtful) and 135 unaffected, a close approximation to equality. In these more complete records the excess of males is not so great, and the proportions of the sexes are, we think, nearer to the truth. Males were found in the 67 families to transmit the condition to their children in 72 instances, whereas in 31 the condition was transmitted through females. If, however, it is correct to say that the females can sometimes

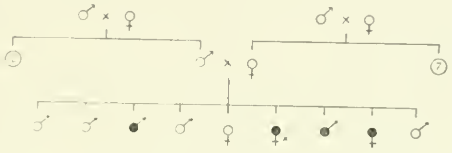

FIG. 1.

H. family. Multiple exostoses.

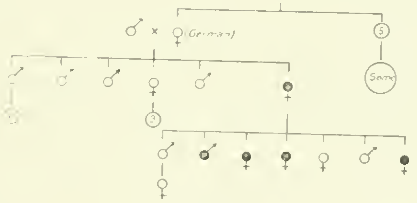

FIG. 2.

B. family. Multiple exostoses.

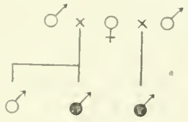

FIG. 3.

Stanley, *Medical Times and Gazette*, 1857, ii, p. 39.

transmit even when themselves unaffected, then probably a large number of the cases that arise apparently *de novo* are really inherited through one or more generations of unaffected females, and the power of transmission is probably represented equally in the two sexes. The severity of the affection varies much in different individuals, but it cannot be said that it is altered either in the direction of greater or less severity in the succeeding generations. An individual who has inherited the abnormality is just as likely, and no more, to be severely affected as his parent or grandparent.

We have here to do with a condition which is handed down by an affected person unchanged to about half his children by a normal consort, and his abnormal offspring again transmit it unchanged to half their progeny. The normal members of the family as a rule do not pass on the condition, but a certain number of the apparently normal females carry the abnormality, and can transmit it to half their children by a normal mate. The hereditary transmission conforms here fairly closely with Mendel's law as worked out in animals or plants. Each somatic cell in the body may be regarded as double in its constitution, one part being derived from each parent. These parental constituents may be similar or different, and the character of the somatic cell may be similar to both the parents, be like one parent only, or be a blend of the two. In the generative cells or gametes, these parental constituents separate or segregate, so that each gamete has only one of them, half the gametes being of the character derived from one parent and half of the character from the other, and each zygote formed by the union of two gametes when developing into an embryo has the constituent derived from only one of its grandparents on each side. These parental constituents are known as allelomorphs; and when in any cell they differ from one another, this difference may usually, if not always, be expressed as the presence or absence of some special feature. For example, the colour of the human iris seems mainly to depend on the presence of something which causes the deposit of visible pigment in front of the iris when the various types of brown eye are developed, or the absence of this something when the eyes belong to the various shades of blue. If only the factor for presence be in the somatic cells, then the quality will always show itself. If both opposing factors, however, be present in all the somatic cells, then it will depend on whether the factor for presence or the factor for absence is the dominant one as to whether the particular quality is shown. As a rule, the allelomorph for presence is dominant, and always shows its attendant quality whenever it exists in the somatic cells, whether it is pure there or mixed with the opposite allelomorph for absence. Sometimes, however, the usually dominant factor may be present and yet not show its quality, so that an individual belonging to a family in which that quality exists may be able to transmit it to his descendants, although he himself does not exhibit it. Bateson has shown that this incomplete dominance occurs with regard to extra toes in fowls, and this may explain some types of inheritance in human beings, such as in epidermolysis bullosa. In other cases the allelomorph for presence may be dominant in one sex and recessive in the other, as has been found to be the case with horns in sheep, which are dominant in the male and so always exhibited when the factor is present, but recessive in the female, in whom horns only appear when both allelomorphs are of the same kind—those which produce horns. Thus the power to develop horns may be imparted by hornless females to some of their male offspring by a hornless male, but hornless males never transmit the power to develop horns. Colour blindness affords an example of similar inheritance in man.

One can imagine a condition being completely dominant in one sex and incompletely dominant in the other. Under these circumstances, the one sex would be more affected than the other, and one would rather expect the affected individuals of the one sex to be more severely attacked than those of the other. So far no example of this form of inheritance seems to have been met with in animals or plants where it could be thoroughly investigated by experimental breeding. This seems, however, the easiest explanation of the greater incidence of multiple exostoses in the male and the fact that it is not infrequently transmitted by normal females, but it is to be noted that we have no very strong evidence that the affected females are any less severely attacked than the males, although the very slight cases seem all females. Another example of such a form of inheritance is possibly afforded by hæmophilia. Bulloch has recently argued, with much plausibility, that females are never affected by hæmophilia properly so-called. Under these circumstances, hæmophilia would be a strictly sex-limited condition, like Daltonism. If so, there is another condition—certainly less definite than hæmophilia—which is also characterized by an abnormal tendency to bleed, and which is hereditary. Allowing for an exaggerated proclivity to class females as bleeders whose menstruation has been excessive or who have had epistaxis, this other hæmorrhagic diathesis attacks females as well as males, but in fewer numbers and less severely, while it is commonly transmitted through normal females belonging to the abnormal families. Some two years ago one of us (Dr. Gossage) suggested a similar explanation for cretinism, which attacks males more frequently and more severely than females, and in which, according to McCarrison, the mother of a cretin child frequently shows signs of thyroid deficiency. Dr. McCarrison's assistant, Sirdar Sahib Kehar Singh, has since then carefully collected details from fourteen families at Gilgit, noting the occurrence of cretinism, goitre, myxœdema, deaf-mutism and tetany. The results of this careful and able piece of work show that no such simple scheme of heredity can possibly prevail in cretinism, if it be hereditary at all. Dr. Gossage's hearty thanks are due to Sirdar Sahib Kehar Singh and to Major McCarrison for the completion of this investigation.

If this view be correct, that there is a deficiency in the number of affected females because the condition, while dominant in the male, is only incompletely dominant in the female, then we ought to add a sufficient number to the total of abnormal females to make it equal to that of the males. This would make the grand total of abnormals 171 to 135 normal—a decided excess of the abnormal. It is improbable, however, that all these thirty-five families are really complete, and it is especially the normal members who tend to be left out. As a matter of fact, an excess over the expected number of abnormal individuals is found in other conditions in man where we have reason to suppose that the abnormal condition is dominant to the normal. For example, this has been shown to be the case by one of us with regard to tylosis palmaris et plantaris.

The following are some details of the cases on which this paper has been founded, the cases having been examined by one of us or by one of our colleagues. We beg to tender our thanks to our colleagues for permission to use their notes:—

Case I.—James H., aged 9 in 1901, a fairly healthy looking boy, who was alive and well when last heard of in 1909. The following exostoses were found:—Chest: Two or three spines at the sternal end of the left clavicle, spines on second and third left ribs, and a small exostosis on the fifth right rib. Right arm: Bony swelling in front of lower end of radius (removed in the East London Hospital for Children) and another at the outer side, larger swelling at the lower end of ulna. Left arm: Spiny process in front of great tuberosity of humerus, large bony swellings at lower ends of radius and ulna. Right leg: Exostoses about lower condyle of femur and near adductor tubercle (one of which was removed), exostosis at upper end of tibia (removed) and also a sharp, bony spine, an exostosis at lower end of fibula. Left leg: Exostoses on condyles of femur, large exostosis on inner part of head of tibia and another at head of fibula, spine above external malleolus. In this case there were at least twenty-two separate exostoses.

Case II.—Violet H., aged 4, sister of James H., was brought to the East London Hospital for Children in 1903, suffering with general tuberculosis, from which she died ten days later. Right arm: Exostosis on humerus near the shoulder-joint, an exostosis, stated to have been present at birth, over the lower third of ulna, large exostoses over lower ends of radius and ulna, small exostosis on the little finger. Left arm: No exostoses noticed. Right leg: An exostosis on the femur, just above the knee-joint. Left leg: An exostosis on the inner side of the tibia, just below the knee-joint. Except the exostosis on the ulna, none of these had been noticed until the age of 3, after which they were all stated to have increased rapidly in size.

Case III.—Mrs. H., mother of Cases I and II, a healthy woman aged about 45 when interviewed in 1909. She stated that she had never had similar lumps to those in her children, and no exostoses could be found on examination. Her husband, her husband's relatives, and her own relatives had never had similar tumours. She gave a full family history, which is represented in fig. 1. In this the abnormal individuals are marked black, the normal white, and sex is indicated in the usual way. (Several normal persons may be shown by a numeral in a white circle.)

Case IV.—Fred. B., aged 14 in 1902, a healthy boy in whom the osteomata had first been noticed when aged 2: height, 4 ft. $3\frac{3}{4}$ in. Right leg: Toes free; small nodule head of fifth metatarsal; tarsus free; tibia—an irregular lump, 1 in. from back to front, above inner malleolus; fibula—prominent lump below inner side of head, small nodule above back of external malleolus, large boss, size of half a small orange, over front of head (probably from tibia); femur—one small growth $1\frac{1}{2}$ in. above external condyle, one from inner side of popliteal aspect, low elevation posterior aspect below great trochanter. Left leg: Toes free; small nodule dorsal aspect of fifth metatarsal; small elevation outer side of head of os calcis; tibia—large irregular mass posterior aspect of malleolus, smaller one anterior aspect lower end of tibia, smooth elevation inner side of head; fibula—growth posterior aspect lower end, and discrete projecting nodule anterior aspect of head; femur—bifurcated mass on posterior aspect of outer condyle, bifurcated mass anterior and inner aspect of inner condyle. Dorsal aspect of trunk: Small mass about 2 in. from head of twelfth right rib, rounded nodule outside angle of sixth rib; scapulæ both winged by masses on anterior surface—on the right side a large, nodular, irregular mass, 2 in. by 2 in. by $1\frac{1}{4}$ in., growing from the ribs over which scapula rides, one nodule on spinal border of right scapula and two near root of spine, two nodules on angle and spine of left scapula, and one on lower part of axillary border. Anterior aspect of trunk: Clavicles—nodules at outer ends of both, very small left, about size of a bean right; two or three nodules on left sixth and seventh ribs. Right arm: Tiny nodule on second phalanx of second finger and also on first phalanx of first finger; metacarpus free; one tiny nodule dorsal aspect trapezoid; radius—two tiny nodules and one as big as a marble at lower end (two dorsal and one ventral); ulna—one inner aspect lower extremity, whole hand deflected towards ulnar side; humerus—one in axilla and one just above deltoid insertion. Left arm: One at the base of second phalanx of second finger, one near the heads of both first and second metacarpals, one on anterior aspect of trapezius; radius—two small ones at dorsal aspect of lower end and a larger prominent one at ventral aspect; one on inner side lower end of ulna; humerus—large one in axilla and one on outer aspect about middle of deltoid. None on the vertebræ, pelvis, or other bones. Feet in good position and no impairment of movement. All the calvarial sutures are very prominently marked.

Case V.—Florence B., aged 12 in 1902, a healthy girl who had had scarlet fever when aged 7; height, 4 ft. 3 in. Right leg: Toes and metatarsus free; small nodule inner dorsal aspect of base of cuboid; tibia—one 2 in. above malleolus and a prominent one 2 in. below inner aspect of head; fibula—diffuse one about head; femur—four at least about lower end, two in popliteal space, marked spine above external condyle, enlargement of posterior aspect of upper part of shaft. Left leg: Toes free; metatarsus free; one nodule head of astragalus; tibia—two at lower end, one prominent one below inner side of head; fibula—discrete nodule lower end, diffuse swelling upper end; femur—several at lower end, one large mass in outer part of popliteal space. Trunk: Ribs free; nodules at inner and outer ends of clavicles; pelvis—nodule over right posterior superior spinous process and one at middle of left iliac crest; one on last dorsal vertebra; scapulæ—symmetrical bunches about bases of spines and a nodule at middle of spine on right side (injury at 3 years). Right arm: Nodule dorsal aspect base of first phalanx of second finger and one or two over heads of metacarpal bones; much ulnar deflection of hand, ? absence of lower epiphysis of ulna, large mass about lower end of ulna which is possibly callus; humerus—one nodule beneath deltoid and one in axilla. Left arm: Nodule at base of second phalanx of fourth finger; fairly large nodule over dorsum of head of second metacarpal bone; nodule dorsal side of trapezoid; one, dorsal, lower end of radius and one on inner side of lower end of ulna; humerus—one anterior aspect of shaft at junction of upper and middle thirds; one small one in axilla; hook-shaped spine beneath deltoid. Head and neck *nil*. Now, in 1910, she is a healthy girl of short stature. The exostoses are much the same, but she has a fresh one at the root of the terminal phalanx of the first finger of the left hand, which followed an injury (*see* figs. 5, 7 and 9).

Case VI.—Harriet B., aged 10 in 1902, had had rheumatic fever when aged 5; height, 3 ft. 8 in. Right leg: Toes free; nodule on inner side of head of os calcis; one on outer side base of fifth metatarsal bone; tibia—one above and behind malleolus and one on anterior aspect just below head; fibula—large mass on outer side below head; femur—at least six about lower end, largest in middle of popliteal space, one on anterior and one on posterior aspect of upper end of shaft. Left leg: Toes free; one nodule each over the heads of the second, third and fifth metatarsal bones and over the bases of the first and fifth; several nodules over the cuneiform, os calcis and astragalus; tibia—markedly nodular at lower end, an exostosis just below inner side of head; fibula—nodule over malleolus, mass below head on outer side; femur—several about inner, outer and anterior aspect of lower end, none in popliteal space. Anterior aspect of trunk: Several large and prominent nodules over the ribs, on the right side in the anterior axillary line, on the left further forward; spines of vertebræ and pelvis free; slight lateral curvature of spine; scapulæ—very large mass on anterior aspect of right, symmetrical nodules about vertebral borders and spines of both. Right arm: Nodule on anterior aspect of head of first phalanx of second finger; nodule near head of first metacarpal and also on dorsal aspect of os magnum; radius—nodule on outer and anterior aspect of lower end;

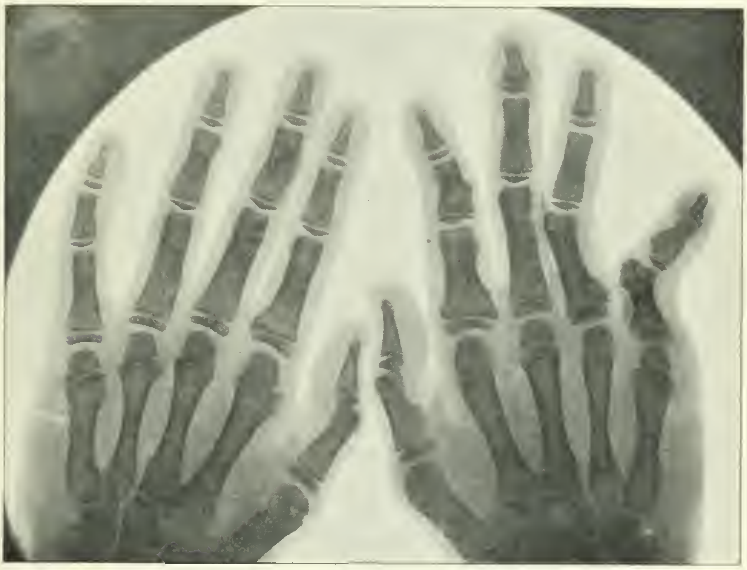

Fig. 4.

Dorothy B.—Note the distortion of the joint surface of the first phalanx of the right little finger, and the many irregularities of outline of the shafts of the phalanges.

ulna free; humerus—one on outer side, upper end, beneath deltoid, and one in axilla. Left arm: Nodule at base of second phalanx of second finger and of second phalanx of third finger; nodules, dorsal, at lower ends of third and fourth metacarpals; carpus free; radius—several on both anterior and posterior aspect of lower end; ulna—one just above styloid process posteriorly; humerus—two beneath deltoid, one anterior and one posterior; one small one in axilla. Very slight ulnar deflection of hands.

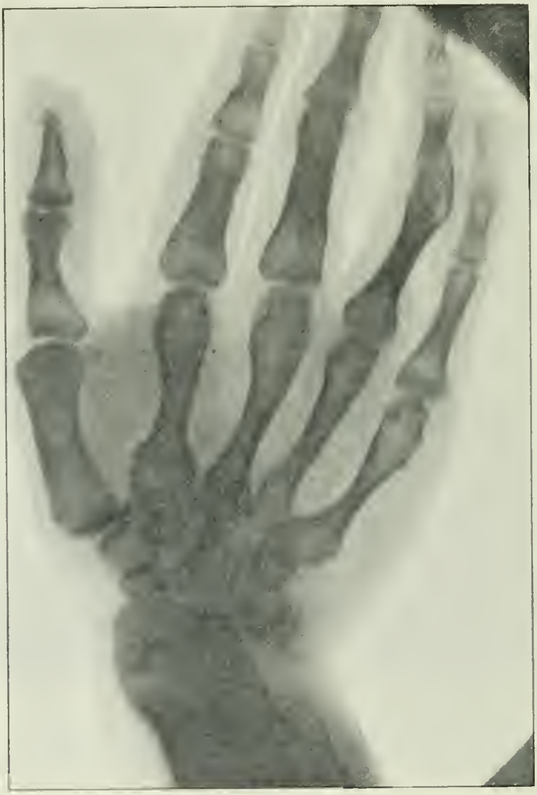

Fig. 5.

Florence B.—Note the ulnar deviation, the massive lower end of the radius. The interphalangeal joint of the ring finger became ankylosed after an attempt to excise a spur-like osteoma.

Case VII.—Dorothy B., now aged 12. She was examined in 1902, but most of the notes have been unfortunately lost; still it can be definitely said that the tumours have increased in number and size in the past eight years. At present the description and location of her exostoses are as follows:—Right leg: Small nodule at the proximal end of first phalanx of little toe; metatarsal bones and tarsus free; tibia—a mass as large as a small orange projecting from back of head into popliteal space; femur—thickening over external condyle, an irregularly shaped projection on outer side just below great trochanter, nodule at same level on inner side. Left leg: Toes free; nodules on heads of second, third and fourth metatarsal bones; tibia—hook-shaped projection on inner side of head; fibula—mass on posterior aspect of head: femur—hook-shaped projection over external condyle and below this a small nodule, two small nodules on inner side at upper end. Trunk: Nodule at posterior angle of eighth left rib, pedunculated projection from upper vertebral border of right scapula, nodule at lower angle, a bunch of two or three nodules at upper vertebral angle of left scapula and one on the spine. Right arm: Nodules at proximal and distal ends of first phalanx and at distal end of second phalanx of first finger (second phalanx of this finger is bent so that distal part of the finger leans towards the thumb); nodule at base of first phalanx of third finger (this finger is bent sharply towards the fourth finger between the first and second phalanges); nodules at distal end of first phalanx and at proximal

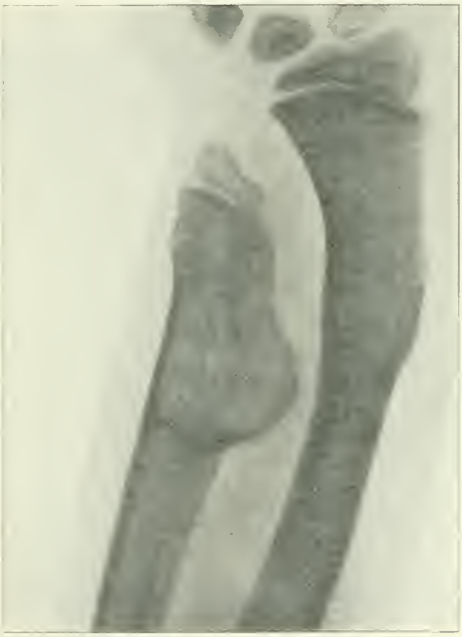

FIG. 6.

Dorothy B.—Lower end of radius and shortened ulna.

end of end phalanx of little finger (this finger is bent sharply outwards at junction of first and second phalanges); there is also a nodule on the palmar aspect of proximal end of the first phalanx of thumb; nodule on inner side of proximal end of first metacarpal bone; there is a large mass over the distal end of the ulna and thickening of the outer side of the lower end of the radius; the upper end of the humerus is markedly thickened on both the outer and inner sides. Left arm: Nodules on distal ends of second phalanges of two middle fingers and on proximal end of first phalanx of third finger; nodules at distal ends of first, third and fourth metacarpal bones; there is some thickening of the lower end of the ulna, chiefly on its radial side; nodule at upper end of the humerus beneath the deltoid. Although there is very striking deformity of the right hand in this patient, there is no marked ulnar deviation of the hands as a whole like that in her sister Florence (Case V) (*see* figs. 4, 6 and 8).

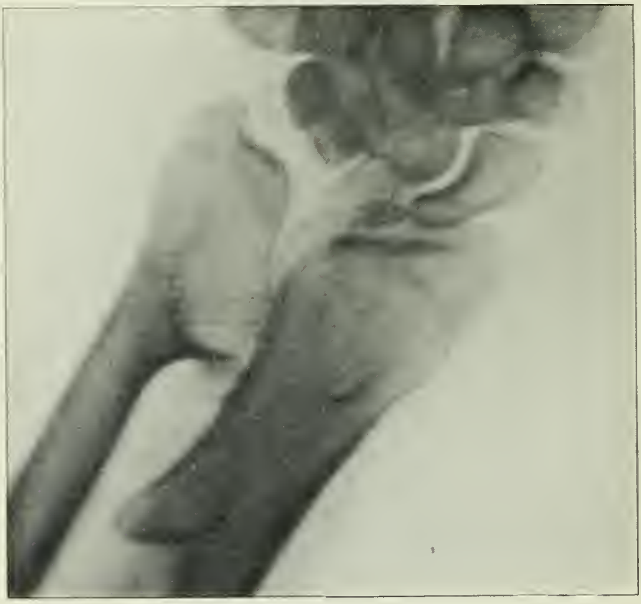

FIG. 7.

Florence B.—Lower end of radius and ulna.

Case VIII.—Ernest B., aged 16 in 1902, a delicate, overgrown boy, who shows no signs of osteomata. Is now, in 1910, stated to be strong and healthy; is married, and has a healthy female child aged 18 months; this child shows no signs of bony tumours.

Case IX.—Mrs. B., mother of the above and two other healthy children, who show no signs of exostoses, is now a woman aged 47. Neither her father nor her mother, nor any of her mother's relatives, nor any of her five brothers and sisters or their children, have ever had lumps similar to those on her own children. She stated that she herself, however, had had a few lumps about the fingers and shoulders when a young girl, but that these had disappeared. Her husband and his relatives have never had similar lumps. On examination she was found to be a healthy woman of rather low stature. In spite of her

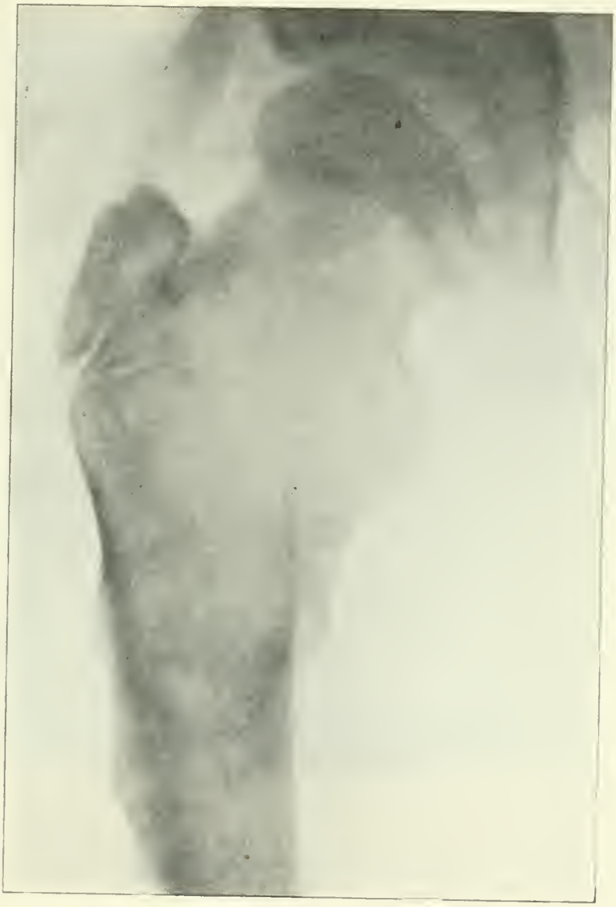

FIG. 8.

Dorothy B.—Upper end of femur. The mass is definitely juxta-epiphysial. The periosteum of the shaft has apparently been irregularly active.

declaration that her lumps had disappeared, no fewer than six typical exostoses were found. There were two small nodules on inner side of head of right tibia, one about the size of a pea on inner side of head of left tibia, and below this a larger one as big as a nut. There was one the size of a pea at the outer end of right clavicle, and a small spine on the proximal phalanx of the second right finger. The family record is represented in fig. 2.

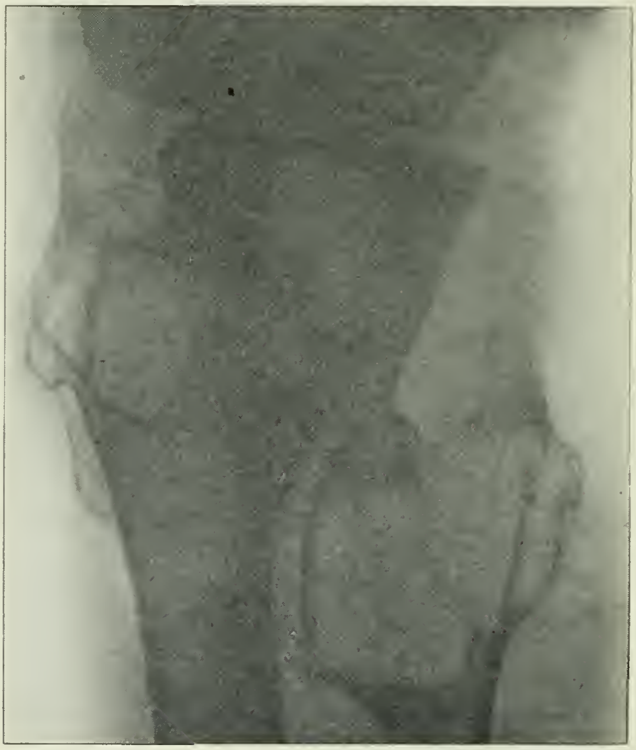

FIG. 9.

Florence B.—Upper end of tibia and fibula. The tenuous cancellous character of the irregular growth is easily made out.

The description of the cases in these two families shows that they correspond closely with those described by others. It is of special interest to notice that in the B. family there is a marked deficiency of length in the bones of the affected members. Thus, while the normal eldest son, Ernest, was 5 ft. 8 in. high at the age of 16, his affected brother Fred was only 4 ft. 3 in. at the age of 14. Again, the normal

Edith B. at the age of 8 was described as head and shoulders taller than her affected sister Harriet, aged 10. Similarly, Florence B., both at the age of 12 and now when fully grown, is decidedly below the normal height. It is also of interest to note the varying intensity with which the individuals are affected, Mrs. B. having only six small tumours, Florence and Harriet B. being moderately affected, while in Fred and Dorothy B. the growths are very large and numerous. We have investigated the family records as closely as possible, but have unfortunately been unable personally to interview more than the nine members of the two families here described. The details as to the other members are therefore less certain than we could wish, being dependent on the statements of Mrs. H. and Mrs. B. We would call attention to the large number of normal relatives claimed by both these women—a number that would not be expected according to the view advocated in this article. It is of interest that Mrs. B.'s mother was German, and multiple exostoses seem much commoner in Germany than in England.

The radiograms of the B. family, some of which are here reproduced, illustrate very well the gross disturbance of growth at the intermediary cartilage. It is apparent that there is much more than mere outgrowth of bone in various shapes and directions, such as might be inferred from inspection of macerated specimens. In some instances the normal differentiation into compact and cancellous tissue seems to be lost for two or three inches at the extremities of the diaphysis. In many situations, particularly at the ends of the metacarpals, and in the upper extremity of the humerus—not illustrated in the reproductions—there is a marked transparency of the bone comparable to that seen in some forms of osteo-arthritis.

BIBLIOGRAPHY.

Hereditary Cases with Fairly Complete Family History.

Affected individuals marked black, normal white. Sex indicated in the usual way.

	♂	♀	○
Bessel-Hagen. *Arch. f. klin. Chir.*, Berl., 1891, xli, p. 420	3	2	5
Idem	2	0	3
Cotes. *Trans. Clin. Soc. Lond.*, 1890, xxiv, p. 228	1	0	4
Dittrich. Inaugural Dissertation, Kiel, 1892	1	1	2
Fischer. *Deutsch. Zeitschr. f. Chir.*, Leipz., 1880, xii, pp. 34 and 357	3	0	3
Idem	4	0	1
Idem	2	0	2
Francis. *Lancet*, 1902, i, p. 1765	1	0	0
Gibney. *Amer. Journ. of Med. Sci.*, 1875, lxxii, p. 73	3	2	3
Heymann. *Virchows Archiv*, Berl., 1886, civ, p. 145	7	1	11
Hœber. Inaugural Dissertation, Munchen, 1901	4	2	0
Jungmann. *Berl. klin. Wochenschr.*, 1902, xxxix, p. 890	1	1	2
Keller. Inaugural Dissertation, Halle, 1901 (Cf. also Braune and Cholewa.)	3	0	6
Kirmisson. *Rév. d'Orthop.*, 1905, vi, p. 245	1	1	0
Kremser. Inaugural Dissertation, Munchen, 1893	2	0	1
Lippert. *Deutsch. Arch. f. klin. Med.*, Leipz., 1903, lxxvi, p. 63	2	4	17
Marle. Inaugural Dissertation, Berlin, 1868	2	0	1
Idem	1	4	4
Nasse. *Klin. Beitr.*, neue Folge, 124 (Quoted by Knoop, Inaugural Dissertation, Bonn, 1897.)	4	0	2
Paget. "Lectures on Tumours," 1851, p. 79	1	0	3
Poore. *Lancet*, 1873, ii, p. 771	0	1	4
Pulle. *Niederland. Tijdschft. v. Geneeskunde*, 1900, i, p. 890	1	0	1
Reinecke. *Beitr. z. klin. Chir.*, Tüb., 1891, vii, p. 657	3	2	9
Reulos. *Le Progrès Med.*, 1885, ii, p. 71	6	5	11
Richter. Inaugural Dissertation, Giessen, 1908	3	1	4
Roberg. Inaugural Dissertation, Bonn, 1886	3	5	5
Idem	2	0	1
Spengler. Inaugural Dissertation, Strasburg, 1887	8	0	6
Idem	0	3	5
Stanley. *Med. Times and Gaz.*, 1853, n.s. vii, p. 39	2	0	1
Stolzenberg. Inaugural Dissertation, Greifswald, 1898	2	3	2
Weber. *Virchows Archiv*, 1866, xxxv, p. 501	2	1	1
H. family	2	2	5
B. family	1	4	8
(Total families—34)	83	45	133
To which must be added—			
Tillmann. *Münch. med. Wochenschr.*, 1898, p. 1073	5		2
(35 families)	133		135

Hereditary Cases with Incomplete Family Records.

Ausch. *Deutsch. med. Wochenschr.*, 1904, p. 1304.

Benneke. *Centralbl. f. Chir.*, Leipz., 1901, xxviii, p. 315.

Chiari. *Prager med. Wochenschr.*, 1892, xvii, p. 403.

Cruveilhier, quoted by Braune. Inaugural Dissertation, Halle, 1882.

Drescher. Inaugural Dissertation, Giessen, 1889.

Fischer. Loc. cit.

Hartmann. *Archiv f. klin. Chir.*, Berl., 1893, xlv. p. 572.

Hashimoto. *Archiv f. klin. Chir.*, 1885, xxxii, p. 1.

Herbst. Inaugural Dissertation, Berlin, 1890.

Kienböck. *Wien. klin. Wochenschr.*, 1903, p. 109.

Läwen. *Deutsch. Zeitschr. f. Chir.*, 1904, lxxv, p. 14.

Lippert. Loc. cit.

Maclean. *Bristol Med.-Chir. Journ.*, 1890, viii, p. 217.

Marle. Loc. cit.

Nast. Loc. cit.

Pels-Leusden. *Deutsch. Zeitschr. f. Chir.*, 1907, lxxxvi, p. 434, who gives a complete bibliography.

Price. *Lancet*, 1868, i. p. 631.

Roberg. Loc. cit.

Schaefer. *Beitr. z. klin. Chir.*, Tüb., 1901, xxxi, p. 228.

Schmidt. Inaugural Dissertation, Griefswald, 1868.

Schor, quoted by Herbst. *St. Petersb. med. Wochenschr.*, 1881, p. 327.

Seidel. *Centralbl. f. Chir.*, 1885, xii, p. 12.

Sonnenschein. Inaugural Dissertation, Berlin, 1873.

Stanley. "Diseases of Bones," 1849, p. 213.

Starck. *Beitr. z. klin. Chir.*, 1902, xxxiv, p. 508.

Villemin. *Rév. d'Orthop.*, 1903, iv, p. 49.

Ziegler. *Münch. med. Wochenschr.*, 1892, xxxix, p. 552.

Other References.

Bateson. "Reports to the Evolution Committee of Royal Society," 1905, ii, p. 114.

Gossage. *Quart. Journ. of Med.*, 1908, i, p. 331.

Karewski. *Berl. klin. Wochenschr.*, 1892, xxix, p. 257.

Kohler, quoted by Pels-Leusden. *Fortschr. a. d. Geb. d. Röntgenstrahlen.* 1905, viii, Heft 1.

Kryger. *Archiv f. klin. Chir.*, 1898, lvii, p. 859.

McCarrison. *Proc. Roy. Soc. Med.*, 1908, ii (Med. Sect.), p. 1.

Ollier. Cp. Payr in Wallstein u. Wilm's "Lehrbuch der Chirurgie," 1909, ii, part 2 p. 403.

Reubsaët. *Thèse de Par.*, 1909.

DISCUSSION.

Mr. WILLIAM BATESON, F.R.S., thanked the Council for inviting him to hear the paper. He knew nothing of the subject beyond what he had learned from Dr. Gossage; but it was an admirable thing that Dr. Gossage should have co-ordinated this evidence. Dr. Gossage had made a number of other collections of a similar character, and this last was a most useful addition to the series. When they came to consider the system of heredity which this particular condition followed, the problem was a rather baffling one. The condition was evidently transmitted in a very complex manner, and was influenced presumably by various factors present in the body besides the particular factor to which the disease itself was due. Nevertheless, he thought Dr. Gossage was entitled, on the evidence he had put forward, to his view that the inheritance was more or less that of a Mendelian dominant: but so many complications must necessarily be introduced that, as Dr. Gossage had stated, this conclusion must at present be regarded as provisional. They were greatly in need of the data of new examples of cases in which heredity was sex-limited. The best-known case up to the present was colour-blindness, and there was also hæmophilia; but beyond these they had no considerable evidence to work upon of sex-limited heredity in man, although in animals there were several instances. It was true that they now understood fairly well the lines which were followed by these systems of inheritance; but more examples would be welcome, especially with regard to numerical effects. He confessed to a difficulty in thinking that the condition which formed the subject of the paper was a case of sex-limited inheritance comparable with that of colour-blindness. The point that weighed particularly with him was the evidence as to inheritance from the affected females. One would expect the daughters of affected females to be affected in greater proportion than the daughters of unaffected females by affected males, but there was no evidence at all of this from the genealogical tables which Dr. Gossage had shown. He would like, while speaking to the Section, to take the opportunity of saying how greatly they were in need of evidence of sex-limited conditions in which the limitation was to females rather than to males. It was possible that such things did not exist, but if they did exist students of heredity ought to begin to learn something about them. Their study would greatly contribute to the general theory of heredity. The only disease to which it might possibly apply, he thought, was Graves' disease. He had examined the evidence as well as he could, and on the whole, though he did not think that Graves' disease could be brought within the sex-limited class, the mode of its descent was worth investigating.[1] He was unaware of any other condition that seemed likely to come within the scope of that system of transmission. He was grateful to Dr. Gossage, who had made admirable collections on similar subjects at intervals, but he thought that this was one of the most important he had yet done.

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[1] Evidence as to heredity in Graves' disease has been collected by Ernst Schultheiss, "Üb. Erblichkeit bei Morbus Basedowii," Inaugural Dissertation, Jena, 1909.

Dr. Russell Wells said he was by no means convinced that Mendelism applied to questions of disease in the human subject. Possibly the class of cases mentioned might be instances of Mendelian inheritance, but he did not see that the tables put forward proved anything beyond the fact that the condition in question ran in families, and was probably transmitted by inheritance. If one took the histories of the individual families given and examined them separately, it was noticeable how many of them did not seem to correspond to what the hypothesis of Mendel required. To take one instance, in more than one case all the children had exostoses, though he did not gather that both parents were abnormal. No doubt it was particularly difficult to get complete histories, and they were indebted to Dr. Gossage and Mr. Carling for the great amount of work the paper represented; but he noted that even where they had fairly complete family histories they based their argument on the last generation alone. He would be glad if, in his reply, Dr. Gossage would point out all the data this investigation supplied in support of the Mendelian hypothesis. Possibly he was somewhat biased because he had performed some experiments in breeding rabbits, which he would probably never publish because circumstances prevented them being complete, but which made him doubt whether the full Mendelian doctrine could be applied to the higher animal kingdom, as there was reason to believe it could to the vegetable. The year before last he crossed a Himalayan doe rabbit with an Angora buck. These two sorts of rabbits are quite distinct varieties, breeding true; they are both albinos in the sense that they have pink eyes, but the Angora has long white hair all over, while the Himalaya, though chiefly white, has black ears, nose, paws, and tail, and its hair is short. The progeny of this union were all white rabbits with short hair. Some died, but he saved sufficient to mate bucks with does of the same litter: on the Mendelian hypothesis he expected to find in their progeny some with long hair and others with black points, but it was not so; all the young in this second generation were like their parents, completely white rabbits with short fur. Some of this second generation were mated together, and their young ones again were all white rabbits with short fur. The interesting point is that this experiment resulted in the production of a race of rabbits intermediate between the Angora and the Himalaya, having neither the long hair of the one nor the black points of the other, and not, as one would expect on the Mendelian theory, some with one or other of these characteristics. He could not say what the numbers were, but he would try to ascertain them.

Mr. Carling said he could not add anything to the aspect of the question now being discussed, and he disclaimed credit for the industry which had been displayed in collecting the family histories; that credit belonged to Dr. Gossage. But it might be of interest to show a specimen from the museum of the Dreadnought Hospital, which illustrated many of the surgical points: the size of the tumours, their liability to compress important structures, the general tendency of exostoses in the limbs to run upwards, and some of the difficulties which might be experienced in removing them. It was noteworthy that the man from whom the specimen was obtained died of chondro-sarcoma of the rib. Whether it began in an exostosis could not now be said, but it was interesting that multiple benign osteomata should be associated with terminal sarcoma of bone.

Professor BATESON remarked that the Himalaya rabbit was one of the strangest problems which students of the Mendelian theory had to deal with, and any evidence about it would be most welcome.

Dr. GOSSAGE, in reply, said that one of the points brought up by Dr. Russell Wells was as to how far these families of multiple exostoses could be taken as evidence in favour of Mendelism. It was doubtful whether one could take anything from human beings as evidence of Mendelism if one had to start afresh, and had no other evidence of Mendelism at all. Mendelism depended on evidence which was obtained from experimental work with animals and plants in regard to which one could repeat the experiment until the proof seemed certain. Evidence from human beings could not be corrected like that derived by experimental methods, because each marriage was haphazard; but one was justified in saying that when one found that an abnormal individual mated with a normal had half his children normal and half abnormal, and that similar phenomena went on from generation to generation, it conformed with Mendel's law. In multiple exostoses there was evidence that the families of the affected people when crossed with normals were, taking them as a whole, half of them abnormal and half of them normal. He said taking them as a whole, because one could not judge from a single family, in which it was a "toss up" whether a particular child would be normal or abnormal, and the total number of children was very limited. In the tables there were numbers running into hundreds; there were the actual figures of 133 abnormal and 135 normal, drawn from a large number of families, and the collecting of large numbers of families was the only way in which one could possibly put forward evidence from human beings. But supposing it to be true that the female could often carry the abnormality without showing it, one had to add a considerable number of females to the abnormals, which made the abnormals somewhat too numerous. In other conditions where the presumption that they conformed with Mendelism was more justified, the same excess of abnormals was obtained, and it was not difficult to explain, because if one tried to get a family history from a woman of the less intelligent class it would be found that she remembered the abnormals but not the normals, sometimes even among her own children, still more so among her brothers and sisters and other relatives. So the abnormals would be mentioned, while the normals tended to be omitted; 171 and 135 were sufficiently near equality to justify the consideration that the condition was handed down more or less on Mendelian lines. As to the exact manner in which it was handed down, that was a very difficult problem, one in regard to which human evidence could not possibly supply an answer. The only way in which a working theory could be obtained as to the manner of the hereditary transmission would be by finding some such condition in animals where it allowed of being submitted to exact experimental investigation. Multiple exostoses were not, properly speaking, a disease, except in so far as they occasionally caused unpleasantness and the pressure of the tumours gave discomfort. Many of the people were perfectly healthy, and so the condition was better described as an abnormality.

Medical Section.

November 22, 1910.

Dr. J. Mitchell Bruce, President of the Section, in the Chair.

The Use of Belladonna in Dyspepsia.

By F. Craven Moore, M.D., and Laurence Kilroe, M.D.

Eleven years ago the late Professor Riegel published the results of an investigation into the action of atropine on gastric secretion, an investigation to which he had been aroused by the final establishment by Pawlow of the importance of the vagus in the nervous mechanism of gastric secretion.

He found in the dog, provided after the method of Pawlow with a secondary stomach, that the subcutaneous administration of atropine simultaneously with, or immediately prior to, feeding by the mouth induced a prolongation of the latent period and a diminution in the volume of the gastric juice secreted even to one-eighth of the average normal amount; that its administration subsequent to feeding also resulted in a diminution of secretion, the latent period, however, remaining unaffected; and, further, that the diminution in the volume of juice secreted was associated with a reduction in its acidity even to one-half or one-third of the normal.

In the human subject he found by observations on sixteen individuals, sufferers from ulcer of the stomach or from nervous dyspepsia, that the administration of atropine, subcutaneously especially, and also by the mouth, induced a reduction variable, but also in some instances very considerable, in the acid values of the stomach-contents. He concluded from this that atropine exerted in the human being as in the dog not only a qualitative effect in diminishing the hydrochloric-acid content of the gastric juice, but also, very probably, a quantitative effect in diminishing the volume of juice secreted. Troller, working with Riegel, endeavoured to establish the latter effect by actual observation on the human subject in a case where the secretion of gastric juice was induced by the mastication of meat without any of it being swallowed.

The action of atropine in reducing the acid values of the stomach-contents was confirmed later by von Aldor in six cases, and by Schift in eight out of eleven cases. These observers also investigated the influence of atropine on the peptic activity of the stomach-contents, and with somewhat divergent results, for whilst von Aldor found that the peptic activity was reduced in association with the acid values, though to a less extent, Schiff found it to be quite unaltered, or but slightly and inconstantly reduced. Observations somewhat less complete, but also confirmatory of Riegel's results, have been made by Pirrone, Ferranini, Plesoianu, Forlanini, and Lieutier; whilst Leubuscher and Schäfer and Bouveret and Hayem, on the other hand, were unable to determine that atropine had any action on the secretory activity of the stomach.

There thus appeared to be sufficient evidence to merit this drug worthy of trial in such conditions as are associated with an excessive secretory activity of the gastric glands, and for some years it has been the practice of one of us (F. Craven Moore) to employ at first atropine, and later belladonna, in cases where an examination of the stomach-contents after a test-breakfast has revealed abnormally high acid values. The results of any such treatment are difficult to formulate, but the impression gained is that the administration of belladonna exerts a definite beneficial effect on the subjective manifestations of such conditions. Accordingly it was decided to utilize the opportunities at our disposal to inquire further into the action of this drug by repeated observations on the individual members of a considerable series of consecutive cases.

The considerable variations in the acid and peptic values of the stomach-contents after a test-meal of constant composition which occur from day to day in one and the same individual render it necessary that any adequate conclusions as to the effect on these of any particular factor must be based on averages of repeated observations on the several cases of the series. The importance of this does not appear to have been adequately recognized by previous observers, and accordingly it was anticipated that some amplification of the recorded conclusions might at least result from its adoption.

Our observations were made on a series of 40 individuals, most of whom had manifestations of some gastric disorder. In 12 cases some 80 observations were made on the acid values of the stomach-contents only, in 21 cases 140 observations included both the acid and peptic values, and in six cases 40 observations further included estimations of the volume of the stomach-contents.

The methods employed consisted in the administration of a test-breakfast of 10 oz. of freshly-infused tea without milk or sugar and four "breakfast" biscuits on the empty fasting stomach at 8 a.m., and its subsequent withdrawal one hour later. The evacuated stomach-contents were immediately filtered, and in the filtrate the free HCl and the total acidity were estimated by titration with decinormal soda solution, dimethylamidoazobenzol and phenolphthalein, respectively, being used as indicators, and the results are expressed simply in terms of the number of cubic centimetres of decinormal soda solution required to neutralize the respective acidities in 100 c.cm. of filtrate. The peptic activity and the pepsin-content were estimated by Schiff and Nierenstein's modification of the method of Mett, and the volume of the stomach-contents by the dilution method of Matthieu and Rémond. Alternately with such a control test-breakfast, or on several consecutive days after a series of such, a similar test-breakfast was preceded by the administration of tincture of belladonna (standardized to 0·05 per cent. total alkaloids) in doses of ♏v or very occasionally of ♏x.

The results of our observations show conclusively that the administration of belladonna by the mouth before food in average doses of ♏v of a standardized tincture results in a considerable reduction in the acid values (both absolute and relative) of the stomach-contents, a reduction which in the whole series of observations averaged 13·6 for the free HCl and 13·1 for the total acidity. A consideration of the individual cases shows this action of belladonna to be curiously variable in the different series of cases, in different individuals of the same series, and even the same individual at different times.

On classifying all the cases investigated on the basis of the control estimations into those with hyperacidity (free HCl = 40+), those with normal acidity (free HCl = 20 — 40), and those with hypo-acidity (free HCl = 20 —), as in the following table (Table I), it is of practical interest to find that the action of belladonna in reducing the acidity of the stomach-contents is most marked in those conditions associated with hyperacidity, and least in those with hypo-acidity; whilst subclassifying them according to lesion reveals the curious fact that the drug is particularly effective in reducing the hyperacidity associated with duodenal ulcer.

TABLE I.—EFFECT OF BELLADONNA ON FREE HCl VALUE IN VARIOUS ACIDITIES.

HCl	Gastric Ulcer	Duodenal Ulcer	Miscellaneous	Total
Hyperacidity 40+	[1][7] – 14·7 [2]	[6] – 27·7	[7] – 10·0	– 17·0
Normal 20 – 40	[5] – 12·3	[1] – 4·0	[8] – 11·8	– 11·4
Hypo-acidity 20 –	[2] – 10·1	[0] —	[1] – 4·0	– 8·0

[1] The figures in brackets denote the number of cases investigated.

[2] The difference between the average free HCl values of the control and the belladonna observations.

It is important to note that, in the absence of any precise estimation of the volume of gastric juice secreted, nothing can be inferred from variations in the acid values of the stomach-contents as to the acidity of the native juice; in fact, as the work of Pawlow and of Bickel seems to have established that the acidity of the native juice is curiously constant, variations in the acid values of the stomach-contents can be but the expression of variations in the volume of gastric juice secreted.

The results of our observations on the effect of the administration of belladonna on the peptic activity of the stomach-contents present many features which are difficult of interpretation. Table II (p. 28) presents the averages of the several observations in each of twenty cases in which the peptic activity was investigated.

It should be noted that in these estimations the figures representing the peptic values are obtained by squaring the length in millimetres of a column of coagulated egg-albumin digested in twenty-four hours at 37° C. by (A) the undiluted filtrate, (B) one part of filtrate diluted with three parts of $\frac{N}{20}$ HCl, and (C) one part of filtrate diluted with fifteen parts of $\frac{N}{20}$ HCl.

The figures in column A may be regarded as approximate estimations of the peptic activity of the stomach-contents at the time of their withdrawal from the stomach, whilst those in columns B and C are to be regarded as approximate estimations of the relative amounts of pepsin itself present in the stomach-contents.

From the average figures of all the cases it is apparent that the absolute diminution in the acid values is associated with an absolute diminution in the peptic activity of the stomach-contents and in the pepsin concentration of the stomach-contents, and also from the figures of the total observations it would appear that the relative diminution of the peptic activity and of the pepsin exceeds that of the acid values. A similar relation obtains in the gastric ulcer series, whilst in the miscellaneous series the relative diminution in the pepsin concentration of the stomach-contents is somewhat less than that in the acid values, the diminution in the relative peptic activity being greater. In the duodenal ulcer series, on the other hand, very striking discrepancies are manifest on comparing the effect of belladonna on the acid and peptic values, both absolute and relative. In the individual observations on all classes of case similar discordant results were of occasional occurrence; thus in any one case it happened not very infrequently that two test-breakfasts giving approximately the same acid values yielded very different degrees of peptic activity and pepsin concentration.

The explanation of these apparent discrepancies is very elusive; the method is comparatively simple, the technique is seemingly capable of standardization, and by dilution the influence of variations in the acidity and in the concentration of antipeptic substances is, it is believed, largely eliminated, and yet it is difficult to get rid of the idea that some fallacy in the method is in some measure responsible for these discrepancies.

It may be, of course, that our results are but an expression of the quasi-independence of the acidogenic and pepsinogenic functions of the gastric glands established by Pawlow, although it is well to remember that Pawlow's conclusions as to the pepsinogenic functions of the gastric glands are largely, if not wholly, based on observations made according to the method of Mett.

In order to determine whether the reduction in the acid and peptic values of the stomach-contents produced by the administration of belladonna is in any way associated with mere dilution of the stomach-contents—dilution secretion—observations were made on six cases to determine the average volume of the stomach-contents, but, as is apparent in the tabulation of the results, no such variations (*see* Table III, p. 30) in the volume of the stomach-contents was demonstrable by the method employed as could afford an explanation of the reduction of the acid values by mere dilution.

TABLE II.

No.	Name	Age	Sex	Diagnosis	CONTROL					BELLADONNA					No. of observations	
					HCl	T.A.	Pepsin			HCl	T.A.	Pepsin				
							A	B	C			A	B	C	C	B
16	A. U.	37	F.	Gastric ulcer	78·5	89·5	186·5	178·0	66·0	77·6	91·0	157·5	90·0	42·0	2	2
17	T. C.	48	M.	,,	69·0	86·0	158·5	61·5	47·0	61·3	80·3	59·5	36·0	40·0	4	3
18	H. G.	22	M.	,,	59·0	80·0	126·5	118·5	63·3	50·7	73·0	95·0	118·0	67·0	6	8
19	A. B.	32	F.	,,	44·0	66·5	126·0	102·5	47·0	21·5	53·0	30·0	55·0	32·0	2	2
20	A. M.	34	F.	,,	33·6	55·2	72·6	107·0	41·0	20·2	44·5	61·0	75·5	31·0	5	4
21	A. S.	20	F.	,,	33·0	53·5	93·0	175·5	48·0	6·0	26·0	0·125	21·0	4·6	2	2
22	R. G.	40	F.	,,	17·8	39·2	13·0	73·0	48·0	8·3	27·0	0·83	17·6	10·0	4	3
27	W. D.	49	M.	,,	27·6	45·0	67·2	95·5	42·0	27·0	48·0	105·0	68·0	36·0	4	1
43	J. P.	41	F.	,,	13·3	33·6	4·5	52·2	31·3	2·6	15·0	1·0	20·6	7·6	3	3
15	A. W.	49	M.	,,	86·0	101·0	170·0	119·0	52·0	63·5	83·5	62·0	56·0	27·0	4	5
				Average ...	46·0	65·0	101·7	106·2	48·5	34·0	54·0	57·0	55·0	29·7		
23	H. L.	40	M.	Duodenal ulcer	79·0	93·8	106·0	171·4	43·2	54·0	77·2	112·0	96·0	44·7	5	5
26	G. L.	50	M.	,,	34·3	54·0	134·0	163·0	92·3	30·3	46·3	138·0	161·0	108·0	3	3
38	W. S.	25	M.	,,	55·5	79·0	101·5	100·0	44·2	29·0	48·0	76·5	115·5	42·0	2	1
41	J. M.	31	M.	,,	55·5	71·5	128·0	83·0	48·0	48·0	66·0	100·0	76·5	45·5	2	1
				Average ...	56·0	72·6	117·4	101·3	57·0	40·3	59·4	106·6	112·0	60·0		
28	W. B.	27	M.	Gastroptosis	45·6	69·0	85·0	96·5	65·5	36·0	64·0	57·5	79·0	49·7	3	9
30	S. S.	34	F.	,,	36·2	63·2	166·5	161·6	103·4	18·0	41·0	19·6	86·6	78·0	5	4
29	A. C.	20	F.	Chlorosis	39·0	63·5	69·5	58·5	30·0	22·5	36·5	49·0	36·0	16·0	2	2
32	S. W.	18	F.	,,	35·5	73·0	45·5	105·5	43·5	18·8	46·5	34·2	107·4	50·6	2	5
36	E. C.	25	F.	,,	4·2	21·0	0·73	31·0	30·0	—	15·5	—	9·5	1·75	4	2
31	M. O.	55	F.	,,	32·0	57·5	110·0	93·0	30·0	19·0	42·5	18·0	42·0	18·0	2	2
				Average ...	32·1	57·9	79·5	91·5	50·4	19·1	41·0	29·7	60·1	35·6		
Average of all cases ...					43·9	64·7	103·2	106·4	50·7	30·7	51·2	58·7	68·3	37·5		

Relative Diminution in Acid and Peptic Values.

	HCl	T.A.	Pepsin		
			A	B	C
Gastric ulcer	25 per cent.	17 per cent.	44 per cent.	48 per cent.	38 per cent.
Duodenal ulcer ...	28 ,,	18 ,,	9 ,,	2 ,,	5 ,,
Miscellaneous	40 ,,	29 ,,	62 ,,	34 ,,	23 ,,
Total cases ...	30 ,,	21 ,,	43 ,,	35 ,,	26 ,,

The irregular and relatively small variations in the volume of the stomach-contents in the individual cases disappear in the averages of the total observations. If it is assumed that the diminution of the acid values of the stomach-contents produced by belladonna is the result of a diminution of the volume of gastric juice secreted rather than of a diminution of its HCl concentration, as is in accordance with the observations of Pawlow on animals and of Bickel on the human subject, then the absence of any corresponding diminution in the volume of the stomach-contents can only be interpreted as being due to a coincident inhibitory action of the drug on the motor functions of the stomach leading to delayed emptying, an action of belladonna which has already been established.

A consideration of the individual observations shows that the action of belladonna in diminishing the acid and peptic values of the stomach-contents is transitory. Thus a control test-breakfast on a day following the administration of belladonna, or after several consecutive days on which the drug has been administered, will yield acid values closely approximating to those obtained by such previous controls. In no case did we find any clear evidence of the establishment of tolerance to the drug, although such has been stated by Schiff to obtain comparatively early.

In addition to these observations on the acid and peptic values of the stomach-contents, we have more recently been able to ascertain the action of this drug on certain disorders of gastric motility. In the course of some radiographic observations it was noted that the administration of belladonna rapidly sufficed to relax abnormal spasm of the gastric musculature, and even accelerate the transit of the stomach-contents into the duodenum. This spasm-inhibiting action, which is referred to by Jonas, is probably of considerable therapeutic importance, and with the action on gastric secretion renders belladonna worthy of a definite place among the serviceable drugs in the treatment of gastric disorders.

TABLE III.

No.	Name	Age	Sex	Diagnosis	Control			Belladonna			No. of Observations	
					HCl	T.A.	Volume of stomach contents in c.cm.	HCl	T.A.	Volume of stomach contents in c.cm.	C	B
37	A.W.	49	M.	Gastric Ulcer	86·6	100·3	156·3	70·25	89·5	158·7	3	4
39	H.G.	22	M.	,,	55·2	77·7	222·2	45·0	69·3	212·0	4	3
40	A.B.	32	F.	,,	55·0	73·0	97·0	20·0	60·0	98·0	1	1
42	C.W.	20	F.	,,	38·5	62·5	208·2	26·0	48·6	199·6	4	3
43	J.P.	41	F.	,,	13·3	33·6	138·3	2·6	15·0	184·3	3	3
38	W.S.	25	M.	Duodenal Ulcer	55·5	79·0	333·5	40·0	64·5	259·5	2	2
41	J.M.	34	M.	,,	50·0	68·7	179·0	44·3	66·3	214·0	4	3
	Average ...				50·6	70·7	190·6	35·4	59·0	189·4		

BIBLIOGRAPHY.

von Aldor. *Zeitschr. f. klin. Med.*, Berl., 1900, xl, p. 248.

Bouveret und Hayem [cited by Riegel]. *Zeitschr. f. klin. Med.*, 1899, xxxvii, p. 398.

Ferranini. *La Riforma Medica*, 1895, xi, pt. 2, pp. 579, 591.

Forlanini. *Gazetta Medica de Torino*, 1892.

Kaudewitz. Cited by Riegel.

Leubuscher und Schäfer. *Deutsch. med. Wochenschr.*, 1892, xviii, p. 1038.

Lieutier. *Thèse de Par.*, July, 1904.

Mathieu et Rémond. *Comptes rend. Soc. de Biol.*, Par., 1890, 9 me sér., ii, p. 591.

Nierenstein and Schiff. *Berl. klin. Wochenschr.*, 1903, xl, p. 268.

Penzoldt. *Pharmacol. clin.*, 1893.

Pirrone. *Riforma Med.*, Roma, 1903, ix, pp. 841, 879.

Plesoianu. *Thèse de Par.*, 1897.

Riegel. *Zeitschr. f. klin. Med.*, 1899, xxxvii, p. 381.

Sailer and Farr. *Amer. Journ. of Med. Sci.*, Philad., 1907, n.s., cxxxiii, p. 113.

Schiff. *Archiv f. Verdauungskrankh.*, 1900, vi, p. 107.

DISCUSSION.

Dr. HERTZ said he was very glad to hear the results obtained by Dr. Craven Moore and Dr. Kilroe, because they confirmed the clinical experience which he had himself had with atropine in the treatment of gastric and duodenal ulcer. He wished to know how long before a meal the belladonna was given, and how often in the day the dose was repeated. Dr. Moore said that in his early experiments he used atropine and later belladonna. He (Dr. Hertz) would like to know whether Dr. Moore had some special reason for preferring belladonna to atropine. One of the most remarkable conclusions drawn from this investigation was that, although the percentage of hydrochloric acid was always diminished, the volume of the gastric contents did not diminish. That differed strikingly from the effect of atropine on other glands; the characteristic action of atropine was seen in salivary secretion, in which the volume of the saliva was diminished, but its character remained unaltered. In the case of the stomach, it would seem that one particular part of the functional activity of the gland cells was altered, as they secreted less hydrochloric acid but the same quantity of water as before. He was much interested in the experience of the authors in regard to belladonna relieving "spasm" of the stomach. He asked what was the evidence of the spasmodic condition, and whether they had actually seen it by means of the X-rays. He had watched with X-rays the effect of belladonna on spasm in the intestines. A patient with a growth of the pelvic colon had been given a bismuth meal the previous day, so that the whole colon was visible. He had very severe paroxysms of pain; every time he groaned one could see the whole of the colon contract, the bismuth being rapidly projected from one end to the other in an extraordinary way. It was not a peristaltic wave, but a tetanic contraction affecting a considerable portion of the gut. In the intervals between the paroxysms the lumen of the colon was rather smaller than usual. Belladonna was given in rather larger doses than Dr. Moore gave it; the pain disappeared, and the next day none of the spasmodic movements were present, and the lumen of the colon had increased to three times what it was before. There was thus a diminution in the tone of the gut, as well as disappearance of the spasmodic conditions.

Dr. NATHAN RAW said the valuable work which Dr. Craven Moore and Dr. Kilroe had done served to confirm the opinion that belladonna was of the greatest value in some forms of dyspepsia; but he wished to know whether the authors had taken into account the idiosyncrasy of any patient towards belladonna, because it was known that many people were susceptible to quite minute doses of belladonna or atropine.

Dr. HERBERT FRENCH desired to refer to what might appear to be a different branch of medicine—namely, to the bullous dermatoses, such as pemphigus, erythema bullosum, dermatitis herpetiformis, herpes gestationis, and similar skin eruptions associated with bullæ. The older view that they were of nerve origin was being given up in favour of the view that they were attributable to a poison or poisons produced inside the body. There was evidence that at least some of them were due to poisons of a ptomaine nature absorbed from the alimentary canal, as in a case which occurred recently, as follows: A family were very poor, and had some meat given to them for their Sunday dinner. The children did not like the taste of it, and refused to eat it; they said it was stinking. But the mother and father ate it. The father had acute ptomaine poisoning of the ordinary type, whilst the mother, instead of developing acute diarrhœa and vomiting, broke out into severe and typical pemphigus. He did not know whether all cases of bullous dermatoses would some day be traced to allied or similar causes, but it was of interest, in connexion with the present discussion, to recall the fact that, although many of these cases were benefited by arsenic, some of them were not, and that, as Dr. J. F. Payne had pointed out, when other remedies failed to relieve those dermatoses, small doses of belladonna would often succeed. He had tried it and found it an admirable remedy. Was it possible that the belladonna modified the secretions of the alimentary canal sufficiently to cure what was the original cause of the skin trouble—namely, the production of some toxic substance in that canal?

Sir JOHN BROADBENT asked what evidence the authors had of the existence of the spasm which they spoke of. Was the evidence a particular kind of pain after meals, or was it based upon X-ray examination? He thought that in most cases of duodenal ulcer there was some dilatation of the stomach, not only in cases of marked hypertrophy due to stricture, but also where there was duodenal ulcer without any marked thickening or constriction being evident at the operation when gastro-enterostomy was performed. He wondered whether that dilatation was due to a reflex spasmodic contraction of the pylorus at the time the food began to pass over the duodenum, owing to irritation of the ulcer. Would Dr. Moore state what evidence he had on the question of spasm? It was difficult to be sure that there was spasm at the pyloric end of the stomach, or in any particular part of it. One could say a person had colic in the intestine, but it was difficult to localize it. In certain cases of hour-glass stomach there was something of the kind, but it was difficult to be certain of pyloric spasm in duodenal ulcer without some definite evidence, such as that afforded by bismuth and X-ray examination. Another point was as to the effect of belladonna on the pepsin secretion. One would have thought that if it had a marked effect on the acid secretion, it would have a marked effect also on the pepsin secretion, and that when trying to check hyperacidity one might interfere too much with the pepsin secretion. He asked whether the dose was limited to ♏v, or whether a larger dose was given when administered to check the spasm.

Dr. KILROE said that, whilst there was nothing to add to what had been said by Dr. Craven Moore as to the experimental results, he would like to emphasize that aspect of the subject which could not be so definitely formulated—namely, the effect of belladonna on the subjective manifestations of dyspepsia. As house physician to the Ancoats Hospital he had abundant opportunity of noting the effect of the use of belladonna, and he was very positively impressed with the benefit which followed its administration in cases where there were manifestations of hyperacidity, and particularly when associated with painful spasm; even in some cases of gastric carcinoma the pain of this disease was relieved by belladonna.

The PRESIDENT (Dr. Mitchell Bruce), in thanking the authors for their papers, said he would be glad if Dr. Moore would explain on what grounds he based his diagnosis of gastric and duodenal ulcer: was it on ordinary clinical grounds or on surgical or post-mortem evidence? He also wished to ask what was Dr. Moore's final impression of the practical value of the method of treatment by belladonna. Were the cases permanently benefited by it, or had the drug to be given from time to time? He had gathered that it had to be continued: that the improved condition depended largely on such continuance. He would also like to know the relative value of the different preparations of belladonna. Some of these were notoriously imperfect, and therefore unreliable, while those skilfully prepared by good chemists were particularly active. He was interested also in the relative value of belladonna and atropine; the latter must be a more elegant drug to use than belladonna. He had never used belladonna for that purpose; what he had tried to do in order to diminish the hyperacidity of gastric contents and secretions had been to give an acid before meals, which was theoretically indicated in order to diminish the activity of the gastric juice. He was very glad to find that belladonna provided a means of controlling hyperchlorhydria; and members would be glad to hear Dr. Craven Moore's final conclusions on the matter, and whether he would continue its use in routine practice.

Dr. CRAVEN MOORE, replying to the President, said that the majority of the cases of duodenal ulcer and some of gastric ulcer were confirmed at operation, otherwise the diagnosis depended on the presence of definite physical signs and symptoms. With regard to the mode of action of belladonna, it was probable that the drug paralysed the terminations of the secretory fibres of the vagus, and in that way interfered with the psychical secretion rather than with the secondary secretion, which was of chemical origin. In a case where there was continuous secretion, it had been shown by Dr. Braham (Ancoats Hospital) that belladonna was less effective if administered on the fasting stomach which contained some active juice than if the stomach had been previously washed out. The explanation seemed to be that in the former case there was sufficient active juice to initiate digestion immediately and start the secondary secretion. With regard to the clinical value of the drug, he was quite satisfied as to its beneficial effects, and, though it obviously had no lasting influence, it was possible by its use to keep the local conditions well within the normal limits, and so allow of a gradual natural restoration of the disordered functional activity which was responsible for the patient's disability.

Medical Section.

January 24, 1911.

Dr. J. Mitchell Bruce, President of the Section, in the Chair.

On Auscultation of Joints.

By A. E. Garrod, M.D., F.R.S.

That noises are produced in joints is a familiar fact, for such sounds are in some instances so loud as to be clearly audible to bystanders, and far more often attract the notice of the persons in whose joints they are produced. The loudest articular sounds are those which have little or no morbid significance, and, speaking generally, the most noisy joints are seldom those which are the seats of the more serious forms of disease. Apart from the more conspicuous reports and crackles above referred to there are abnormal articular sounds, of much fainter character, which are only audible with the stethoscope, and, from experience gained from systematic examinations extending over some years, I have become convinced that auscultation affords valuable aid in the diagnosis of the several forms of morbid change to which the joints are liable.

The method of examination is very simple. The only apparatus needed is an ordinary binaural stethoscope, and any difficulty which may be experienced in keeping the mouth of the stethoscope accurately applied to the changing surface of a joint in movement is easily overcome by the use of a small chest-piece, or better still by affixing to the chest-piece one of the india-rubber ring coverings which are sold by instrument makers. A little practice enables one to dispense with any special chest-piece, and teaches how and where the instrument is best applied to the several articulations.

Of all joints the knees are those which best lend themselves to such examination, and no difficulty is met with in keeping the chest-piece applied over the soft parts on either side of the patella. During the examination the joint should be slowly flexed and extended, either by the patient himself or in a passive manner by the examiner. Auscultation of the shoulder-joint is also easy, and the sounds produced during rotatory movements, as well as during elevation and depression of the arm, may be alike studied. Nor is any serious difficulty met with in the application of the method to any of the larger joints, such as the elbow, wrist, or ankle. Even small articulations, such as the temporo-maxillary or the carpo-metacarpal joint of the thumb, admit of examination with the stethoscope.

I cannot pretend to give, in this paper, any complete or adequate account of the sounds heard in moving joints, nor of their diagnostic significance, for I can lay no claim to the knowledge required for the production of such an account. My aim is rather to direct attention to the value of this clinical method, and to stimulate others to employ it, in the hope that the accumulated experience of many observers may lead, in time, to a just appreciation of the meaning of the sounds and an accurate differentiation of their several varieties.

Our knowledge of the meaning of sounds heard in auscultation of the chest has been built up by comparison of clinical observations with post-mortem findings, but the qualities of the sounds are in most instances indicative of the method of their production. Thus a pericardial friction sound strongly suggests its origin, and when we hear sibilus the rush of air through narrowed passages is clearly indicated. Again, from the time and distribution of a cardiac murmur the seat and nature of a valvular lesion may be inferred, and the inference drawn during life has been so often confirmed by examination after death that there is no longer any room for doubt as to interpretation. Yet experience shows that even skilled observers are sometimes misled by clinical signs.

In the case of joints the opportunities of confirming our findings by actual examination of the parts are few, and therefore the acquisition of certain knowledge as to the meaning of articular sounds must be mainly a matter of clinical observation, checked by such information as X-ray pictures afford—information which must, in the nature of things, chiefly relate to the condition of the bony structures.

When examined with the stethoscope a normal joint of a young subject is silent. It is true that loud reports are sometimes produced in such joints and can be produced by some persons at will. Such sounds have no morbid significance and may be compared to the click produced by lifting a ball from a closely fitting socket—in other words, they are vacuum sounds.

In the joints of subjects past middle life abnormal sounds are often heard, even though the patient makes no complaint of his articulations and is conscious neither of pain nor stiffness. They may be ascribed to the changes due to wear and tear, but experience teaches that in cases of incipient osteo-arthritis abnormal sounds may precede all subjective sensations. Thus, when one knee is alone complained of, its fellow may exhibit well-marked auscultatory signs. On the other hand, silence of a joint is by no means a sure indication of health. In cases of synovitis, in which the articular surfaces are held apart by effused fluid, no sounds are heard as a rule, but when the effusion has been absorbed a pronounced crackle becomes audible at any part to which the stethoscope can be applied, and this sound, to which the name of "synovial crackle" may be applied, is one of the commonest of morbid sounds heard in joints. Long after the attack of synovitis is past, especially in cases in which the joint which was its seat is apt to give trouble after any unusual use or strain, the synovial crackle may still be audible, and, as far as can be judged on clinical grounds alone, it would appear to indicate a swollen condition of the synovial membrane. In its most characteristic form it resembles closely the crackle heard when the stethoscope is applied to a region which is the seat of surgical emphysema.

It is not only when effusion is present that diseased joints are silent. A very limited experience of auscultation of tuberculous joints leads me to believe that these are frequently, or even usually silent, and the same is also true of joints affected by the more acute forms of rheumatoid arthritis, although in the later stages the synovial crackle is usually heard in them. This is in accord with the scanty post-mortem evidence available as to the condition of the joints in the acute stage of the disease in question, which tends to show that the joints themselves are, at first, but little implicated, and that the inflammatory swelling is mainly peri-articular.

The knee-joints of patients in or past middle life, and especially of women about the period of the menopause, are often the seats of pain and stiffness, which is especially felt in kneeling, or in going up and down stairs, and for which medical advice is sought. In such joints a peculiar abnormal sound is heard, especially well when the stethoscope is applied on either side of the patella. In characteristic cases this is rather a scrunch than a crackle, and has a wholly different character from the synovial sound already described. The character of the scrunch suggests abrasion of cartilages rather than synovial swelling, and radiograms of the articulations in which it is heard usually show slight but distinct evidences of osteo-arthritic changes. I am strongly inclined to think that this scrunch is an early and important sign of osteo-arthritis. It must be mentioned, however, that cases occur in which it is difficult to decide to which class the sounds heard should be assigned, and it is more than probable that, in many instances, the synovial crackle and osteo-arthritic scrunch co-exist.

It should be mentioned that I have here employed the words rheumatoid and osteo-arthritis to designate distinct morbid conditions, which I believe to be manifestations of two distinct diseases, and that I believe the osteo-arthritic sounds to be physical signs of that process which results in fibrillation and erosion of cartilage, and the formation of chondrophytes and osteophytes.

There is another articular sound which is much less commonly heard than either of those which have already been referred to, and which can usually be felt as well as heard. It is a rubbing sound which suggests the friction of ill-lubricated surfaces upon each other, and is such as might result from the movement upon each other of eburnated surfaces of bone. However, such an explanation is not tenable, in view of the fact that, after persisting for a time, the friction may wholly disappear, and it is in the more advanced stages of rheumatoid arthritis, rather than in association with osteo-arthritic changes, that this abnormal sound is heard. Into one or other of the above-mentioned categories the morbid sounds heard in joints appear to fall for the most part.

Lastly, the friction which may be felt over inflamed tendon sheaths produces a quite characteristic noise, which must be distinguished from that produced within the articulations.

To sum up, it may be claimed for auscultation of joints that this method is capable of affording early warning of troubles in articular structures, and sometimes reveals the commencement of disease in joints which to inspection and palpation appear to be normal, and which may even cause no discomfort to their possessors. Again, the method gives promise of help in the differentiation of articular diseases, and in distinguishing between those in which the cartilages and bones are implicated and those in which the synovial membrane and soft parts around the joints alone suffer. If these claims be justified, auscultation of joints does not merely supply information which can be equally well obtained in other ways, but promises to lead to the extension of our clinical knowledge of the conditions in which it is employed.

DISCUSSION.

Dr. VOELCKER asked whether Dr. Garrod had experienced any difficulty in distinguishing sounds produced by the stethoscope on the skin, and the sounds which were actually heard within the joint. On listening to joints he had been met with this difficulty, and would like to know how the accidental sounds produced outside the joint could be eliminated.

The PRESIDENT (Dr. Mitchell Bruce) asked whether Dr. Garrod found anything diagnostic of gouty changes in the joints. His own opinion was that in a very large proportion of cases the chronic affections of joints met with in ordinary practice were really gouty. Also, would Dr. Garrod say whether he had made any observations which would enable one to diagnose gout in distinction from the popular "rheumatism" and the very popular "rheumatoid arthritis" or "osteo-arthritis" which were so often diagnosed, especially by surgeons, and which often ended favourably in a short time. It was useful to know something of the pathological anatomy of an affected joint, but it was still more important from the prognostic and therapeutical points of view to know the nature of the disease.

Dr. GARROD, in reply, said that he had never found sounds produced by friction of the stethoscope upon the skin to cause any serious inconvenience in the auscultation of joints. A little practice enables one to dispense with any special chest-piece, and to dismiss any extraneous sounds. He was unable to say whether or not any abnormal sounds were heard in a joint the seat of acute gout, nor had he recognized any special auscultatory sounds, which might help to diagnose a gouty origin of more chronic articular troubles.

Absence of Abdominal Respiratory Movement as an Indication of Pericarditis.

By W. Essex Wynter, M.D.

In a disorder so serious and attended with so little obvious indication as pericarditis, an additional and manifest physical sign, provided it is to be trusted, should be especially welcome. The sign suggested in this communication is inhibition of the action of the diaphragm, indicated by suppression of normal abdominal respiratory movement, and has probably been hitherto overlooked through examination being unduly restricted to the chest in such cases. Loss of abdominal respiratory movement as an indication of disease, except in paralysis of the phrenic nerves, has been to a great extent monopolized by observers in connexion with peritonitis and acute abdominal lesions. It is largely from this standpoint that Dr. William Pasteur dealt with this phenomenon and the resulting thoracic lesions in his highly instructive Bradshaw lecture.[1] Primary thoracic disease, however, has its share in causing suspended function of the diaphragm, though in the majority of instances affecting the lung and pleura this is one-sided, and the influence on abdominal movement proportionately less marked. In acute pericarditis, on the other hand, the cessation of phrenic action is bilateral, and consequently absence of abdominal movement is as obvious as in abdominal disorders, albeit other abdominal manifestations, such as muscular rigidity and general tenderness, are lacking.

The two instances I wish to mention first bring out this point with great force, in that the loss of abdominal movement led to mistaken diagnosis of abdominal lesions, and was only ascertained subsequently to indicate pericarditis, the observation being therefore entirely impartial.

The former of these, seen in October, 1905, was that of a young Irishman in the service of a medical friend, who had been seized with acute epigastric pain while waiting at table. Conspicuous absence of abdominal movement led to the supposition that he had a perforated gastric ulcer, and I was asked to take him into the Middlesex Hospital for operation. The observation and inference had been made by an independent local practitioner, and was quite disinterested as regards any connexion with pericarditis, which was not thought of at the time. When I saw the man next morning the acuteness of the pain had diminished, he had enjoyed some sleep, and was obviously better, clamouring for food, and impatient of restraint. There was no evidence of free gas in the peritoneum or of local irritation, and it was considered best not to move him. A routine examination revealed nothing. Next day he got up and went out of his own accord, but experienced a recurrence of his pain. When re-examined on the fourth day, pericardial friction was quite obvious, and I received him under my own care in a medical ward. By that time there was marked dyspnœa with præcordial pain, the abdomen remaining immobile. Pericardial friction was audible four days. Temperature 101·4° F., pulse 96. On getting up a week later the temperature rose to 103° F., and the pulse to 104, but no further development occurred, and he was able to leave the hospital ten days later, after a stay of just three weeks.

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[1] The Bradshaw Lecture on Massive Collapse of the Lung," 1908.

The second case was somewhat similar, except that I shared with a surgical colleague a mistaken diagnosis. On November 2, 1909, S. W., a man, aged 40, was admitted with complaint of pain in the abdomen extending into the thighs, headache, anorexia, vomiting, and parched mouth of two days' standing. The temperature was 102·6° F., pulse, 92. He lay on his back with the legs drawn up, the right rectus abdominis was tense, and there was tenderness in the right iliac region. Abdominal respiratory movement was scarcely discernable. A routine examination of the state on admission was made, but nothing more definite could be found than a reduplication of the second cardiac sound. No local indication of pericarditis then existed. A consultation was held, and the almost inevitable conclusion arrived at that the case was one of appendicitis. Symptoms subsided with rest in the course of the next three weeks. Abdominal movement was more apparent and advantage was taken of this quiescent period for surgical interference. Five weeks after the original attack, December 7, appendicectomy was performed ; but careful examination revealed no morbid condition, and there were no adhesions. The incision healed rapidly, but after five days there was a rise of temperature with recurrence of symptoms, and on December 12 pericardial friction was well marked, the heart's apex being in the fourth space in the nipple line. Pulse, 90 to 100. The diaphragm was not contracting at all, the abdomen actually sinking in with each inspiration. The stomach was dilated, and its resonance could be traced as high as the fourth intercostal space in the axilla. Breath sounds were absent at both bases, but there was no complete dullness on percussion. On December 16, Mr. Cecil Lyster supplied a report under X-ray screen examination to the effect that there was a large shadow in the heart region suggestive of pericardial effusion. The diaphragm was motionless in the position of expiration. On December 22, diaphragm movement was returning, though friction could still be heard over the heart. Breathing became audible at both bases. A week later friction had disappeared, but a systolic apex murmur was audible. The man was discharged on January 8.

Both these cases afford examples of defect or absence of abdominal movement being observed quite independently of any idea of pericarditis, which only manifested itself by the usual clinical signs later. They also illustrate temporary remission of symptoms attending rest in bed without special treatment, and suggest that inflammation of the pericardium, possibly commencing in some other part than that immediately in contact with the chest wall, indicated its presence by reflex inhibition of the diaphragm some days before the appearance of ordinary signs.

On December 1, 1905, W. S., male, aged 21, with a history of rheumatism six years, and also three months before, was admitted with endocarditis and pericarditis. Temperature 102·2° F., pulse 130, respiration 36. There was a double aortic bruit, and in addition, the bruit de galop and friction were heard over the præcordia. Abdominal respiratory movement was absent. By December 4 these active signs subsided and there was a return of abdominal movement, but joint pains continued till December 11. He was discharged a month later, January 12.

On September 22, 1906, W. A., male, aged 9, was admitted on account of pain in the epigastrium, of one week's duration, extending to the left clavicle. Tonsillitis had also existed five days. Temperature 102·6° F., pulse 120. The boy lay with his arms above his head, flushed and breathing rapidly. The præcordial dullness was extensive and described as pear-shaped. Heart sounds were muffled. There was a faint mitral regurgitant murmur, with accentuation of the pulmonary second sound. Friction could be heard all over the pericardium, but was loudest over the fourth costal cartilage and sternum. There was tenderness on pressure in the epigastrium, and the abdomen moved very little with respiration. There was slight rigidity of the recti abdominis. By October 2 friction had subsided, but there was a recurrence of tonsillitis and he was not discharged till November 10.

On June 6, 1907, D. G., a girl, aged 16, was admitted suffering from rheumatism and tonsillitis of a fortnight's duration. She improved speedily, but on getting up there was a relapse, with pain in the epigastrium. Temperature 102° F., pulse 120, respiration 50. Præcordial dullness extended laterally 1½ in. to the right of the sternum, outside the nipple line to the left and upwards to the third rib cartilage. Pulsation was felt in the third left intercostal space. The observation on abdominal respiration was that "when the chest rises in full inspiration the abdomen falls in, and vice versa." She was discharged convalescent on June 26.

On June 14, 1907, C. B., a boy, aged 6, who had been ill five weeks, was admitted with præcordial pain. Temperature 103° F., pulse 144, respiration 60. There was definite extension of præcordial dullness, and the heart's impulse could be felt in the third and fourth left intercostal spaces. The diaphragm did not move with respiration, and breath sounds were indistinct at both bases. The boy made a good recovery and was discharged on July 3.

On October 5, 1907, F. K., a boy, aged 13, was admitted with joint pains of fourteen and præcordial pain of five days' standing. He had rheumatic fever twelve months and three months ago. Temperature ranged from 102 F. to 104 F., pulse 130. No endocardial murmur was heard nor any friction. Three days later marked immobility of the abdomen was noticed. There was some extension of præcordial dullness 1 in. to right of sternum. The apex was in the fifth space. Soft pericardial friction could be heard, which continued for ten days, the temperature reaching 101 F. A month later, with a relapse friction reappeared, November 17, and lasted five days. He was discharged December 2.

During the past year, 1910, which was prolific in rheumatic affections, five cases occurred in which notes were made concerning the state of the diaphragm, with three screen examinations :—

On March 22, L. D., a girl, aged 12, was admitted with inflammation of an adherent pericardium. The heart had been affected in an attack of rheumatism and chorea at the age of 9. For three weeks there had been pain in the front of the chest, with cough, vomiting, and dyspnœa. The apex was 2 in. outside the nipple line in the sixth space, the impulse extending over a large area of the præcordia with systolic retraction, and there was a mitral regurgitant murmur. The aortic second sound, though loud at the base, was almost inaudible at the apex. The pulmonary second was accentuated. The heart apex was anchored, not shifting at all with change of position. Pulse 146. Some friction was heard at intervals over the heart. The diaphragm was completely inactive. Resonance was impaired at both bases posteriorly, where breathing was scarcely audible. At the end of April there was an intercurrent attack of diphtheria, and she was not discharged till May 24, having been in the hospital just over two months.

On May 16, L. B., a man, aged 21, who had left the hospital only one week before, after rheumatism, from which he had suffered on three previous occasions, was found to be breathing without his diaphragm. Friction was heard over the base of the heart, the apex was in the nipple line, and there was a systolic apex murmur. Temperature 102° F., pulse 120, respiration 30. Five days later (May 21) the diaphragm was beginning to act, though friction was still audible at the cardiac base. He was discharged convalescent on June 14.

On June 7, D. J., a girl, aged 7, was admitted with rheumatism of a fortnight's duration. She suffered from scarlet fever and chorea at the age of 6. Dullness extended between the right and left nipple lines, laterally and vertically from the second rib cartilage to the ensiform. Friction could be heard at the base and there was a systolic apex murmur. Temperature 103 F., pulse 136, respiration 64. Abdominal movement was well marked. Fever lasted till June 21. On June 25 a screen examination was made, showing that the area of cardiac shadow was very large, but only the middle portion showed pulsation, indicating the presence of effusion. Contraction of diaphragm was *normal*. A second examination before the child left on July 8, a month after admission, showed pulsation extending to the full limit of the cardiac shadow, indicating that the effusion had cleared up. The heart shadow was still very large (dilatation and adherent pericardium).

On June 22, F. B., a boy, aged 14½, was admitted with rheumatism and mitral regurgitation. He suffered from rheumatism and scarlet fever three years before. Five days after admission (June 27) friction was detected over the heart, chiefly at the base, and lasted three days. Examination by the screen showed an enlarged heart, the whole area pulsating, so it was concluded there was no effusion. The diaphragm moved very slightly, estimated at ¼ in. A temperature of 103° F. to 102° F. continued for the first five days in spite of salicylates internally, but fell on June 27, when salicylates were applied to the chest.

On July 13, G. T., a youth, aged 17, was readmitted twelve days after being in the hospital for rheumatism, on account of chest pain and dyspnœa which had developed subsequently. Temperature 100° F., pulse 110, respiration 40. The apex was in the sixth space. Nipple line: There was a systolic apex murmur and pericardial friction was audible at the base. A screen examination showed that the cardiac shadow occupied the middle third of the chest. The shadow extended outside the pulsating area on the upper and right borders, indicating effusion. The diaphragm moved slightly on the left side and not at all on the right. He was discharged convalescent on August 19.

A dozen cases afford but a limited basis for extended inference, and these do not include all that came under observation, but with frequent change of assistants, it is a difficult matter to obtain uniform notes, and in several the particular point was not alluded to, showing that it is not, at present, a prominent feature in medical teaching. There is no doubt, however, that when attention is directed to the matter, and a definite note made as to the presence or absence of abdominal movement, this feature will be found fairly constant in acute pericarditis, especially that with fibrinous exudate. When the front of the body is fully exposed, the stillness of the abdomen is very striking, suggesting peritonitis, unless, as sometimes happens, there is inspiratory recession, which might mislead, unless the want of correspondence with the movement of the upper chest is noticed. The inertness of the diaphragm is not merely inferred, but by X-ray screen examination can be actually observed, the midriff mounting high into the thorax, and exhibiting no contraction, and in some instances an actual rising of ¼ in. with each inspiration. In one case the contents of the upper abdomen, such as the kidneys and opaque masses in the intestine, could be seen to participate in this upward inspiratory movement.

Concomitant and indeed consequent phenomena are a tendency to dilatation of the hollow abdominal organs, especially the stomach, collapse and loss of function in the lower lobes of the lungs, slight upward displacement of the heart, and of the organs beneath, and in contact with the diaphragm. Loss of abdominal movement, as a sign of pericarditis, is the more valuable, in that it may precede and outlast the other indications, and so afford grounds for anticipatory or retrospective diagnosis. The condition affords a reasonable explanation of the cyanosis and dyspnœa so usual in the complaint, and of the upright position so commonly assumed by the patient. The former expressing the effect of respiration restricted to the function of the upper lobes of the lungs, and the latter pointing to an unconscious effort on the part of the patient to utilize the weight of the abdominal viscera to depress the passive diaphragm and enlarge the thoracic area.

Restriction of abdominal movement is apparently more marked in fibrinous than in effusion pericarditis, movements being more productive of local irritation in the former than in the latter condition, as is the case in other serous sacs, such as the pleura and peritoneum.

Incidentally, the observation of the existence of a limited amount of serum in at least one of the cases by X-rays, in the absence of other clinical indications, suggests that extended observations may show this to be more frequent than was formerly supposed, for in the recumbent posture a few ounces of fluid would naturally gravitate to the lowest part of the sac, and scarcely reveal its presence over the front of the heart.

So far these observations have been restricted to rheumatic pericarditis, mostly associated with polyarthritis, though some have been independent. The paucity of screen observations has been due to the comparative infrequency of the cases, and the inadvisability of disturbing patients so seriously ill, especially in the acute stage. It is, however, the conviction of the writer that where loss of abdominal respiratory movement is well marked in the absence of more obvious thoracic and abdominal lesions, pericarditis should be closely looked for, and where this sign is wanting dubious indications of pericarditis may be discounted. As none of the cases cited proved fatal, or required surgical operation, this aspect of pericarditis was unsuitable to the recent discussion on the subject at this Section.[1] When the matter first presented itself, a reference to current descriptions of the disorder showed that the feature described was not generally recognized, but it has since been pointed out to me that Gibson, in his text-book published in 1901, makes this statement: "Interference with the action of the diaphragm is sometimes observed, and leads to discovery of pericarditis. It is obviously due to reflex action, and may, or may not, be associated with painful sensations." Commenting on this, I should say that, since I was aware of it, I have not seen an acute case which failed to show this sign, that it is, perhaps, less obvious in effusion, and not present in adhesion of the pericardium. But that its importance as an aid to diagnosis, especially in the more obscure cases, has been understated, and generally unappreciated, future observations will, I am sure, demonstrate.

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[1] *Proceedings*, 1910, iii, pp. 55-136.

DISCUSSION.

Dr. PERCY KIDD said he had noticed abdominal rigidity in cases of pericarditis, but he could not say he had looked for it constantly. He would, however, do so now. Dr. Hutchison, in conversation, had raised the question whether the rigidity was present in every case of pericarditis, and thought that possibly where the diaphragmatic surface was chiefly affected one might expect to find inhibition of the diaphragm. He had several times seen a very motionless abdomen, probably due to the pain produced by movement.

Dr. M. A. CASSIDY said he was very glad to hear Dr. Wynter lay stress on the fact that pericarditis might be confused with acute abdominal lesions, because at least twice during the last three years he had seen cases sent to hospital as acute abdominal lesions, which subsequently turned out to be pericarditis. One of the cases he considered was of sufficient interest to justify a short description. The patient, a girl, aged 20, was admitted to Dr. Mackenzie's ward in St. Thomas's Hospital on October 15, 1909. She gave a history of anæmia and dyspepsia, but had never had rheumatism. A fortnight before she was laid up by "influenza," with tonsillitis and pains in the limbs, and three days before admission she had a sudden attack of severe abdominal pain, chiefly epigastric, associated with frequent vomiting. Her medical attendant diagnosed a perforated gastric ulcer, administered morphia, and sent her up to the hospital for immediate operation. On admission her temperature was 101·6 F., pulse 104, respiration 32. She looked ill, but her general appearance was not suggestive of peritonitis. The abdomen was almost motionless, respiration being entirely costal in type. The abdominal wall was tender and resistant everywhere, but especially over the epigastrium. There were no signs of peritoneal exudate and no abdominal distension. The liver dullness was normal in extent. The cardiac dullness was normal on admission; there was a soft basal systolic murmur and a rough apical systolic murmur, not conducted. The tongue was moist and furred. In this case the difficulties of diagnosis were increased by the fact that morphia had been administered before the patient came to the hospital. The question of immediate laparotomy was seriously considered, but eventually it was decided to await further developments. For several days her symptoms and physical signs were entirely abdominal, and it was not until some ten days after admission that the rheumatic origin of the case was suspected. The area of cardiac dullness was found to increase in extent, and at the same time bruits suggestive of pericardial friction made their appearance. A fortnight after admission there were signs of pulmonary collapse at both bases, and the cardiac dullness extended from the second rib and from 1 in. to the right of the sternum to the left fourth intercostal space, ½ in. outside the nipple line. X-ray examination revealed an enormous heart, or pericardium, the departure from the normal dimensions being chiefly in the transverse direction. The diaphragm was elevated and almost motionless, and the base of the left lung was opaque. The pyrexia and other symptoms responded promptly to the administration of salicylates, and a further confirmation of the diagnosis was afforded by the appearance of a crop of typical rheumatic nodules on December 3.

Dr. NATHAN RAW said he was not aware of the symptoms which had just been so ably described in ordinary cases of acute pericarditis; but on receiving notice of the paper, he had observed in hospital a girl, aged 18, who had a very acute attack of rheumatism, and who, a week after the onset, developed acute pericarditis. The abdomen was quite fixed, the recti muscles were tense, and on ordinary respiration the abdomen was practically motionless. She had very severe pain referred to the præcordial region, but whether that pain was the cause of the almost absolute fixation of the diaphragm and abdominal muscles he could not be quite certain. After an injection of morphia to relieve the acute pain, the movement of the abdominal muscles was easier. He was glad to be able to confirm, in that one case, Dr. Wynter's observation.

Dr. R. HUTCHISON said he was much interested in the paper because, a month ago, he saw a case very much like that described by Dr. Wynter, one exactly like an abdominal case. He thought everyone would have called it a case of "acute abdomen." The patient, a boy, was lying with the legs drawn up and with a rigid belly, breathing rapidly, the respiration being very shallow, and the diaphragm not moving. He was certain the boy had no evidence of acute heart trouble, and the case was thought to be one of peritonitis. A surgeon saw him, but decided to wait until the following day before doing anything. Next day the boy had developed a very loud pericardial friction sound. The rigidity of the abdomen did not seem to occur in all cases of pericarditis. He had had two other cases in children of about the same age at the same time as the above case, who also had pericardial friction well marked, but neither of them had the immobility of the diaphragm to anything like the same extent. He asked Dr. Wynter why it occurred in one case and not in another. He had just suggested to Dr. Kidd that the phenomenon occurred when the diaphragmatic surface of the pericardium was involved. There was a curious interval in some of the cases described.

sometimes amounting to days, before the pericardial signs showed themselves, and that fact was difficult to explain. The Section would feel much indebted to Dr. Wynter for bringing the matter prominently forward, for it appeared to have been largely overlooked in text-book descriptions.

Dr. VOELCKER desired to follow Dr. Hutchison in what he had said concerning the absence of abdominal rigidity in cases of pericarditis. A short time ago he had under his care at Great Ormond Street Children's Hospital a child who had pericarditis, but who had definite free movement in the upper part of the abdomen. He mentioned this case because Dr. Hutchison threw out the suggestion that the restriction of movement might be met with where the diaphragmatic surface of the pericardium was affected. In the case referred to, although the clinical picture pointed towards pericarditis, the only place in which friction sounds could be heard was in the subcostal angle, and this would point to the existence of pericardial roughening on its diaphragmatic surface. A point referred to by Dr. Wynter was the question of pulsation. In one of Dr. Wynter's cases examined by the screen he believed that pulsation was observed in the central part, and from that it was inferred that effusion was present. The presence or absence of pulsation on screen examination he had looked upon as a point in the differential diagnosis between a dilated heart and the presence of pericardial effusion, because as a rule in pericardial effusions there was an absence of the pulsations while in the dilated heart the screen showed a pulsating shadow. Two days ago Dr. Wynter saw with him in Middlesex Hospital a child in whom there was absence of movement in the upper part of the abdomen and in whom the screen showed a very definite shadow without any pulsation. Pulsation was very important in differential diagnosis, because everyone knew that the distinction, by the usual methods of physical examination, between dilated heart and pericardial effusion, however marked it might be in books, was not always simple at the bedside. He once saw a case which, unfortunately, was too bad for the surgeon to operate upon in order to release the heart from the adhesions which were thought to be binding it down. Post mortem, however, no pericarditis was found; it was simply a case of dilated heart; so that it was possible for skilled observers to make a mistake between pericardial adhesions and effusion and a dilated heart. With regard to mistaking pericarditis for abdominal lesions, one sometimes saw the opposite side of the picture. Last Christmas a child was admitted into Middlesex Hospital and was regarded as an obscure case of either pericarditis or pneumonia. There was absence of abdominal respiratory movement, and there were no definite signs in the chest. Movement, though slight in the upper part of the abdomen, was very limited in the lower part of the abdomen, and a diagnosis of appendicitis was afterwards made. At the operation a perforated appendix with general peritonitis was found.

Dr. FAWCETT said he quite realized the value and importance of Dr. Wynter's observations, but at the same time he advised caution in diagnosis in the early stages of a case which at first sight appeared to be an "abdominal" one. When the physical signs of pericarditis appeared only after a considerable interval, as, for example, in the first cases quoted, it might possibly be that the deficient movement of the diaphragm in the commencement of the illness was after all due to a localized peritonitis, the evidence of which became less obvious as the pericarditis developed. Probably the President could recall cases such as one that had recurred to the speaker's mind, where a child was admitted to hospital with signs suggestive of peritonitis in the upper part of the abdominal cavity, but where the development later of pericarditis and pleurisy was thought to explain the early abdominal signs. Nevertheless, the autopsy showed that there was a recent peritonitis in addition, leaving little doubt that to this was to be ascribed the initial abdominal condition. In this group of cases a bacterial infection of more than one serous membrane occurred, for example, as in an invasion by the pneumococcus, where the evidence of infection of the peritoneum might be much less certain than that of the pleura, or pericardium, and these cases must always be borne in mind.

The PRESIDENT (Dr. Mitchell Bruce) desired to ask two questions. First, was pleurisy present in any of the cases, especially the rheumatic cases? It seemed remarkable that there should have been so many cases of rheumatic pericarditis without any reference to pleurisy, because it was common to find pleurisy in rheumatic children. Also, did Dr. Wynter believe that the high position of the diaphragm in inspiration accounted for the situation of the præcordial impulse in acute pericarditis? It was well known that the præcordial impulse in acute pericarditis with effusion lies upwards and outwards from the normal; and it long had been matter for discussion why the præcordial impulse should be found there. The point raised by Dr. Fawcett appealed to him; it had occurred to him that the first two of Dr. Wynter's cases were abdominal cases, genuinely so from the first, and an inflammatory process in the abdomen might have spread through the diaphragm into the pericardium. Happily, both ended in recovery, so the diagnosis could not be tested. The other cases were unquestionable, because they were ordinary rheumatic cases.

Sir JOHN BROADBENT asked about the association between pleurisy and pericarditis in these cases. Recently he was asked to see a case in the wards in which there was very marked immobility and extreme rigidity of the abdomen. But on deep inspiration the lower part of the abdomen moved: it was a spastic rigidity rather than the kind usually met with in peritonitis. It proved fatal, and there was diaphragmatic pleurisy in addition to pericarditis. He asked whether, in Dr. Wynter's cases of pericarditis, there was not, on deep inspiration, some movement of the lower part of the abdomen, though the upper part might be held rigid.

Dr. WYNTER, in reply, said there was no movement in the abdomen at all. He did not see how, mechanically, there could be movement of the lower abdomen unless it were an inverse action of retraction. It was not true paralysis of the diaphragm, but suspended function. Of course, he did meet with cases complicated with pleurisy. But he had eliminated them because he knew that if he brought forward cases in which there was pericarditis and pleurisy it would invalidate the argument, for everyone knew that basic pleurisy arrested the action of the diaphragm on its own side. But he had a case which had pericarditis first, followed by left, and then right, pleurisy, which recovered. He thought such a case was too mixed to be admissible in support of the argument he brought forward. Screen examinations showed that with the rise of the diaphragm there was some elevation of the heart, though only slight, and in general one did not see much difference in the position of the apex in fibrinous pericarditis. In the larger effusions the heart was floated up and somewhat rotated. He felt very grateful to the other Fellows who had supplied confirmatory evidence of the importance of the connexion between loss of abdominal movements and pericarditis. He did not think that what Dr. Hutchison observed with regard to the interval between the primary attack and the recognition of pericarditis invalidated the point at all, but that it rather substantiated the suggestion made that probably the arrest of abdominal movement was due to involvement of the diaphragmatic surface of the pericardium; and it seemed reasonable that if the pericarditis began on the diaphragmatic surface, the loss of abdominal movement would precede the recognized friction over the surface of the heart, which would develop later. Of course, it was possible for peritoneal troubles to extend through and infect the pericardium, or for both to be due to the same infection. But he thought the second case, in which a mistake in diagnosis was made, was rather sound evidence against that, because the man had had his initial symptoms five weeks before laparotomy was done. At that laparotomy an investigation of the abdomen showed that it was normal. It might be suggested that the occurrence of peritonitis five days later was the outcome of the laparotomy, but that would be unkind to the surgeon, and it was not borne out by the result of the case, because on treatment by salicylates it subsided in three days. Also one had to explain the initial symptoms, and there was nothing in the abdomen to explain them. He was not prepared to say that every case of pericarditis would present that symptom, but he had a fair list of cases which showed an exaggerated degree of it, and, if looked for, he thought it would be found more constantly. Fluid in the pericardium produced pressure, but it freed the surfaces from irritation; and he thought it was found in peritonitis that there was much more restriction while the exudate was comparatively dry than when it was more fluid. He disclaimed any originality for the subject, but he did not think it had hitherto been put forcibly enough.

Medical Section.

February 28, 1911.

Dr. J. Mitchell Bruce, President of the Section, in the Chair.

Some Observations on Low Blood-pressures.

By W. Edgecombe, M.D.

For some years past I have been struck by the number of cases which have come under my care suffering from no disease in particular, but complaining of general slackness and want of tone, in whom the blood-pressure was abnormally low, the systolic reading being frequently below 100 mm. Hg. In the majority of such cases there is no heart lesion discoverable, but the first sound at the apex is usually lacking in vigour; in others there are definite signs of dilatation. Improvement in their subjective sensations and objective physical signs is almost always accompanied by a rise in blood-pressure, and this has led me to seek for the best and most certain means of procuring this result. With this end in view I have made a long series of observations, extending over two years, on a subject, aged 40, normal in every respect, but having a persistently low blood-pressure. The results have been supplemented by serial observations on another subject, in addition to routine observations on patients.

Method.

The method adopted has been to take a reading morning and evening, day after day, at approximately the same hour, under the same conditions as to posture, and with the same instrument, except where otherwise stated (Oliver's compressed air manometer), so that the results should be comparable. The systolic pressure alone was taken; the so-called diastolic pressure being rejected as fallacious owing to the large personal equation the reading of it involves. The subject followed his ordinary life, but special circumstances, such as exercise, extra mental work, excitement, worry, fatigue, &c., were noted and their effects observed.

The results of the long series of observations have been examined to determine the average level of blood-pressure, both morning and evening, and the average daily change, to serve as a basis from which might be noted deviations consequent upon special procedures. The effect of various drugs has been tried, and, as the chief object was to test whether they produced a *sustained* rise in blood-pressure, the method of taking morning and evening readings was preferred to that of observing the *immediate* effect of the drug; the latter, however, was also done in certain instances.

BASIS FOR COMPARISON.

The night and morning readings of the systolic pressure of the test subject for 200 consecutive days gave the following results:—

TABLE I.—TWO HUNDRED DAYS CONSECUTIVE READINGS.

	mm. Hg.	Pulse-rate
Average morning pressure	94·1	66·1
,, day rise	10·8	8·8
,, evening pressure	104·9	74·9
., night fall	9·5	—

The average *morning* reading, taken before breakfast after bathing and dressing, was 94·1 mm. Hg.; the average *evening* reading, taken about 11 to 11.30 p.m., was 104·9 mm. Hg., showing an average rise during the day of 10·8 mm. Hg. The morning pressure was lower than the evening in 92 per cent. of observations; the two were equal in 4·5 per cent., and the morning pressure was higher than the evening on only 3·5 per cent. of occasions. The normal condition, therefore, in the test subject is an average day rise of 10·8 mm. Hg.

The maximum *morning* reading was 110 mm.; the minimum, 80 mm. The maximum *day rise* observed was 25 mm.; the minimum, 0·5 mm. Hg. The maximum *evening* reading was 120 mm., and the minimum, 85 mm. Hg. Conversely there is a fall of blood-pressure during the night averaging 9·5 mm. Hg. This fall occurred in 92·5 per cent. of observations; the readings were equal in 2 per cent., and a rise of pressure took place only in 5·5 per cent. of nights observed, so that the normal condition in the test subject is a night fall of 9·5 mm. Hg.

The maximum night fall noted was 30 mm.; the maximum night rise, when a rise occurred, was 8 mm. Hg.

It will be noticed that the average day rise (10·8) is somewhat greater than the average night fall (9·5), a difference which is accounted for by the fact that more uniform conditions obtain during the night, contrasted with the varied disturbing influences present in the daytime.

The cause of the day rise in systolic pressure is increased heart action resulting from stimuli of various kinds applied during the day, and not primarily to increased peripheral resistance; for the vaso-motor tone, as measured by the relation between the venous and arterial pressures, is highest in the earlier hours of the day, and becomes less towards evening. The fall in pressure that would otherwise result from diminished vaso-motor tone is more than counterbalanced by increased cardiac output, and a rise in pressure takes place.

PULSE-RATE.

At each observation of the blood-pressure the pulse-rate was noted, and the record has been examined to see if there is any definite correspondence between the two. The average pulse-rate at the morning observations (200 in number) was 66·1, and the average evening pulse-rate 74·9, showing a day rise of 8·8 beats on the average, so that a rise in the pulse-rate corresponds to some extent to a rise in systolic blood-pressure. This does not, of course, always hold good. Numerous examples may be picked out when a pulse-rate equal night and morning occurred with very different pressures, or when the pulse-rate is high the pressure is low, and the reverse. For example:—

TABLE II.

	a.m. pressure	Pulse	p.m. pressure	Pulse
(1)	85	66	110	70
(2)	90	60	107	60
(3)	97	64	100	110

No. 1 shows a difference of 25 mm. Hg. in pressure and only 4 in pulse-rate; No. 2 shows the same pulse-rate, but a difference of 17 mm. in pressure; No. 3 shows a difference of 46 in pulse-rate, but of only 3 in pressure. This is well known, and illustrates the extraordinary adaptability of the mechanism for maintaining an even level of blood-pressure under varying conditions of heart action.

DETAILED DAY OBSERVATIONS.

On several separate days observations were made at intervals through the day to mark the oscillations of blood-pressure, and to note the period when the day rise takes place. The following is a typical example :—

TABLE III.—ORDINARY DAY (OCTOBER 21).

a.m.		Pressure	Pulse
7.20	In bed, recumbent	80	60
7.30	After two small cups of tea	85	64
8.0	In bed, recumbent	85	56
8.15	Up, sitting	90	56
8.45	Dressed, before breakfast	88	60
9.5	After breakfast (bacon, egg, coffee)	95	72
9·30	After a pipe	101	80
9.45	Still smoking	103	80
10.30		100	72
p.m.			
12.30		95	64
1.0	After a smoke before lunch	93	66
1.45	After lunch (fish, rice pudding, cheese, water)	98	72
2.0	Smoking	95	72
4.0	Before tea	95	72
4.15	After two small cups of tea	103	66
5.30		95	72
7.0	After a game of billiards, before dinner	103	84
8.0	After dinner (fish, meat, sweet, savoury, dessert, coffee, cigarette)	99	90
9.0	After a pipe	105	96
11.0	Smoking all evening	105	84
11.45	After a whisky and soda	97	78

Note the rise in pressure after morning tea and after afternoon tea; the rise with change of posture on getting up; the rise after the morning pipe and after-dinner pipe; the absence of further rise after continued smoking in the evening; the rise after the excitement of billiards; the fall after alcohol at night.

The usual sequence of events is: a rise in pressure of about 6 mm. after getting up out of bed; a further rise after breakfast, partly the result of the food and liquid ingested, but mainly dependent on whether tea or coffee be taken with the meal. If tobacco be smoked after breakfast a further rise takes place. (The effect of tea and tobacco are dealt with more in detail later on.) The level of blood-pressure reached after breakfast is maintained, with varying oscillations depending on the circumstances of the day, until the evening, so that the day rise occurs mainly in the morning soon after breakfast. If, however, neither tea nor coffee be taken, nor tobacco smoked, the rise after breakfast is minimal—2 to 3 mm. only, and the level of pressure remains fairly constant throughout the day, provided other sources of heart stimulation be avoided.

EXERCISE.

The effect of exercise appears to be an increase in extent of the day rise, this increase being roughly proportionate to the severity of the exercise taken. For example, the average day rise of eighty days (not consecutive) on which *no exercise* was taken amounted to 9·2. This, it will be noted, is somewhat less than the average rise (10·8) of the 200 days, which include all observations (except those relating to altitude, to be mentioned later). The average rise of twenty-one days on which *moderate exercise* was taken reached 11·6. (By moderate exercise is meant a fair walk of three or four miles, a cycle ride, or golf not exceeding one round.) The average rise of eleven days on which *considerable exercise* was taken—two rounds of golf or its equivalent—amounted to 13·5 mm. Hg.

TABLE IV.

	a.m. pressure	Day rise	p.m. pressure
Average of 80 no-exercise days	94·5	9·2	103·7
,, 21 moderate exercise days ...	96·2	11·6	107·8
,, 11 harder exercise days ...	93·5	13·5	107·0
,, 200 "all in" days	94·1	10·8	104·9

The average *night fall* following exercise days is greater than the normal fall, but not so great as the day rise of the exercise days themselves; consequently the level of pressure tends to be higher the following morning. For example: The average of twenty-one moderate exercise days—morning pressure, 96·2; day rise, 11·6; evening pressure, 107·8; subsequent night fall, 10·2; average pressure the following morning, 97·6. The *immediate* effect of exercise, as is well known, is a rise in blood-pressure due to increased cardiac output; and this is followed during or shortly after cessation of the exercise by a fall due to relaxation of the peripheral circulation. The *secondary* effect, as shown by these results, is an increase in the normal day rise; a marked increase in the evening level of pressure on the day of the exercise; an increase, but to a less extent, of the following night fall; and a slight rise in the morning level of pressure on the day following the exercise.

Thus the general tendency of exercise in the subject of low blood-pressure is to raise the permanent level, a result due, doubtless, to increased power of contraction of the cardiac muscle. The value of judicious exercise in improving the vigour of a toneless circulation is thereby demonstrated. Also, however—if these results be applicable in any degree to the subject of high blood-pressure—they emphasize the recognized need for caution in the use of exercise—at least of strenuous exercise—in this condition. For the immediate temporary rise induced by exercise throws a further strain upon heart and vessels already subject to a continuous abnormal stress. The secondary more enduring rise, if it occurred, would show that the reserve power of the heart was not exhausted; but would mean a not inconsiderable tax on that reserve power. If it did not occur the inference would be that the reserve power was insufficient and a serious risk of dilatation would ensue if the exercise were continued.

MENTAL WORK.

Mental work is known to exert a powerful influence in raising the blood-pressure. This effect is strikingly shown by the following results: The average day rise of ten days on which extra hard mental work was performed, involving keen and prolonged concentration, amounted to no less than 17·6 mm. Hg., which, compared with the normal average rise of 10·8 mm., is remarkable. The maximum rise noted was 25 mm. The succeeding night fall, 16·9 mm., is less than the day rise of the "mental" days; therefore the average level of pressure is higher the following morning.

TABLE V.

Average	morning pressure, mental days	95·9	mm. Hg.
,,	day rise, mental days	17·6	,,
,,	evening pressure, mental days	113·5	,,
,,	night fall following mental days...	16·9	,,
,,	morning pressure, next morning	96·6	,,

This result shows the importance of mental work, especially if combined with worry and excitement—the effect of which is shown below—as a factor in the production of high blood-pressure, and, if long continued, of arteriosclerosis; and emphasizes the need for the withdrawal of such influences in the treatment of patients suffering from advanced arterial changes.

If the converse be true, then the subjects of low blood-pressure must, by the confession of their vessels, be dull dogs indeed! Broadly speaking, however, in all seriousness, I am inclined to consider from my observations that an individual's mental activity is roughly proportionate—within certain limits—to the height of his blood-pressure. Happily though, there are exceptions to this generalization.

EXCITEMENT AND WORRY.

The effect of excitement and worry in the test subject was, curiously enough, almost identical with that of hard mental work. The average of five days on which unaccustomed excitement occurred—such as attending a public meeting and making a speech, or acute worry from private reasons—give a day rise of 17·6 mm. Hg. and a maximum rise of 25 mm.

TABLE VI.

Average a.m. pressure, excitement days	94 mm. Hg.
,, day rise, ,, ,, ...	17·6 ,,
,, p.m. pressure, ,, ,, ...	111·6 ,,
,, ,, following morning	96·8 ,,

Obviously the influence of such factors on the subject of high blood-pressure must, a fortiori, be strongly prejudicial.

To estimate the effect of these factors on what I have called the "permanent" or enduring level of blood-pressure a comparison is made between the average morning reading on the day of, and that of the day after, the operation of the factor in question. These times are selected, as the blood-pressure is then less likely to be affected by other disturbing causes. The rise in the permanent level of pressure, resulting from exercise, undue mental activity, and excitement, is seen to be but slight in each case—viz., exercise, 1·4 mm.; mental work, 0·7 mm.; excitement, 2·8 mm. By such an amount no great immediate effect could be produced. There is reason to believe, however, that these factors, if long continued, produce a "summation" effect. The observations here given do not extend over a sufficiently long period of time to establish this point. Whether or not it be established, even so slight a rise, added to a pressure already too high, must exert a deleterious influence.

In illustration of the effect of these factors—mental work, excitement, and worry—in producing a state of permanent high pressure with thickened arteries and enlarged heart, I may cite a case which recently came under my care. A gentleman, aged 65, had thickened fibrotic arteries, an hypertrophied heart, with a systolic aortic bruit, and a systolic blood-pressure of 240 mm. Hg. He complained of dyspnœa on exertion and a dull pain over the aortic area. In all his organs he was perfectly normal. He had lived an exemplary life as to food and drink, and had had no illness or disease of any kind. All his life he had been actively engaged in a harassing business, and during later years he had entered, in addition, upon a strenuous political career. The most careful inquiry and examination failed to elicit any other cause for his vascular condition.

ALTITUDE.

During the course of the observations the test subject spent the month of January at a Swiss resort at an altitude of 6,000 ft., during which time the morning and evening readings were continued. Owing to the disturbing effect of the rarefied air at a high altitude on the readings of the compressed air manometer, Martin's mercurial instrument was used, and, for the sake of uniformity, during the periods before and after as well. The results are shown in the table below. The average *morning reading* of twenty-nine days spent there was 109·7 mm. Hg.—i.e., 15·6 above the normal of 94·1, and 10·8 mm. above the average of twenty-nine days preceding. The maximum reading was 115 and the minimum 105 mm. Hg. The average *evening reading* was 118—i.e., 13·1 mm. above the normal, and 10 mm. above the average of the preceding period; the maximum reading being 126 and the minimum 108 mm. Hg. The average *day rise* was 8·3, compared with the normal rise of 10·8. The maximum day rise was 18 mm., the minimum 0·3 mm. It is noteworthy that the rise took place rapidly, for the pressure on the evening of arrival was 120 mm., that of the previous evening having been 110 mm.

TABLE VII.

	Average a.m. pressure	Day rise	Average p.m. pressure
29 days before	98·8	9·1	108
29 days in Switzerland... ...	109·7	8·3	118
16 days subsequently	102·5	6·3	108·8
200 "all in" days	94·1	10·8	104·9

Thus the effect of a period of residence at an altitude of 6,000 ft. was a rise of about 10 mm. Hg. in the general level of blood-pressure. On returning to the ordinary conditions of life the evening pressure somewhat rapidly regained its former height. In the morning pressure, however, the fall was slower, for the preceding *absolute* level was resumed only after the expiration of fifteen days, at which time the *average* level still remained well above the normal.

Altitude, therefore, causes an enduring rise in pressure to a far greater extent than was seen to be the case with exercise, mental work, and excitement. The factors concerned in the production of this notable rise may briefly be discussed. They include increased heart action, increased peripheral resistance, and increased viscosity of the blood; the first of which is probably predominant. In evidence of this the record of the pulse-rate, taken at the same time as the pressure readings, showed an average morning pulse of 69·3, and an evening pulse of 84·1, being a difference of 14·8 beats during the day, compared with the average increase, under conditions of ordinary life, of only 8·8 beats.

The causes leading to increased heart action are: The greater respiratory activity demanded by the rarefied state of the atmosphere, and the greater amount of exercise, of which far more was taken by the test subject while in Switzerland than at home.

The second factor, increased peripheral resistance, is operative by the influence of cold, which, as is well known, causes a rise in blood-pressure. The cold, however, during the daytime is usually tempered by a hot sun, while after sundown most of the time is spent indoors in a well-heated hotel. The influence of this factor is therefore probably but slight.

The third factor, increased viscosity of the blood, is of considerable moment. The rise in the number of red corpuscles per cubic centimetre of blood, which occurs at high altitudes, is phenomenal, and the extra energy required to propel a concentrated blood through the capillary tract, especially if this be constricted through cold, must be relatively enormous. Excessive loss of fluid from the skin, by evaporation into the dry and rarefied air, accounts in a large measure for the blood concentration; but there is evidence also to show that actual new formation of red cells takes place as a result of the general stimulation of metabolism.

Applying these results, it is obvious that caution should be exercised in the selection of cases to be sent to such high altitudes. Young subjects, free from organic cardiac or vascular disease, merely suffering from a toneless, jaded, fagged-out condition, or in a state of convalescence from illness, may be sent with impunity; indeed, with the certainty that they will derive great benefit, provided the heart has not recently been damaged by any of the acute infections. For the necessary reserve power is present in youth and the heart will respond to the stimulus calling for extra work. But in middle age and the elderly it is otherwise. If there be arteriosclerotic changes, and the heart be already enlarged, there is, unquestionably, danger in sending them to high stations, not only from the risk of cerebral hæmorrhage, but mainly from the risk of inducing dilatation of the heart, the reserve power of which is too limited to meet the extra demands made upon it.

An interesting example of this consulted me during last season at Harrogate. A gentleman, aged 64, had thickened arteries, an enlarged heart, and a systolic blood-pressure of 250 mm. Hg. His heart had stood well the strain of years of high pressure, and he was unconscious of any symptoms, until quite recently when he was sent to Switzerland, to an altitude of about 5,000 ft. While there he had an alarming attack of dyspnœa and heart failure; whereby attention was drawn to his cardio-vascular condition. Since then he has been obliged to relinquish his activities and lead a very quiet life, but in spite of this his heart gives rise to serious anxiety.

The rise in blood-pressure due to altitude has an important bearing on cases of phthisis—so frequently sent to high resorts—with respect to the risk of hæmoptysis. In the young with healthy, resilient arteries the danger is not great, though by no means negligible. In the middle-aged there is, in my opinion, a real risk, and cases of phthisis having a blood-pressure above the normal to any marked extent should not be sent to high altitudes. Fortunately, in this regard, the blood-pressure in phthisis is usually low; nevertheless, while out there I have seen accidents happen which might not have occurred at a lower level. It would be interesting to learn the relative frequency of hæmoptysis in cases of phthisis at high stations, compared with those treated at lower levels. I am not aware of any data on the subject; a comparison would be difficult to establish owing to the many fallacies introduced by collateral circumstances.

CONSTIPATION.

During the course of the series of observations the days were noted on which, from some cause or other, the bowels failed to act. The effect of constipation appears to be a diminution in the extent of the daily rise in blood-pressure. Thus, the average day rise of nine days (not consecutive) on which there was no action of the bowels amounted only to 7·4 mm. Hg., as against the normal rise of 10·8 mm.; the maximum rise being 12 and the minimum 0. The subsequent night fall, however, is 8·0 mm., which is somewhat greater than the preceding day rise; the converse, it will be noted, of the normal event of a higher day rise than night fall. Consequently the level of the pressure on the morning following constipation days is lower than that of the constipation days themselves. The differences, however, in each case are but slight. For example:—

TABLE VIII.

a.m. pressure, constipation days	96·5
Day rise, ,, ,, ...	7·5
p.m. pressure ,, ,, ...	104
Subsequent night fall	8·0
a.m. pressure next morning	96

Whether the lessened day rise is due to the absorption of toxic material from a loaded bowel, and its action as a depressant on the circulation, there is no evidence to determine, but the suggestion is apparent.

APERIENTS.

To ascertain the effects of aperients, *Apenta water* was taken on thirteen successive mornings in doses sufficient to move the bowels freely without causing purgation. The result is shown in the following table :—

TABLE IX.

	a.m. pressure	Day rise	p.m. pressure
Average of 13 days before Apenta	91·3	12·8	104·1
,, 13 days, Apenta daily...	93·8	12·4	106·2

The day rise is practically the same both before and during the Apenta period; they are both somewhat higher than the normal, 10·8. During the Apenta period the level of morning and evening pressure is distinctly higher than before, pointing to the conclusion that free action of the bowels remove some substance having a depressor effect. The figures for the post-Apenta period cannot be given, as certain drugs were then taken which interfered with the results.

The Old Sulphur Water (Harrogate).—This water, which has a marked aperient action, was taken on six successive mornings in doses sufficient to produce a free watery evacuation of the bowels. It contains, in addition to a large quantity of salines and sulphur, an appreciable amount of barium chloride, a drug which has a marked effect in raising the blood-pressure. The results are tabulated below :—

TABLE X.

	a.m. pressure	Day rise	p.m. pressure
Average six days, pre-sulphur period	97·5	9·5	107·0
,, ,, ,, sulphur water, daily	99·2	10·5	109·7
,, ,, ,, post-sulphur period	97·3	4·7	102·0

The day rise in all three periods is less than normal. In the sulphur period a slight increase is produced; followed, in the post-sulphur period, by an extraordinary drop, the explanation of which is not obvious. The average level of blood-pressure, both morning and evening, is raised during the sulphur period, as was the case with Apenta water, and probably for the same reason. The barium chloride may have a contributory effect. Clinically, in my experience, the effect of the old sulphur water on cases of high tension, especially in the gouty state, is a reduction in pressure, possibly from the elimination of substances having a pressor effect. It may be that there are substances present in the bowel having a pressor or depressor effect according to the nature of the case. At present, however, we know little or nothing of the actual nature of these bodies.

ALCOHOL.

Alcohol, as would naturally be expected, causes a lowering of blood-pressure, which appears as a diminished day rise, and a lower level of evening pressure, on the days when it was taken in considerably more than ordinary amount. The customary daily consumption of alcohol by the test subject was one and a half glasses of light port at dinner and a whisky and soda at night (taken after the evening observation). The average day rise of ten days on which, for some reason or other—e.g., attending a public dinner or other festive occasion—the consumption of alcohol was largely increased, amounted only to 3·2 mm., compared with the normal day rise of 10·8, and on two occasions out of the ten there was actually a day fall of −2 mm. and −5 mm. respectively. Consequently the average pressure on the evenings of alcohol days is less than normal—viz., 99·7, as against 104·9—and the usual night fall becomes less or, more often than not, is converted into a night rise. Peripheral dilatation must be held as the main cause of the lowered pressure. The custom in treating cases of high blood-pressure is usually to prohibit alcohol entirely. I am doubtful whether this is a necessary or even a wise proceeding. When alcohol has been taken to excess it is desirable, of course, that the quantity should be limited; but when it has always been taken moderately it is, in the majority of instances, unnecessary or even harmful to withhold it altogether. On the other hand, to prescribe alcohol as a tonic with the object of raising the blood-pressure in cases of low tension is not only useless but harmful, as it tends to produce exactly the opposite effect. The legitimate use of alcohol should be confined to those instances in which it definitely improves a flagging appetite.

TEA.

Tea causes a rise in systolic pressure of a somewhat transitory nature. Thus a cup of tea taken in bed in the morning puts up the pressure usually about 5 or 6 mm. Tea or coffee taken at breakfast is mainly responsible for the rise observed after that meal. Tea in the afternoon raises the pressure about 8 mm. for half to one hour (*vide* Table III, showing events of an ordinary day). Complete abstinence from tea for one day gave a result as shown in the following table :—

TABLE XI.—DAY WITHOUT TEA OR COFFEE.

a.m.		Pressure	Pulse
7.45	In bed, recumbent	80	54
7.55	,, ,, after two cups of hot water	81	60
8.15	Sitting up, out of bed	87	60
8.40	Dressed, before breakfast	93	64
9.5	After breakfast (egg, fish, hot milk)	95	66
9.40	After smoking	99	74
11.30		95	64
p.m.			
1.0	Before lunch	95	64
2.15	After lunch and a smoke	97	72
4.10		93	63
4.20	After hot water, vice tea	94	64
4.45	After a smoke	97	66
8.15	After dinner (soup, meat, savoury, dessert, cigarette, no coffee)	97	72
10.0		95	72

It will be observed that from breakfast-time onwards through the day the blood-pressure remains fairly constant, with but slight variations, in spite of the free use of tobacco. In subjects who indulge largely in tea—and they are chiefly women—the blood-pressure will usually be found high. This is the prime factor, I am convinced, in some cases I have met with of high pressure in women, in which no other cause could be found. The excessive consumption of tea must undoubtedly be regarded as a cause leading to arteriosclerosis.

CAFFEINE.

Further to test this point a dose of 10 gr. of citrate of caffeine was taken by the test subject three times a day for three successive days—in addition to the normal consumption of tea—and the result, compared with a like period before and after is shown in the following table :—

TABLE XII.

	a.m. pressure	Day rise	p.m. pressure
3 days before	89·3	8·6	97·9
3 days of caffeine	91·3	11·3	102·6
3 days after	91·6	8·6	100·2

Not only was the day rise increased, but the morning and evening averages were both raised, the evening notably, and the rise persisted to a less degree during the three following days. Corroboration is thus afforded of the statement made above that tea is a likely cause of permanent high pressure in those who indulge too freely.

TOBACCO.

The test subject is, and has been for years, a somewhat heavy smoker. His ordinary consumption is about 4 oz. of mild tobacco per week, almost entirely in a pipe; cigars and cigarettes being smoked only occasionally. During the series of observations he followed his usual habit, except where otherwise stated. In an individual thus habituated to tobacco a pipe smoked early in the morning causes a distinct rise in blood-pressure. Thus, the average of the morning observations on twenty-seven days when smoking was indulged in before breakfast, and before the morning reading was taken, was 100·3, compared with the normal level of 94·1 mm.—an increase of 6·2 mm. A pipe smoked immediately after breakfast causes an increase in the usual rise observed after this meal, which rise, as has been stated, is mainly occasioned by the tea or coffee taken with the meal. Tobacco smoked later on in the day, either before or after a meal, usually has little or no effect; sometimes a rise was noted, but only of a few points; sometimes even a fall, but generally no marked result was observed (see Tables III and XI).

The variations in blood-pressure during a day of complete abstention from tobacco are given in the table below, which should be compared with Table III, showing the events of an ordinary day.

TABLE XIII.—DAY WITHOUT TOBACCO.

a.m.		Pressure	Pulse
8·45	Before breakfast	91	60
9.5	After ,, (bacon, egg, coffee)	98	72
9.35		95	64
p.m.			
1.0	Before lunch	95	52
2.30		93	60
5.30	After tea at 4.15	93	60
7.0	Before dinner	97	72
8.15	After dinner (soup, meat, sweet, savoury, dessert, coffee)	97	60
9.15		95	60
10.0		98	70

The range of variation is seen to be comparatively slight, the maximum oscillation being 7 mm., which is much less than that of an ordinary normal day. Abstention from all three—tea, coffee, and tobacco—results in a lesser variation still, the maximum oscillation being 5 mm., as the following table shows:—

TABLE XIV.—MORNING WITHOUT TEA, COFFEE AND TOBACCO.

a.m.		Pressure	Pulse
8.45	Before breakfast	88	60
9.15	After ,,	93	64
9.50		88	68
11.30		91	52
12.45		93	60

It would appear, then, from these observations that the day rise in blood-pressure is mainly the effect of tea, coffee, and tobacco; that the rise is established soon after breakfast and continues, without much further increase, throughout the day. Tobacco and tea taken later on in the day, when the rise is fully developed, have little or no effect in further increasing it; an additional rise, if it occurs, is only transitory.

The general level of blood-pressure in habitual heavy smokers is frequently low; in the absence, that is, of overshadowing causes tending to raise the pressure. I have met with cases of persistent low blood-pressure in men perfectly healthy in all respects, who admitted being very heavy smokers, too often to doubt that a casual relationship exists between the two. It is probably due to gradual enfeeblement of the heart by the prolonged toxic action of nicotine. Though the heart may show no evidence of actual dilatation, the rhythm is easily disturbed, if not irregular: and the amount of muscular exercise borne by these subjects is less than normal, breathlessness and fatigue being easily induced on exertion.

Whether tobacco is one of the direct causes of arteriosclerosis—as is maintained chiefly, I believe, by the French observers—I have never been able to satisfy myself. In occasional smokers the question does not arise; in habitual smokers the rise induced by tobacco is too slight, it would appear, to be an important factor in the production of arteriosclerosis by virtue of the pressure *per se*. Moreover, so far as my observations go, tobacco has no cumulative effect in raising the blood-pressure. The enduring effect of tobacco in excess is usually a lowering of the general level of blood-pressure by enfeeblement of the central pump. Therefore, if arteriosclerosis be induced by tobacco it is in a form accompanied by low rather than high pressure. The examples of heavy smokers I have met with showed no obvious thickening of the arteries accessible to the touch.

In order to explain the apparent anomaly that the initial effect of the occasional or moderate use of tobacco should be a rise in blood-pressure, while the late effect of excessive smoking is frequently a reduction in the level of pressure, the suggestion has been made that certain antibodies may be formed in the blood as a result of the continued action of the toxic bodies absorbed from the tobacco. Reasoning by analogy with the reaction excited by other toxins, the suggestion is plausible, but as yet it is a matter of pure speculation.

EFFECT OF DRUGS.

In text-books of medicine, and in monographs on medical subjects, the general statement is frequently met with, in dealing with treatment, that "drugs should be given which raise the blood-pressure," without, in most cases, specifying which drugs. From the routine measurement of blood-pressure in practice it is my experience that most of the ordinary medicines used as cardiac and vascular "tonics" have little or no effect in raising the pressure. No doubt when tested on animals in the laboratory, by laboratory methods, the pressor effect is marked and in some cases striking; but when given to patients in ordinary doses by the mouth, the effect on blood-pressure is, to say the least, not conspicuous.

With the object of inquiring into this question various drugs were taken by the test subject in the usual doses for varying periods and the results noted. As the best indication of any enduring, as contrasted with immediate or transitory, effect a comparison should be made of the *morning* observations before, during, and after the administration of the drug in question; for this is less likely to be disturbed by extraneous influences, and gives the surest guide to any alteration in the general level of blood-pressure.

Calcium Chloride.

This substance, reputed to have a marked effect on the contraction of cardiac muscle, was taken in doses of 15 gr. three times a day for five successive days, with the following result:—

TABLE XV.

	a.m. pressure	Day rise	p.m. pressure
Average of 5 days before	94	10	104
,, 5 days $CaCl_3$	91	10	101

The result on the pressure is a slight reduction in the general level, but the amount of the day rise remains unaltered.

On another subject who suffered from chilblains the effect of calcium lactate in 15-gr. doses, three times a day, was tried, with the result given below:—

TABLE XVI.

	a.m. pressure	Day rise	p.m. pressure
Average of 5 days before	102·4	13·1	115·5
,, 5 days calcium lactate ...	107·4	5·6	113·0
,, 5 days after	104·2	7·5	111·7

A distinct rise was effected in the morning pressure of a somewhat enduring nature, as the average morning reading is higher after than before the exhibition of the drug. The evening pressure was conversely influenced, hence the day rise during and after the drug is markedly lessened.

Strychnine.

Strychnine was taken by the mouth in doses of 5 ♏ of the liquor strychninæ three times a day for two series of observations, lasting eight and ten days respectively.

TABLE XVII.

First Series.

	a.m. pressure	Day rise	p.m. pressure
Average of 3 days before	95·3	9·0	104·3
,, 8 days strychnine	97	10	107
,, 6 days after	93·8	12	105·8

Second Series.

	a.m. pressure	Day rise	p.m. pressure
Average of 10 days before	95·8	11·7	107·5
,, 10 days strychnine	95·9	10·1	106
,, 10 days after	100·2	8·1	108·3

In neither case is the result very striking. In the first instance there was a small rise of 2 mm. in the morning reading during the period the drug was taken, which fell, however, to below the original level after the drug was discontinued. In the second series no rise took place in the average level of morning pressure during the drug period, but a distinct rise of nearly 5 mm. occurred subsequently, showing that strychnine does exert something of a sustained pressor effect, which may in part account for its repute as a "tonic" drug.

Digitalis.

This drug was taken in the form of Parke, Davis and Co.'s concentrated standardized tincture—which is eight times as strong as the B.P. tincture—in doses corresponding to 10 ♏ of the latter, three times a day. It was experimented with on two occasions, the first being when the level of blood-pressure was unusually low and the subject felt slack and out of sorts. Contrary to expectation, the effect was trifling, probably because the dose taken was insufficient.

TABLE XVIII.

First Series.

	a.m. pressure	Day rise	p.m. pressure
Average of 4 days before	83	5	88
,, 4 days digitalis	82·5	8	90·5
,, 4 days after	80	11·5	91·5

The average morning level was actually lower during and after the drug period; the day rise was slightly increased both during and after, and the evening level was somewhat higher.

A second series of observations was undertaken some months later, the dose being increased to 15 ♏ of the same preparation.

Table XIX.

Second Series.

	a.m. pressure	Day rise	p.m. pressure
Average of 4 days before	91·7	9	100·7
,, 4 days digitalis	93·5	7·5	101
,, 4 days after	94	8·2	102·2

A distinct rise in the average level of morning pressure took place, though to a less extent than was anticipated; it amounted only to rather less than 3 mm. Probably here, again, the dose was either too small or was not taken long enough, for Mackenzie and others have shown that a very considerable dose is required in many cases before the effect of the drug becomes apparent. It was discontinued because it gave rise to an uncomfortable sense of constriction in the chest. Other observers have noted a definite reaction after quite small doses, much smaller than those employed in the experiments under consideration. The discrepancy is doubtless due partly to differences in the potency of the digitalis used, and partly to idiosyncrasies of the subjects experimented upon. Also the fact must be taken into account that we are here dealing with a normal heart whose reaction to digitalis may be, and probably is, different from that of a diseased organ. These results, so far as they go, would seem to corroborate the statement, expressed by many writers, that digitalis should not be used, or rather abused, in cases other than those presenting a definite heart lesion.

Pituitary Extract.

This was taken three times a day for four days in the form of Burroughs Wellcome and Co.'s "tabloids," containing 2 gr. of pituitary extract in each. Result:—

Table XX.

	a.m. pressure	Day rise	p.m. pressure
Average of 4 days before	92·5	5·7	98·2
,, 4 days pituitary	87·5	17·5	105
,, 4 days after	91·2	15	106·2

Contrary to expectation the morning level of pressure was found to be lowered during and after the taking of the drug. The evening pressure was markedly raised, and consequently the day rise shows a great increase, which continued to a less degree after cessation of the drug. This result is not easily explained. That pituitary extract taken in this form produces no enduring rise in pressure is shown by the fact that the morning reading during and after the drug period is lower than before; and, if the increase in evening pressure, with the resulting augmented day rise, be due to a temporary pressor effect, it is difficult to see why this should have continued after the drug was withdrawn, without there being a corresponding rise in the morning pressure.

The effect of pituitary extract administered hypodermically was observed on two separate occasions. The preparation used was Burroughs Wellcome and Co.'s "Vaporole" pituitary extract, each phial of which contains 1 c.c. of sterile extract, equivalent to 0·2 grm. (gr. 3) of fresh posterior lobe. On the first occasion half the contents of one phial was injected twice in the day; on the second, some months later, a full dose was given twice. The results are stated below:—

Table XXI.

First Observation—Half Doses.

a.m.		Pressure	Pulse
9.20		93	66
9.30	Injection ½ c.c. 20 per cent. pituitary extract	—	—
noon			
12.0		95	60
p.m.			
2.0		97	64
6.0		100	64
6.10	Injection ½ c.c. 20 per cent. pituitary extract	—	—
7.0		100	66
11.0		90	70

The average morning pressure for three days prior to the injection day was 82, and for three days after 89, showing that the pituitary extract causes not merely a temporary but a sustained rise in blood-pressure.

Table XXII.

Second Observation—Full Doses.

a.m.		Pressure	Pulse
11.40		99	72
11.45	Injection 1 c.c. 20 per cent. pituitary extract	—	—
11.55		100	66
p.m.			
12.15		102	66
12.40		103	68
1.20		103	72
2.0		103	72
3.30		100	66
5 30		103	66
7.30		101	81
9.0		98	78
9.5	Injection 1 c.c. 20 per cent. pituitary extract	—	—
9.20		105	84
9 40		110	78
10.15		105	78
10.45		105	72
11.15		103	72

The average morning pressure for three days prior to the injection day was 91, and for three days after 98, showing that here again a sustained rise in pressure was produced of the same extent as in the previous experiment.

On the first occasion a slight rise is produced by the first injection, but there is no apparent effect after the second. As, however, the observation could not be made for nearly an hour after the injection a rise may have escaped detection. With the larger doses a rise of only 4 mm. occurred within an hour after the first injection; but after the second, later on in the day, a marked effect was obtained, a rise of 12 mm. being observed in forty minutes. After both experiments, with half doses and with full doses, an enduring rise of 7 mm. resulted in the morning pressure.

It is noteworthy that after each injection, whether of full or of half doses, a marked blanching of the face occurred within fifteen minutes, which lasted nearly an hour. Accompanying this phenomenon was a distinct sensation of chilliness, almost amounting to shivering. Also, with the larger doses marked peristaltic action was set up, resulting in a movement of the bowels within half an hour of each injection.

Glycogen.

Glycogen has been recommended by Oliver as having the effect of raising the blood-pressure. It was taken in doses of 2 gr. three times a day for four days, with the following result:—

Table XXIII.

	a.m. pressure	Day rise	p.m. pressure
Average of 4 days before	91·2	7·2	98·2
,, 4 days glycogen	89·8	8·2	98
,, 4 days after	92·2	8·2	100·4

The effect on the test subject is seen to be practically *nil.*

Ernutin.

Ernutin (B. W. and Co.) is said to contain the alkaloid ergotoxin and the active principles of ergot in a pure state. It was taken by the mouth in 30 ♏ doses three times a day for three days.

Table XXIV.

	a.m. pressure	Day rise	p.m. pressure
Average of 3 days before	88·6	5·6	94·2
,, 3 days ernutin	87·3	4·3	91·6
,, 3 days after	88·0	6·0	94·0

No marked effect was observed.

Lodal.

Lodal (B. W. and Co.) is prepared by the oxidation of laudanoseine (the alkaloid occurring in opium) in a manner analogous to the preparation of cotarnine or narcotine. It is said to have a "pronounced effect on the heart, slowing and strengthening the beat, and producing a rise in blood-pressure in which vaso-constriction is a definite factor." It was taken in doses of 1 gr. three times a day for three days.

Table XXV.

	a.m. pressure	Day rise	p.m. pressure
Average of 3 days before	83 ...	10 ..	93
,, 3 days lodal	84·3 ...	10	94·3
,, 3 days after	85 ...	12 .	97

The effect is trifling, a slight rise in morning pressure being produced during and after the drug period.

Hydrotherapeutic Measures.

Practising at Harrogate, as I do, the opportunity falls to my lot of observing the effect of various hydrotherapeutic procedures on the blood-pressure. The majority of the different kinds of baths there available are usually given at a warm temperature, and consequently act as vaso-dilators and reduce the pressure; and the majority of cases of chronic disease we see there have, I should say, a normal or supernormal pressure. So that the choice of measures to reduce the pressure, if it be thought advisable to do so, is abundant. It is otherwise, however, with the cases of subnormal pressure, for it is by no means an easy matter to procure a sustained rise in the general level. In my experience the number of baths that can be relied upon to produce this effect is limited to three—the Vichy douche, the aërated Nauheim bath at temperatures on or below the indifferent point (90° F.), and the needle bath graduated from warm down to cool or cold.

Of these baths the *Vichy douche* alone needs a brief description. The patient is placed in the recumbent posture on a table covered with an india-rubber air mattress. At a height of about 2ft. above the table, and extending its whole length, is suspended a bracket pierced with numerous pin-holes through which a needle douche is projected downwards on to the patient throughout the duration of the bath, while massage is administered to the whole body by one or more attendants. After about twenty minutes' rubbing a separate needle douche is given in the erect posture and the bath terminates. The immediate effect on the blood-pressure is a rise of 10 to 15 mm. Hg., and after a series of baths the enduring level of pressure is raised. Numerous examples might be quoted from my case-books; the following typical ones will suffice:—

C. P. H., aged 35. Suffering from general fatigue, both bodily and mental, the result of overwork and prolonged residence in the East; heart sound; systolic pressure, 98 mm. Hg. After a course of six Vichy baths the pressure rose to 112 mm. Hg.

Major B., aged 46. Suffering from "hepatic inadequacy"; heart sound; systolic pressure, 102 mm. Hg.; after a course of eight Vichy baths the pressure rose to 115 mm. Hg.

The bath has an extraordinarily exhilarating effect, as judged by objective signs of improvement and by the expression of their subjective sensations on the part of those undergoing a course.

The *aërated Nauheim bath* at temperatures of 94° to 100° F. usually causes a reduction of pressure, roughly proportionate to the height of the temperature employed. When given at a temperature of 90° F. or below, the result is a rise in pressure, not only immediate, but sustained after a course of baths. For example:—

Mrs. P., aged 62. Mitral regurgitation; slight œdema of the feet; systolic pressure, 95 mm. Hg.; after three baths, 105 mm. Hg.; after six baths, 125 mm. Hg.; after nine baths, 130 mm. Hg.

Mrs. B., aged 56. ? Myocarditis; no bruit detected; systolic pressure, 110 mm. Hg.; after seven baths, 115 mm. Hg.; after twelve baths, 123 mm. Hg.

The *needle douche*, even at warm temperatures, causes a rise in pressure, for the stimulating effect of the percussion by the innumerable jets of water overshadows the vaso-dilating effect of the heat. Given at low temperatures, the rise is more marked, and is not only immediate but is cumulative after a series of baths.

Excluding cases of acute illness with febrile disturbance—the acute specific fevers, &c.—which are outside the scope of this paper, the following are some types of disorders which frequently present a low blood-pressure:—

(1) Subjects with *poor circulation*, cold hands and feet, and liable to chilblains. I have met with several examples in which the systolic blood-pressure rarely rose above 95 mm. Hg. By means of baths, massage, and exercise a temporary rise may be effected, during the maintenance of which the objective and subjective signs of defective

circulation improve manifestly; but it is difficult to effect an enduring rise in the general level of blood-pressure in these cases, for they are prone, on cessation of treatment, to relapse to their former state. It is as though the hydrostatic pressure of the circulation were set from birth at a permanently low level. Such subjects are often sluggish both in body and mind.

(2) Cases of pure *neurasthenia*, having as their prominent characteristic profound fatigue, either somatic or psychical, or both. One case I met with recently, a woman, had a systolic pressure of only 85 mm. sitting and 80 mm. recumbent; she was subject to most violent headaches and had been dosed freely and for long periods with the usual headache remedies. A rise in tension of the circulation temporarily afforded relief to the pain. Whether the low blood-pressure in these cases of neurasthenia is the cause or the effect of the extreme fatigue it is not easy to say. The probable sequence of events is a lowering of blood-pressure by toxic or other causes, and it is the extreme feebleness of the circulation that gives rise to the subjective sensation of intense fatigue. Certain it is that a rise in blood-pressure is an almost invariable accompaniment of improvement. In marked contrast to these is the irritable type of neurasthenic—the "neurotic" individual—who is frequently not a true neurasthenic, but a psychasthenic or a hypochondriac. In these cases the pulse is usually quick and irritable, and the blood-pressure subject to great variation, but generally above the normal and sometimes markedly so.

(3) *Tobacco Poisoning.*—Mention has been made above of the effect of tobacco in raising the blood-pressure, and the apparent anomaly that heavy smokers should frequently have a subnormal pressure.

(4) *Dilated Heart.*—In these cases, with or without valvular disease, the pressure will sometimes be found low, and a rise in pressure is one of the indications of the progress of the case towards recovery.

(5) *Fibrositis.*—I have met with many examples of the so-called gouty or rheumatic manifestations of lumbago, sciatica and neuritis which showed a blood-pressure somewhat below normal. Many of these cases have a subnormal acidity of the urine, and are liable almost constantly to a copious deposit of phosphates; and this leads to, or is accompanied by, a state of nervous depression, which is a very real thing to those who suffer from it. The origin of the diminished acidity is digestive, and the nervous depression is due, I am convinced, to reflex disturbance from irritation of the sensitive nerve mechanism of the bladder and urethra by the phosphatic urine. It is remarkable how rapidly the blood-pressure

will rise and the symptoms disappear if the urine be rendered acid and the phosphates kept in solution. No doubt, also, the antecedent digestive disturbance is responsible for the poison, whatever it may be, which gives rise to the local manifestation of sciatica or lumbago, as the case may be. We are accustomed to regard these cases as gouty or rheumatic; some of them, I am convinced, are neither, but are due to an obscure poison of digestive origin as yet undefined.

(6) *Phosphaturia.*—The clinical symptom phosphaturia—so called, for the phosphates are rarely or never in actual excess—in whatever condition it may occur, is usually accompanied by a low blood-pressure, probably for the reason given above.

(7) *Arthritis Deformans.*—Subacute cases of "rheumatoid arthritis" in young subjects frequently have a blood-pressure below normal, which rises as the condition improves.

Contrasted with the ease with which a high blood-pressure may often be reduced, the permanent raising of a low pressure is a matter of greater difficulty. Fortunately it is seldom necessary, were it possible, to raise the blood-pressure to a great extent; a relatively small rise is, in most cases, coincident with marked amelioration. To effect a rise non-medicinal measures, such as diet, exercise, change of air—for example, to high altitudes—and hydrotherapeutic procedures, such as those mentioned, are to be preferred to drug treatment. With regard to diet, an excess of meaty food should be prescribed, with a corresponding diminution of the starchy elements. In this connection it may be remarked that the test subject, having a persistently low blood-pressure, is, and always has been, a relatively large meat-eater; meat forming the staple article in a somewhat spare diet.

Exercise, carefully graduated, improves the tone of the heart and vessels and raises the pressure, while altitude and the baths mentioned have, as has been shown, a marked effect in the same direction.

If drugs be required, pituitary extract would seem to have the most powerful and rapid effect; but I have little or no experience of its continued use over long periods, and would rather hesitate to give much of so powerful a substance lest unlooked-for late results accrued of a harmful character. Next in potency would be caffeine, to be given only if the patient were not an excessive tea-drinker. Calcium lactate appears to be moderately effective. Strychnine, as we have seen, is disappointing. Digitalis should be reserved for cardiac cases. The other drugs experimented with are apparently of little value for the purpose in view.

DISCUSSION.

The PRESIDENT (Dr. Mitchell Bruce) conveyed the thanks of the Section to the author for his paper. It raised so many points in connexion with the different measures and substances employed that he expected all present would desire to take part in the discussion.

Dr. C. O. HAWTHORNE said all would agree as to the thoroughness and interest of the paper. Considering the detail in which the observations had been made and recorded, and the maintenance of identical conditions throughout, one might expect that such a record would be taken as a standard by means of which subsequent observations might be compared. He hoped he would not be thought to be lacking in appreciation of the author's zeal when he asked whether, after all, so far as inferences were concerned, it did not come to this, that various influences, such as physical exercise, mental excitement, mental work, high altitudes, &c., increased the force of the cardiac contraction, and by so doing produced a higher record on the sphygmomanometer. He meant that all those agents, by causing a more powerful systole of the left ventricle, produced a greater amount of systolic force, which expressed itself in the brachial artery in opposition to the pressure which was inflicted upon the brachial artery by the sphygmomanometer. In other words, what was called blood-pressure in the paper was merely the maximum sphygmomanometric record, that is, the force with which the contraction of the left ventricle reached the brachial artery. He submitted that this was only a limited and special aspect of blood-pressure, and until blood-pressure was considered in other aspects the negative conclusions at which the author arrived on many points could scarcely be regarded as standing without qualification. For example, it was manifest that pressure in the brachial artery had other qualities than maximum force, and one of these must be endurance in time. Unless this was considered and estimated it was premature to say that agents which produced no effect on the record of the sphygmomanometer had necessarily no influence on blood-pressure. Take, say, the subject of digitalis: on the basis of his own observation the author had come to a negative conclusion as to the influence of this drug on blood-pressure. Yet it was certain that digitalis had a most powerful effect upon cardiac action, though if the sphygmomanometer was relied upon alone, it could be shown that digitalis produced no rise in blood-pressure. Here, therefore, was a drug which powerfully affected the condition of cardiac contraction, but according to the sphygmomanometer it had no effect at all. He advanced that instance as an illustration of the necessity of extending observations in connexion with blood-pressure from the sphygmomanometer to methods by means of which one obtained information as to the length of time that the pressure endured, whatever was its maximum, and as to the manner in which such pressure declined. Records of blood-pressure which were simply enumerations of the maximum figure obtained from the sphygmomanometer were partial, incomplete, and might be seriously misleading, and so far as instrumental methods were available, all these observations on blood-pressure made by the sphygmomanometer ought to be checked and supplemented by sphygmographic tracings. It was only when those two were in combination that one had a picture not only of the height to which the blood-pressure rose, but some indication of the length of time during which such maximum endured, and of the manner of the pressure decline.

Dr. NATHAN RAW said he had been interested especially in two directions: first, the effect of high altitude on blood-pressure, and secondly, the effect of calcium chloride in the raising of that pressure. He was sorry to see that the effect of calcium chloride in raising the ordinary blood-pressure was small, as one expected it would raise it in treating acute pneumonia. He asked whether Dr. Edgecombe had made observations at higher altitudes than 6,000 ft. Up to that height he got a rise of blood-pressure, which might be an important factor in hæmoptysis in cases of tubercle.

Dr. HUMPHRIS said that to anyone who had worked at electricity it was well known that one could always raise the blood-pressure by pencil or static brush discharge playing up and down the spine. In some half a dozen cases that he had treated since October last the pressure varied between 90 mm. and 110 mm. The causes were various. In the case of one man, he was with his father during a protracted and fatal illness. Another case was that of a woman in society, who had been out a great deal too much. Another case was that of the editor of a paper, who was overworked and worried by his publishers. Another lost money in a bank. They all complained of practically the same symptoms; there seemed to be nothing organic the matter with them. From the beginning of the treatment in every case there was benefit from the application of the brush discharge up and down the spine, and the blood-pressure was increased 5 mm. to 6 mm. It might be said that one did not know how long the effect was going to last, but those patients had remained quite well for some months. No alteration was made in their eating and drinking or mode of life.

Dr. O. K. WILLIAMSON remarked that, in regard to the influence of altitude on the blood-pressure, the only contribution he could make was to relate some observations on himself in the Alps. At the height of 5,000 or 6,000 ft. there had been but very slight alteration in his blood-pressure from normal limits in the course of a month; perhaps on the average the readings were 5 mm. higher than in England. With regard to the influence of greater altitude, combined with exercise, five years ago, with a very approximate instrument, the hæmodynamometer of Dr. George Oliver, which recorded the maximum oscillation—so that it was not as susceptible of accurate readings as the modifications of the Riva-Rocci instrument—he took some observations on himself at the end of a season's mountaineering, and he found there was practically no difference between the readings before the beginning of the expedition (a traverse of Mont Blanc and two other peaks) and at the top of the mountain. He took an observation on himself at a height of 6,000 ft. on the day before the climb. Next morning, in a hut at about 10,400 ft., he took another observation at 2.30 a.m., just before starting, with practically the same result (6 mm. higher). At the top of Mont Blanc (about 15,800 ft.), at 10.15 a.m., the reading was identical, whilst later in the day, having descended to a height of 6,700 ft., the reading was 10 mm. higher at 4.30 p.m. (125 mm.). These small variations may have been accounted for by the fact that he was then in good training. Three years ago he took observations on a friend during the ascent of the Finsteraarhorn (14,000 ft.), and in him also there was only a slight change in the blood-pressure between the observation before the commencement of the climb at 2 a.m. at a height of 9,400 ft., and that after reaching the top, when at 10.30 a.m. the reading was 10 mm. higher (140 mm.). The instrument was Hill and Barnard's small instrument, which again is not so reliable as the armlet method. With regard to the effect of food or tea, he understood from Dr. Edgecombe that the effect of tea was always to raise the blood-pressure. But he had found in his own case that in the morning his blood-pressure fell a few minutes after taking a cup of tea. His ordinary pressure was 120 mm., and after a cup of tea it fell to 110. With regard to food generally, the result was different in different people. In many he found that the pressure rose 10 mm. immediately after taking it, whereas in others it fell. There seemed to be no general rule as to whether one could expect a rise or a fall. If the result was simply that a certain amount of blood was determined to the splanchnic area, one would expect a fall of pressure. But normally there was compensation produced by vaso-constriction of arteries in other parts of the body which might neutralize the fall or cause a rise, and that accounted for the anomalies met with.

Dr. ROBERT HUTCHISON said he shared the admiration which had been expressed as to the care and diligence with which the observations had been made, but one must not shut one's eyes to certain sources of fallacy which might underlie them. Dr. Hawthorne had pointed out one possible important fallacy—namely, that one had to consider the duration of increased strain or tension in the vessels—i.e., the diastolic pressure—which was far more important from the point of view of pathology than the systolic, which might be transient, and due to temporary stimulation of the heart. Another had been referred to by the last speaker—namely, the part played by idiosyncrasy in the matter of blood-pressure, as in other things in the human body. People reacted differently to tea, tobacco, alcohol, &c., and because those things caused a rise in one person, it did not follow that they would in all. Another fallacy was that the observations were made on a person with rather low blood-pressure, and because certain things raised an abnormally low blood-pressure to the normal, it did not follow that they would raise the normal to supernormal. When mental work raised the blood-pressure, it was a device to get an increased quantity of blood through the brain. But if a person had already got high blood-pressure, he did not need to get more blood there; the brain already had too much. It was a reasonable and plausible conclusion that a person with an abnormally low pressure might need to raise his to the normal, but it did not follow that a person in whom it was normal needed to raise it to supernormal. Conversely, agents which acted in one way on a high pressure did not act similarly on a low one. Pharmacologists had denied that iodide of potassium was capable of lowering blood-pressure at all, yet all clinicians were agreed that cases of abnormally high pressure could be lowered by giving iodide. Therefore what was true of one state of blood-pressure was not necessarily true of a different state of blood-pressure. One must be cautious also about the inferences which there was a tendency to draw from such observations. For instance, the observations of the author tended to show that alcohol lowered blood-pressure, and therefore it seemed that there was no need to forbid alcohol where the pressure was high. He (the speaker) did not think that followed. Alcohol might in gouty subjects, by interfering with the complete combustion of proteids, cause the production of abnormal pressor substances. Therefore it might be good to forbid alcohol in such cases, although the alcohol itself was not directly responsible for a rise of pressure. The whole subject was so complicated, even in regard to tea, coffee, &c., that one should be very cautious in drawing wide conclusions, though he was sure observations such as Dr. Edgecombe had made were urgently called for, extending as they did over long periods of time, but they should be controlled by similar observations made in many other persons.

Dr. F. W. PRICE desired to remark simply on the effect of digitalis on the blood-pressure. During the last year Dr. James Mackenzie, Professor Cushny, and he had been making observations on a series of heart cases, and their investigations had certainly confirmed what Dr. Edgecombe said. Although in some cases large doses of digitalis, even to 1½ dr. a day, had been given, there had not followed in any single case any rise in the blood-pressure.

Dr. BEZLY THORNE said he yielded to no one in his admiration of the paper, and the careful observation and amount of study which it represented. But he was sure Dr. Edgecombe would be the first to acknowledge that one could not generalize from those observations without comparing them with others taken under different circumstances. He particularly sympathized with Dr. Hutchison in his disappointment that the diastolic pressure was not taken and recorded in these cases. He generally abjured the term "diastolic pressure." He had taken blood-pressures for twelve years—since Hill and Barnard brought out their first instrument—and he limited his observations at first to what was called systolic pressure. But after a time he found that observations of extreme or maximal pressure were very misleading, and that they did not gauge the condition of the patient, because they eliminated that most important consideration of vaso-dilatation which took place rapidly under certain circumstances, and the condition of the elastic and muscular coats of the artery. Last summer he attended an Indian Medical Service officer, who had some dilatation of the heart, and the patient came to him one morning and said his pressure was 140 mm. His reply to him was that he did not think he (the patient) knew what his systolic pressure was, but that he would see what the mean blood-pressure was—i.e., the point at which the hydrostatic pressure in the artery exactly, or as nearly as could be observed, balanced the pneumatic pressure in the pressure bag. That was found to be 90 mm. He said to the patient that he thought he could tell him what was the importance of that observation. A patient had just left the room who was 70 years of age, in whom the balance-pressure was the same as his, 90 mm., but her extreme pressure was 240 mm., and she would be dead before the year was out. What was the difference between those two cases? He submitted the difference was not that the heart was exerting any more force in the one case than in the other, but that in the one case the buffer action of well-developed muscular and elastic coats prevented the wave from coming through the air bag, and in the other a wasted artery allowed a full swing of the heart beat to come through. Therefore he submitted that if pressure observations were to teach much and to be of practical value, there must be a comparison between the mean pressure and the maximal pressure, which was often, as he thought falsely, called the systolic pressure.

The PRESIDENT asked the author whether he would regard his results, obtained in a person with low pressure, as applicable to the average man. That point had already been referred to, and it was very important because practically all those observations had been based on a person whose blood-pressure was low. He would also have liked to have heard what was the condition of that person otherwise; was he dyspeptic, had he a dilated stomach, or was he neurasthenic? Many of the patients with low blood-pressure had a dilated stomach and were miserable, with low feelings, and complaints of weakness. He would be glad to know whether the subject of these observations corresponded to that type—he had often found such persons with large dilated lungs, as if they were emphysematous, though not suffering from the effects of emphysema. He was much interested to hear that ordinary mental stress or mental exercise raised the blood-pressure so definitely. That was very important, because it obviously bore on the great question as to how far that was responsible for the large number of cases of wearing-out seen by physicians: patients from the City, professional men, &c., who came earlier in life than they should do with arteriosclerosis and diseased hearts. Was it exercise of their intellectual faculties that produced that? He doubted it, because there was so much evidence to the contrary in the persons of distinguished members of the professions, including their own. With regard to worry, he gathered that the effect of that was not only to raise pressure, but to make the rise more enduring, so that next morning the pressure was higher than it would be after purely intellectual work. That was rather opposed to the interesting account which Dr. Humphris gave, for he said that his patients came with low pressure if they had been worried. Perhaps the author could account for the difference between those observations. With regard to alcohol, he agreed with Dr. Hutchison that one should be very cautious about acting on the conclusions given in the paper. Not only did alcohol raise the blood-pressure under certain circumstances, but manifestly, by producing sclerosis, especially of the kidneys, it was one of the commonest causes of the high blood-pressures that were met with, in which one had to be so careful to forbid alcohol. With regard to tobacco, there was the consoling conclusion that the man who smoked steadily was safer than he who smoked only occasionally; he suggested that it might be due to the fact that habitual smoking produced a kind of immunity against its effects.

Dr. EDGECOMBE, in reply, thanked the President and Fellows for the kind and courteous way in which they had criticized his paper. From lack of time he would not answer each speaker separately, but would deal with the main points. He was aware of the fallacies which had been pointed out. The most important criticism was, that only the obliterating pressure was taken, and the diastolic pressure was neglected. In his judgment, the accurate reading of the so-called diastolic pressure was so difficult, and the personal equation so great, that the results were apt to be fallacious. In ordinary routine work he took both obliterating and mean pressures, and what guided him most was the relation between the two, rather than one actual figure or the other. In a long series of observations, since the mean pressure does not vary to the same extent as the obliterating pressure, it would seem that the observations of the former must be so full of error that any conclusions drawn from them would be of little value. He did not think the conclusions from such observations of the obliterating pressure were worthless, for the reading of it was definite and clear. The point had been raised that the obliterating pressure was only transient, due to the more excitable or vigorous action of the heart at the time. That was the point which he wished to eliminate by extending the observations over a considerable period, in order to determine whether there was a permanent or enduring rise. When this occurred, it must have been due to some sustained increased power of contraction of the ventricle, and therefore the procedures effecting an enduring rise in systolic pressure might be regarded as having increased the power of the heart's contraction. With regard to idiosyncrasies, he knew it was not altogether safe to draw conclusions from observations on one subject and apply them indiscriminately to others. The President asked whether the observations were applicable to an average man. If by that was meant a man with average blood-pressure, he would say that there should be considerable caution, but they were fairly applicable to the subjects of subnormal blood-pressure. In his own practice he had been helped by the results which he arrived at. With regard to the physical condition of the subject of the research, the subject was himself, and he would be sorry if he were in the condition so graphically described by the President. He believed he had quite the average of vigour, but his blood-pressure was low. The effect of strenuous mental work on him was very marked, from which the inference might be drawn that he was normally of sluggish mental habits! He agreed with Dr. Hutchison that in the subject of high blood-pressure the effect of an extra amount of mental work would not be anything like so great, absolutely, nor even relatively; but he claimed that in most people mental work did cause an increase in the pressure. Whether mental activity was roughly proportional to the height of the blood-pressure was an interesting speculation: the facts seemed to point in that direction. As to the effect of mental work and worry, he did not draw hard-and-fast inferences, for there were many fallacies; hence his conclusions were merely tentative. One object of the paper was to stimulate others to take similar observations, because it was only by the collation of material gathered over continuous periods that sound conclusions could be drawn.

Medical Section.

March 28, 1911.

Sir WILLIAM ALLCHIN, Vice-President of the Section, in the Chair.

The Electrocardiographic Method and its Relationship to Clinical Medicine.

By THOMAS LEWIS, M.D.[1]

A BRIEF DESCRIPTION OF THE GALVANOMETER METHOD.

GALVANOMETERS, instruments constructed to measure electric current, and electrometers, instruments constructed to measure electric potential, have been in general use in the physiological laboratory for many years. The time at my disposal will not permit me to detail the history of our knowledge of these instruments. It will be convenient if I turn immediately to the latest development, the introduction of the new "string galvanometer" of Einthoven. It is an instrument which, on account of the quickness of its response, its sensitivity and the ease with which it is managed, is rapidly superseding all other instruments of its class. It consists of a powerful electromagnet, the separate poles of which are closely approximated, and leave between them a narrow chink (fig. 1). In this chink an extremely fine fibre (of platinum or silvered quartz) is suspended. The principle of the instrument is simple. When you pass an electric current through a conductor lying in a magnetic field, conductor and magnet react upon each other, and if one system is fixed and the other is movable, the latter (the movable system) is deviated in a definite direction. In the string galvanometer the magnet is fixed, the string is slack and flexible; the string is the movable system, and it deviates when a current is passed through it. The quickness and sensitivity of the instrument depend upon the lightness of the fibre and upon the strength of the magnetic field. The strength of the magnetic field of the string galvanometer and the lightness of the fibres are such that accurate measurements may be made of very minute currents. When the instrument is properly adjusted the deviation of the string is directly proportionate to the strength of current passed through it.

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[1] Working under the tenure of a Beit Memorial Research Fellowship.

I shall pass over the history of our knowledge of the electric changes which accompany the contraction of muscle and the heart-beat, and turn to the more important facts now known to us, and which immediately concern us.

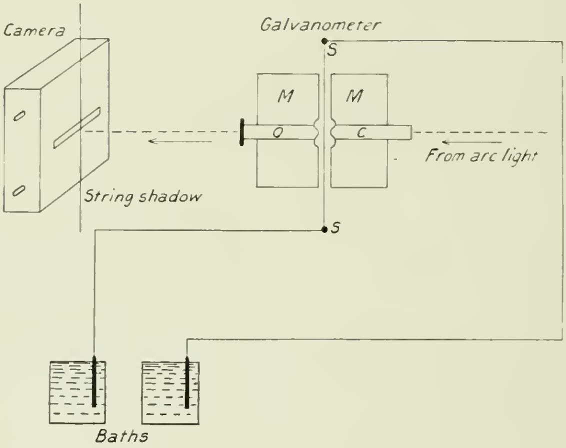


Fig. 1.

A simple scheme showing the arrangement of the apparatus used in securing electrocardiographic curves. It consists essentially of the galvanometer, arc light and camera. The galvanometer consists of a powerful electromagnet, *M M*, between the poles of which is a slit in which lies a string, *S S*, of extreme tenuity and composed of platinum or silvered quartz. The string is connected to a circuit which is completed by the patient, whose limbs are immersed in the baths. The beam of light from the arc light falls through the condenser, *C*, and the shadow of the moving string is projected by means of the microscope, *O*, on to the camera. The shadow of the string lies vertically, the slit of the camera lies horizontally, and the movements of the shadow are at right angles to it. Photographic paper is unwound by means of a mechanical arrangement behind the slit.

When a contraction is started in a muscle strip, be it somatic or cardiac, the point at which activity first manifests itself becomes relatively negative to all other points of the strip surface. The activated or contracting end stands in electrical relationship to the inactive end of the strip, as does the zinc terminal of a Daniel cell to the copper terminal. If the ends of the strip are connected to a sufficiently sensitive galvanometer the electric change in the muscle is recorded by a deviation in a given and known direction. Now when a strip of muscle contracts the state of contraction or activity travels from one end of it to the other; at first the proximal or stimulated end is active, and consequently relatively negative to the distal end. Later, as the contraction wave passes to the distal end and the proximal end becomes quiescent the electric condition is reversed, and the galvanometer shows a deviation which is contrary in direction to the original deviation. The complete electric effect is termed a diphasic effect. Now the actual shape of the curve need not delay us, but it is important for

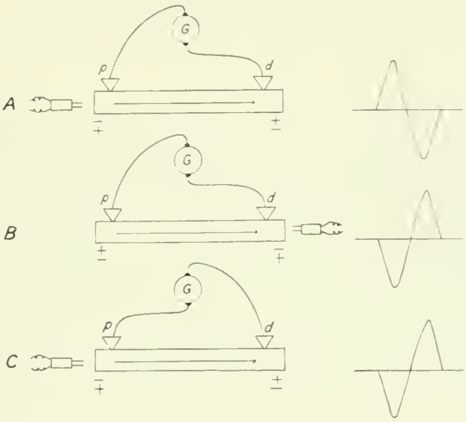


Fig. 2.

A diagram illustrating certain principles of electrocardiographic work. In *A*, *B* and *C* straight strips of muscle are diagrammatized, from the ends of which leads are taken with non-polarizable electrodes to the galvanometer, *G*. In *A* the muscle is stimulated to the proximal end, *p*, and the contraction travels from *p* to *d*. While the contraction is at *p*, *p* is negative to *d*. When the contraction reaches *d* and subsides at *p*, *d* becomes negative to *p*. A diagrammatic representation of the curve obtained is shown to the right of the figure. In *B* the muscle is stimulated at the distal end, and the events are reversed. The contraction passes from *d* to *p*, *d* is first negative to *p*, and later positive to *p*. The type of curve shown to the right is the reverse of that given in A. In *C* the stimulation and direction of the contraction wave are the same as in A, but the electrodes are reversed; *p* is first negative to *d*, and later positive. The type of curve is the same as that yielded by *B*.

us to note that it is dependent upon two factors: first, the point at which the contraction wave starts, and secondly the relationship (in space) of the muscle mass (and all its parts) to the leading off electrodes. Stimulate the muscle at the opposite end and reverse the series of events of the contraction wave, and you obtain a complete and exact reversal of the electric effects. Maintain the point of stimulation and reverse the electrodes, you obtain a similar result. These two facts illustrate the essential principles which must be constantly borne in mind in dealing with the electrocardiogram (fig. 2).

What is the electrocardiogram, and how is it secured? An electrocardiogram is a photographic representation of the electric changes occurring between any two points of the body surface as a result of the heart-beat. It may be obtained by means of special electrodes applied to the base and apex of a heart under experimental conditions. The lead from base and apex of the heart is used because it represents, approximately, the general direction in which the muscle contracts; any other lead will yield an electrocardiogram, but it is essential to fix upon one or more definite leads for the sake of uniformity and comparison, and base-apex leads are usually employed. The electric changes at base and apex of the heart are not confined to them, they spread into the surrounding tissues in contact with them. In human galvanometric work the instrument is usually connected to what closely corresponds to a base-apex lead; a lead from the right arm and left leg is utilized, a lead equivalent to one from the right shoulder and left groin. Others are adopted, but I intend to confine attention to this single lead for the time being.

It is sufficient to connect the right arm and left leg of a patient by means of suitable electrodes, for example, baths of salt water (fig. 1), to the ends of the string (S S) of the galvanometer, to set this string in motion. The movements are minute, but may be magnified and projected by means of a microscope (O C) driven through the poles of the magnet (M M) on to a convenient photographic screen (fig. 1). A beam of light, falling through a condenser upon the string, projects the shadow through the objective. The shadow lies vertically, and at right angles to a slit, behind which a sensitive film is moved on rollers. The shadow moves from side to side across the slit, and hiding the sensitive paper from the light, leaves the impress of its movements upon it.

Up to the present I have described to you the working principles of the instrument, and you have the general idea of the manner in which records are obtained. I have also spoken briefly of the electric changes which accompany contraction in muscle, and have drawn your attention to two important considerations. The curve which is obtained from a contracting muscle depends upon the lie of the muscle in relationship to the leading off electrodes. We need not consider this factor further, for I propose to speak of the single lead—that from the right arm and left leg. Of far greater importance to us at present is the fact that the shape of curve depends upon the direction taken by the wave of contraction in the heart muscle itself.

We may proceed to consider the form of the physiological electrocardiogram, and I shall pass from this to a description of a number of selected electrocardiographic curves of pathological form. The instances given must be regarded only as illustrative.

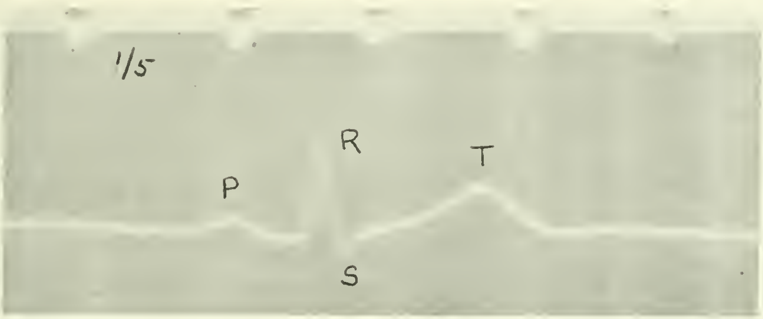


Fig. 3.

An electrocardiogram of a single normal heart-beat. As in all the figures, the upper line is a time marker showing fifths of seconds. The electrocardiogram consists of variations P, R, S, and T, of which P represents the auricular contraction and R, S, and T the ventricular contraction.

The Physiological Electrocardiogram.

As has been stated, the physiological electrocardiogram is obtained by placing the limbs of the subject investigated in baths of salt solution and connecting these baths to the indicator—the string of the galvanometer. The curve photographed is a complicated one and consists of a number of variations. They have been labelled in a purely empiric fashion with the letters P, Q, R, S and T (figs. 3 and 4).

We may divide the whole curve into two strictly separate portions —P, on the one hand, Q, R, S and T, on the other—and I have termed these separate portions the *auricular and ventricular complexes* respectively. P is the result of contraction of the two auricles, Q, R, S and T are the outcome of contraction in the two ventricles. Dealing with the ventricular complex (Q, R, S and T), the dips Q and S are usually inconspicuous, while R and T are prominent. It will simplify matters if attention is concentrated upon the more conspicuous variations, P, R and T; of these P is auricular, and R and T are ventricular. The relationship of these variations to the events of cardiac contraction has been definitely ascertained experimentally. The shortening of the ventricular muscle commences a very small fraction of a second after the commencement of R and finishes a small fraction of a second after the subsidence of the broad wave T.

We need not enter into the hypotheses which attempt to explain the constitution of this electrocardiogram, but may rest satisfied at the present time with the following facts. It represents the electric change in a base-apex lead of a normal mammalian heart, and represents the contraction of the several parts of the cardiac musculature *in a definite order or sequence.* The mammalian heart-beat starts at the junction of superior vena cava with auricle, and the contraction spreads from this point and courses through the auricle to the tissue which joins auricle and ventricle, namely, the auriculo-ventricular bundle. Reaching this tissue the impulse is conveyed along the two divisions of the tract and is distributed through an elaborate arborization and Purkinje network to the ventricular musculature. Let me emphasize the fact that the normal auricular complex (P) indicates in a perfectly decisive manner

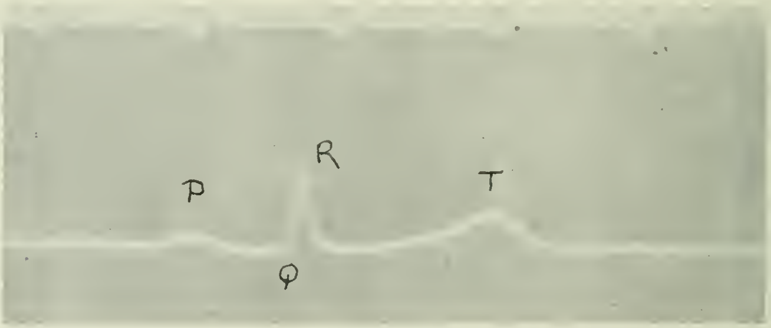


Fig. 4.

A normal electrocardiogram showing P, the auricular representative, and Q, R, and T, ventricular representatives.

the origin of the heart-beat in the neighbourhood of the superior cava; that is a conclusion which we are forced to accept as a result of experimental observations which I cannot now detail. Let me lay equal stress upon the fact that the ventricular complex (R and T, or Q, R, S and T) indicates a ventricular contraction started by an impulse which reaches it through the *normal field of reception*, namely, the arborization of the junctional tissues. It is only the contraction excited in this manner, i.e., through the physiological paths, which gives rise to a curve of the form described. The importance of these facts will be more obvious at a later stage, and in stating them I am merely elaborating the principle which I stated to you when we considered the simple strip of muscle. The shape of the curve depends upon the direction of contraction, *it consequently depends upon the point of origin of such contraction.* Let us take two examples which illustrate this principle, the premature contraction which arises in auricle or ventricle respectively.

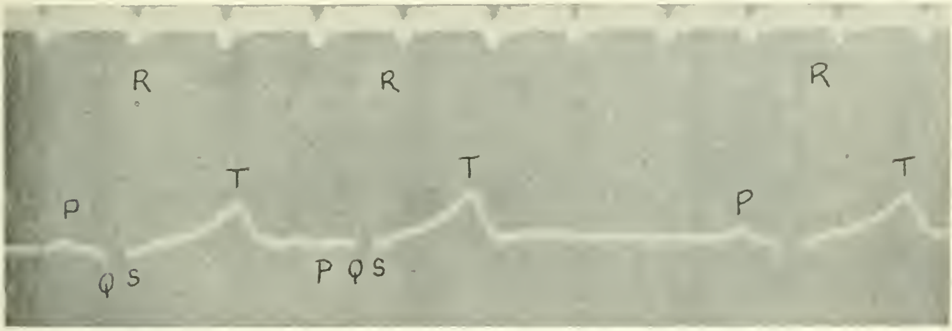


Fig. 5.

An electrocardiogram from a patient showing a premature contraction arising in the auricle. The first beat of the figure shows variations P, Q, R, S and T. They are of normal outline. The second cycle of the figure is the abnormal one. The auricle and ventricle are represented; the ventricle yields a perfectly normal complex, Q, R, S and T. Directly preceding the ventricular contraction, which is premature, is the representative of the abnormal auricular contraction, P. It may be compared with that of the preceding cycle. The shape of P in the abnormal cycle demonstrates that the auricular contraction has arisen at a point removed from the pacemaker. The third cycle of the figure occurs after a long pause and is normal in every respect. (× $\frac{2}{3}$.)

The Premature Auricular Contraction.—You are aware that a pulse may be irregular as a consequence of isolated disturbances occurring at intervals far removed from each other. These disturbances, often spoken of as *intermittences*, are usually the result of *pathological impulse formation* in the heart, and the abnormal impulses may arise in auricle and ventricle, and so far as we know in any part of them. Now, if the premature beat arises in the auricle, it causes and is succeeded by a premature ventricular contraction. In the electric curve it consequently gives rise to an auricular and to a ventricular complex. Except in exceptional instances, which I do not propose to consider, the ventricular complex is a duplicate of the ventricular complex of the normal and rhythmic beats in the same case; and this might have been anticipated, for we know that the impulse generated by the abnormal premature auricular contraction can only spread to the ventricle along the normal paths. The auricular complex of the abnormal beat, on the other hand, has a very variable form, and rarely duplicates the normal auricular complex of the same case. The reason for this is evident. The beat has started, not at the normal pacemaker, the junction of superior cava and auricle, but at some other point, and the contraction *in the auricle* has taken an abnormal course (fig. 5).

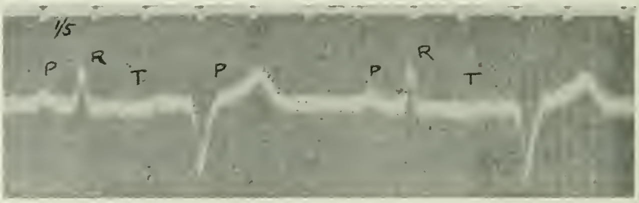


Fig. 6.

From a patient showing premature contractions arising in the ventricle. Each normal cycle is followed by an abnormal cycle. Each normal cycle is represented by the usual variations P, R and T, and is followed by an abnormal cycle representing the ventricular contraction. The abnormal cycle consists of two main variations, one in the downward, the second in the upward direction. The sequential auricular contraction falls in the centre of the premature ventricular contraction. The type of premature contraction shown arises in the apical or left portion of the ventricular musculature. (× $\frac{2}{3}$.)

The Premature Ventricular Contraction.—When a premature contraction arises in the ventricle the abnormal contraction is confined to this chamber. There is no disturbance of the regular auricular rhythm and the P variations are placed throughout at regular intervals. The

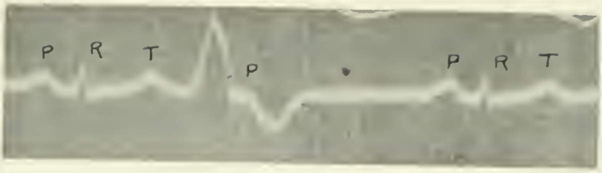


FIG. 7.

A single premature contraction arising in the ventricle. Two normal beats are shown, and are represented by P, R and T variations. The first is followed by an abnormal ventricular complex, of which the first phase is directed upwards and the second downwards. The sequential auricular contraction falls in the centre of the ventricular contraction, and is represented by a wave on the downstroke. This type of premature beat is recognized as one arising in the basal or right portion of the ventricular musculature. (× $\frac{2}{3}$.)

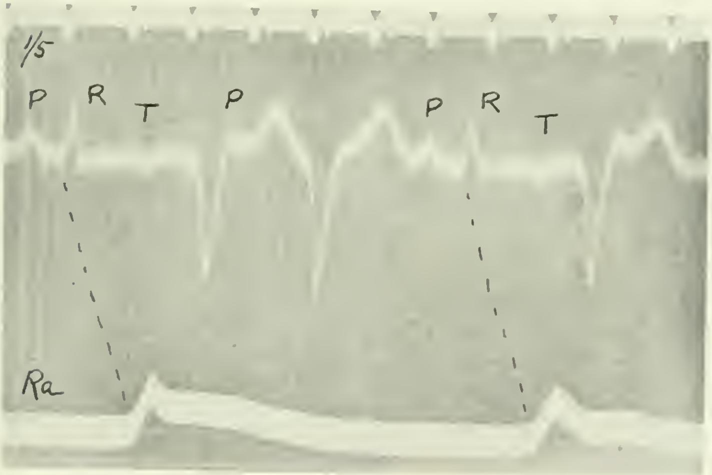


FIG. 8.

Shows premature contractions of the same type as those illustrated by fig. 6. The two figures are from the same case. A radial curve is also included. The premature contractions are more frequent and occur in groups. The figure is given to illustrate two points. First, to show the absence of effect of the abnormal ventricular beats upon the radial pulse; secondly, it is given for the comparison of the outlines of the first two abnormal ventricular complexes. They are not quite similarly shaped, and this is due to the fact that the sequential auricular contraction falls upon the first abnormal ventricular complex, and that the second is free from this complication.

ventricular complex, on the other hand, is no longer of normal form, but varies markedly from it; it may be of many shapes. Usually the normal P variation and the abnormal ventricular complex fall together and superimpose. The shape of the ventricular complex gives a clue to the actual point of the ventricular muscle from which such a beat has arisen; it is possible to obtain approximate experimental duplicates of such curves, but at present the localization is imperfect (figs. 6—9).

In the preceding paragraphs we have considered two of the commonest types of pulse irregularity met with clinically—the premature auricular and ventricular contractions (the auricular and ventricular extra-systoles, as they have often been termed). We may now discuss a rarer form of disturbance.

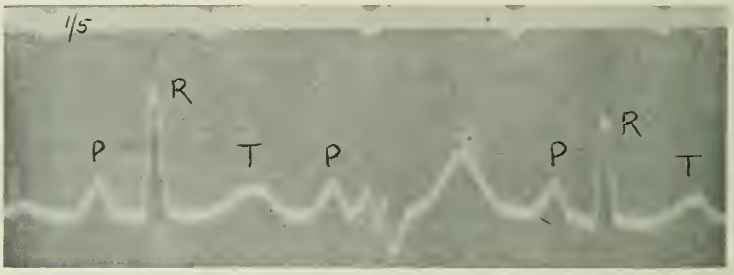


Fig. 9.

An electrocardiographic curve showing a single premature contraction of the ventricle. The first cycle is normal, consisting of P, R and T variations, and so is the last. The sequential auricular contraction, the middle P of the figure, is followed by an abnormal ventricular complex. This abnormal complex represents a ventricular contraction which is not a response to the preceding auricular contraction. This is known from the shape of the curve. It is also evident from the relative shortness of the interval between its commencement and the commencement of the preceding P. From a dog.

A Form of Paroxysmal Tachycardia.—Not infrequently one meets with patients who are the subjects of a well-defined and specific type of paroxysmal tachycardia. The pulse, while beating at a normal rate, becomes abruptly accelerated to rates approaching 150 to 200 beats per minute. The new rhythm is regular, and may be continued for shorter or longer spaces of time (from a few seconds to weeks or even months). It terminates, as it started, quite abruptly; an intervening pause is felt or recorded, and the normal rhythm immediately returns.

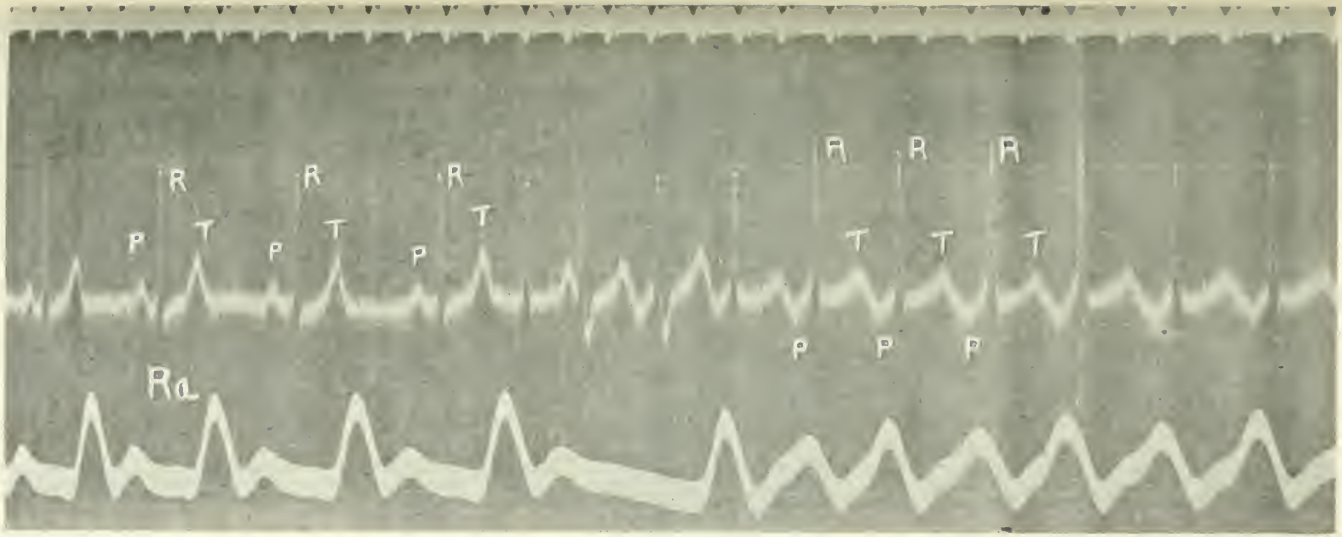


FIG. 10.—An electrocardiogram and radial curve from a case of paroxysmal tachycardia, arising in the lower levels of the auricular musculature. The curve starts with four cycles belonging to the normal rhythm. Each cycle is represented by P, R, and T variations. The paroxysm starts in three curious and anomalous beats, two of which have little effect on the radial curve. When fully established, the paroxysmal beats are represented in the electrocardiogram by a normal ventricular complex, R and T. The shape of these ventricular complexes indicates that the paroxysmal beats have originated from the supra-ventricular portion of the heart musculature. Each ventricular complex is preceded by an inverted P, showing that the contraction in the auricle has taken an abnormal course. It has travelled from below upwards, instead of from above downwards. The paroxysm originated, in all probability, in the neighbourhood of the auriculo-ventricular node.

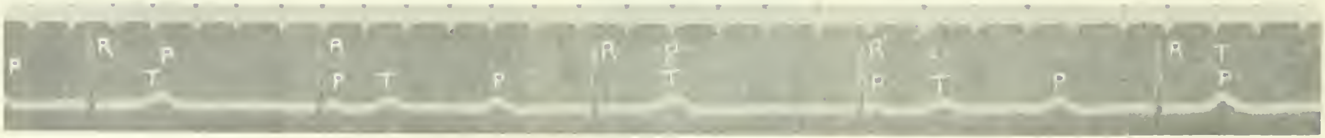


FIG. 11.—An electrocardiogram, showing the last stage of heart block; complete dissociation of the auricular and ventricular rhythms. The ventricular beats, R and T, are placed at regular intervals. The rate is approximately 50 per minute. The auricular contractions, P, also occur at regular intervals; the rate is approximately 78. The summits, P, fall with haphazard relationships to the ventricular complexes; frequently they fall with them; in fact, each ventricular contraction of the curve is complicated by the simultaneous occurrence of an auricular systole. The P variations fall with the ventricular complex at various points, and the auricular and ventricular representatives are superimposed one on the other. $\left(= \frac{7}{10\cdot5}\right)$.

Galvanometric examination of such patients has shown that the fast rhythm of the paroxysm is made up of a number of those beats which we have already examined—namely, premature contractions. Further, it has shown that the paroxysms may arise from a number of foci in the heart muscle, from one or other ventricle, from the junctional tissues, or the auricle itself. The new rhythm has arisen in every case as yet examined from a point in the musculature other than that in which the normal pacemaker lies. This is known by the shape of the electric complexes, auricular or ventricular respectively. In brief, the type of paroxysmal tachycardia referred to is the result of new and pathological impulse formation at an abnormal point. The new rhythms are *ectopic* in origin. It will suffice if a single instance is given (fig. 10).

Auricular Fibrillation.—The commonest of all irregularities of the human heart is that which has been termed *delirium cordis.* It is an irregularity of extreme grade; small and large beats are mixed together in the greatest confusion and at widely divergent intervals. It is especially associated with rheumatic heart disease, and with the widespread and degenerative cardiovascular changes of advancing years.

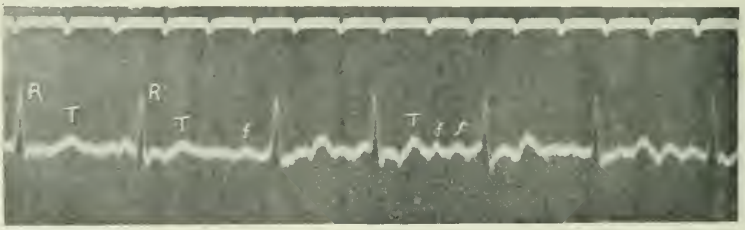


FIG. 11.

An electrocardiogram from a case of fibrillation of the auricle. The ventricular beats are represented by distorted complexes, R and T. The shape of the complexes indicates the supraventricular origin of the impulse creating them. Preceding the ventricular complex there is no regular P variation, representative of auricular contractions under normal circumstances. On the other hand, such P variations are replaced by a series of irregular oscillations, which run throughout the whole curve. They are marked *f f*. These oscillations originate in the auricle, and they produce the distortion of the T variations, which is noticeable in this figure. ($\times \frac{2}{3}$.)

Galvanometric examination of patients afflicted in this manner has shown that we are dealing with a perfectly definite and a single and specific disorder of the heart's mechanism. It has shown in the most conclusive fashion that the mechanism is one which experimentalists have termed "auricular fibrillation." Auricular fibrillation is a condition in which the normal and co-ordinate contraction of the auricle, starting from the pacemaker, is replaced by an inco-ordinate condition, in which the auricle remains in diastole, and in which the whole of its musculature is in a state of subdivided activity. It appears as if impulses are initiated in many foci in its walls, and as if these impulses give rise to a turmoil of confused and interfering contractions, no single one of which is effective in expelling the auricular contents. It is from the auricular musculature, thrown into this disorder, that haphazard impulses escape to the ventricle. They escape irregularly, and consequently the ventricular contractions follow each other irregularly. The electric effects in fibrillation may be divided into the two usual components, the auricular and ventricular. The auricular effects consist of a number of rapid and irregular oscillations. One of the striking features of these oscillations is their continuation throughout the whole cardiac cycle, a fact in accord with our knowledge of fibrillation. They completely replace the P variations of the normal rhythm. The ventricular portion of the curve is constituted by complexes consisting of variations R and T, complexes which are placed at irregular intervals. From the shape of the ventricular complexes we know that the ventricular beats have arisen as a result of impulses reaching them along the normal paths: this is as we should expect, knowing as we do that the impulses are generated in the auricle (figs. 11 and 12).

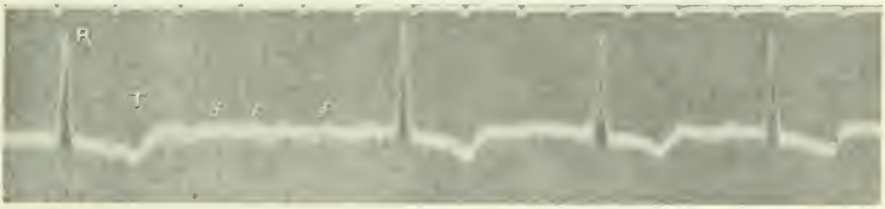


FIG. 12.

An electrocardiogram of a case of auricular fibrillation. The ventricular beats, R and T, are placed irregularly in the curve. The shape of T is noteworthy; it is of the inverted type. The inversion is often held to indicate a severe myocardial lesion. The absence of the normal auricular variation, P, before each ventricular complex is striking. The oscillations which replace the normal representatives of auricular systole are less marked than in the preceding figure, but are quite distinct. ($\times \frac{2}{3}$.)

Heart-block.—Another disturbance of the cardiac mechanism, which is by no means so uncommon as was originally supposed, and which is excellently portrayed by electrocardiographic curves, is heart-block. You know that the ventricle receives its impulses from the auricle through the recently discovered tract of tissue known as the auriculo-ventricular bundle. It often happens that disease hinders the conduction of these impulses in varying degree, and such obstruction often leads to very grave circulatory conditions. The several grades of heart-block are as follows:—

(1) The earliest sign of impaired function is manifested by a prolongation of the interval which separates the commencement of auricular and ventricular systoles, the P—R interval, as it is termed, in the electric curve.

(2) At a later stage there may be a failure of response rather than a delay of it; a ventricular response to auricle is completely missed at intervals.

(3) Further stages consist in more frequent ventricular silences, so that the auricle beats two, three, or more times as frequently as the ventricle.

(4) The final stage is one of complete dissociation of the auricle and ventricle, each beating regularly and of its own accord, independently and at different rates (figs. 13 and 14).

In the preceding paragraphs I have given you a few of the more simple forms of cardiac disorder in illustration of the galvanometric method, and I do not propose to exemplify the subject further. But it should be understood that the knowledge which the instrument affords us does not end here. I have not touched at all upon a wide field—the examination of hearts in which the ventricle beats regularly and in which the sequence is normal. It is perhaps premature to speak of them in a paper which merely pretends to outline the subject; but it is only fair to the instrument to state that it promises well in this direction. Those who are specially studying the electrocardiogram see that in the near future it may permit of the diagnosis of the auricular hypertrophy of mitral stenosis, and, as a consequence, the valve lesion itself, at so early a period of the life-history of the disease that the ordinary physical signs are wanting. There is also very suggestive evidence that it will supply us with more certain means than we enjoy at present of ascertaining and dissociating hypertrophy of one or other ventricle. These questions may be left for a future occasion.

I may conclude by summarizing the present position of the galvanometer in its relationship to the practice of medicine.

A SUMMARY OF THE RELATIONSHIP OF ELECTROCARDIOGRAPHIC WORK TO THE PRACTICE OF MEDICINE.

An estimate of the practical value of a single method of physical examination is at no time easy, and it is especially difficult when we deal with a method which is still in its infancy. In offering my own impressions of the relationship of the electrocardiographic methods

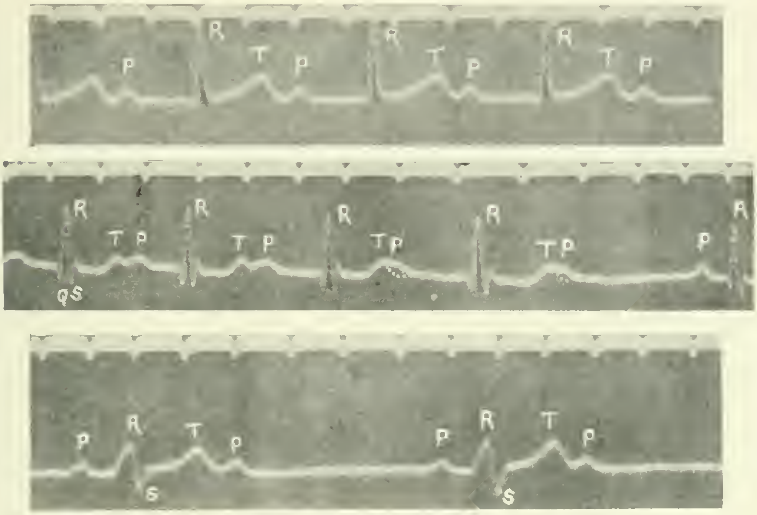


FIG. 13, *i*, *ii* and *iii*.

Three clinical curves to illustrate stages of heart-block. In fig. 13, *i*, the heart is beating regularly and the sequence is normal, in so far as each ventricular contraction, R and T, is preceded by an auricular contraction, P. The curve is nevertheless abnormal; the P—R interval is increased from the normal 0·12 to 0·17 sec. up to 0·3 sec. Fig. 13, *ii*, is from a case of mitral stenosis on digitalis. It is an example of dropped beats. The ventricular complexes are represented by the variations, Q, R, S and T, and in the early stages of the curve each ventricular beat is preceded by an auricular contraction, represented by P. It is to be noted that as the curve proceeds the P—R interval gradually increases until P falls back and is superimposed upon the preceding variation, T, of the ventricular contraction; the interval, P—R, for this cycle is nearly 0·3 sec. Up to this point the auricular impulse has had increasing difficulty in reaching the ventricle; the dropped beat is the final expression of this hindrance. At the end of the figure the next response of ventricle to auricle is shown. After the long rest the P—R interval decreases markedly. Fig. 13, *iii*, is from a case of heart-block in which the ventricle is responding to alternate beats of the auricle. The auricular complexes, P, are twice as numerous as the ventricular complexes, R, S and T. The curve is remarkable in that it shows a prematurity of alternate P variations, an early occurrence of the auricular contraction immediately following each ventricular contraction. The explanation of this phenomenon is unknown. (× $\frac{2}{3}$).

to the practice of medicine, let it be understood that I give them as they appeal to me at the present time and after several years' study.

I should do wisely, I think, if I placed in the foreground, not the method itself, but the general conclusions which have been arrived at as a result of its employment, confining attention to those facts which have an immediate influence upon our conceptions of pathological conditions. Regarded from this point of view, our attention concentrates upon two conditions, which are usually referred to as *delirium cordis* and paroxysmal tachycardia respectively.

As a result of investigations carried out with the galvanometer, we are now in a position to assert in the most positive fashion that what has been termed in the past delirium of the heart is, in fact, a condition known to experimentalists by the name "fibrillation of the auricle." The clinical importance of recognizing this disorder of the cardiac mechanism can scarcely be exaggerated. It is directly accountable for 50 per cent. of all irregular pulses as they are encountered in a general out-patient department, and for an even higher percentage in the wards of a general hospital. In patients who are the subjects of heart affections attributable to a rheumatic infection, and in elderly patients who succumb as a result of progressive sclerotic myocarditis, the final breakdown is often, if not generally, heralded by the onset of this curious, distinct, and easily recognizable mechanism. The importance of identifying it is obvious, not only on account of its universality, its profound influence upon the life-history of the heart affected by it, but equally because, being a mechanism *sui generis*, it reacts to cardiac drugs in a specific manner. This is not the place in which to refer to the disorder in greater detail, but only to emphasize the imperative need of an acquaintanceship with the condition, and to claim for the galvanometer its final isolation and analysis.

I pass to the second condition, namely, paroxysmal tachycardia. Formerly it was regarded as of obscure origin, and frequently attributed to an affection of the cardiac nerve-trunks or their connexions. Now we possess clear evidence that cases which fall within this category may be placed in several distinct classes. Of these classes, two are peculiarly striking. In the one, the paroxysms of tachycardia consist of temporary crises of auricular fibrillation. There are paroxysms of rapid heart action, in which the ventricular beats follow each other irregularly. In the other form, which I have already briefly described, the paroxysms depend upon the awakening of new impulse formation in the heart musculature, and in some fixed point at a distance from the normal seat of impulse formation. It has been ascertained that the attack is the result of a temporary dislocation of the site of the *primum movens*; or, perhaps, as it may be expressed more correctly, it is caused by the extinction of the rhythm of the normal pacemaker at the development of a new and more rapid rhythm in a point some distance removed from that which originates the normal rhythm. For the elucidation of these facts we are indebted to the new method. And, it may be stated in parenthesis, that our knowledge of the actual site of origin of the normal mammalian heart rhythm is largely referable to the same source. The normal rhythm of the heart arises, as we now know, from a small area of the right auricle immediately abutting upon the entrance of the superior vena cava.

I have placed these additions to our general knowledge in the forefront of the account rendered by the galvanometer, because they must necessarily and immediately modify our conception of, and our attitude towards, many clinical conditions, and because they form additions to the chapter of cardiovascular pathology which we can ill afford to neglect. Even a summary perusal of the multitude of the newly acquired facts furnished by the instrument has been impossible in the time at our disposal, and in this brief account these isolated examples have been stressed, not only because I am in a position to speak with a first-hand knowledge of them, but because they appeal to me as constituting generalizations, which cannot fail to receive general clinical application in the near future.

In closing an outline of the general conclusions in respect of which the new method debits us, we cannot lose sight of its important bearing upon other and more strictly clinical methods of examination. It may be stated that the galvanometer has already substantiated the great bulk of the facts previously acquired by means of the polygraph. It is true that it has at times led to conclusions at variance with previous conceptions, but the point which should be specially emphasized is that the galvanometer shows in the most unequivocal manner that we are fully justified in accepting the present-day interpretation of the vast majority of venous pulse records; and that the older method yields analyses of the heart mechanism which are almost always upheld if submitted to the arbitration of this new instrument; and the importance of this conclusion will be more evident when it is remembered that the galvanometer is essentially a laboratory instrument, while the polygraph accompanies the clinician to the bedside of his patient. From this point of view the polygraph may be regarded as a go-between, obviating the necessity of transplanting a cumbersome apparatus.

Of the more immediate benefits conferred by the instrument I speak more hesitatingly. The size, weight, and cost of the galvanometer and its fittings materially restrict its sphere of usefulness. It has to be maintained as a fixed installation, and it is difficult to see how it can ever leave the immediate precincts of the laboratory or hospital. These disabilities are reduced to some extent by the possibility of bringing a relatively large area within its immediate working sphere. The instrument may be connected by properly insulated wire to any ward or room within a convenient range. A single outfit will consequently supply a single institution or several institutions in its immediate vicinity. As a part of the equipment of a modern hospital or physiological laboratory it is already almost a necessity. Once installed it gives a ready and infallible analysis of the sequence of contraction of the cardiac chambers. A few minutes (three or five) places the clinician in full possession of all the necessary data for the analysis of an arrhythmia in all but exceptional instances, and the data are of the most accurate nature. In several respects it presents material advantages over the polygraphic method. Its superiority as an instrument of precision is unquestioned, a quality which is dependent upon the directness of the method. A venous record is an expression of an effect of the heart upon venous inflow; the electric curve is an expression of the heart-beat itself. A second and considerable advantage consists in the ease with which the majority of the records are read; polygraphic curves often require minute and often tedious measurement; the majority of electrocardiograms may be read at first sight. In the third place it gives valuable information, not only as to the instants at which auricle and ventricle contract, but as to the direction which the contraction waves pursue in individual chambers, and consequently as to the points at which such contraction waves arise in the chambers. In brief, it is a weapon which has no equal for precision where an analysis of the cardiac mechanism is required; it takes precedence of all other methods where evidence is conflicting; the information obtained is secured with a minimal expenditure of time.

The comparison of one method of physical examination with another is always difficult, but restricting comparisons to hospital patients, I have little hesitation in stating that in the routine examination of the heart patients the galvanometer method affords, or will afford in the near future, information of equal or greater value than any other single method at our disposal, be it instrumental or subjective. It must be allotted at least an equal place with percussion and auscultation, with sphygmomanometry and radiography; and in this respect it has this to be said for it, that the information it offers it offers definitely, the information it withholds it withholds as definitely; it is, in fact, a method of exactitude, a method of precision.

DISCUSSION.

The CHAIRMAN (Sir William Allchin) said it was unfortunate that the President of the Section was unable to be present, for Dr. Mitchell Bruce was among the few who were able to make remarks at all pertinent to such a contribution as Dr. Lewis had just delivered, as such work was naturally confined to a few; and one could not help admiring the very lucid way in which Dr. Lewis had put forward a very complicated subject. One also must realize the amount of work he must have undertaken to enable him to speak in so clear a manner on the subject. This work contributed from another direction towards what in so many quarters was being done to improve diagnosis and investigate with greater precision the phenomena of disease.

Dr. GOSSAGE said it was difficult to speak on the subject, because he, and probably all others in the room, had had no experience of working with the instrument which Dr. Lewis had described. Once he had the great advantage of seeing Dr. Lewis take an electrocardiogram from one of his (Dr. Gossage's) patients; an obscure case, on which the record threw much light. Probably most of the Fellows would agree with the conclusions at which Dr. Lewis arrived. The evidence given in the paper was only a small part of what the author and others had in the background, evidence which completely justified everything which had been said. The question as to the employment of the electrocardiogram as a clinical instrument was involved in more difficulty; it required much education before the instrument could be used; moreover, it was an instrument only suited to an institution, to which the patient must be brought. The apparatus could not be possibly conveyed to the patient.

Dr. ALEXANDER MORISON said he felt the same inability to criticize the excellent paper which had just been heard, but wished to express his admiration of the work which Dr. Lewis had done. That gentleman had excluded from his paper all controversial matter, and he (Dr. Morison) had no intention of importing any. But he would be glad if, in his reply, Dr. Lewis would make clear the circumstances which caused him positively to assert the necessary initiation of a delirium cordis in the auricle. One knew that in the ordinary examination of a delirious ventricle one had a large number of aborted diastolic periods, and as the blood must go somewhere, there was probably an accumulation in the auricle. He was still inclined to regard the auricular comparative inactivity as due more to a mechanical over-filling than the arrhythmical ventricular action due to a rapid multiplication of the stimulating impulses arising in the auricle. All would agree that no more important work had been done in the matter of the minute anatomy of the heart than had been done of late years in elucidating the functions of the conducting apparatus.

He did not know that all the conclusions which had been based upon it were sufficiently sound to warrant an ex cathedra interpretation of them. With regard to the sino-auricular node or pacemaker, he had seen that part of the heart disabled by sarcomatous growth without disturbing the rhythm of the ventricle. He did not know whether Dr. Flack's results were generally accepted, but the effects of its destruction of that node supported his own observations and did not seem to be followed by the consequences one would anticipate from current interpretations of its function. It was argued there was some similar source of origination of impulse elsewhere, as the destruction of the node did not seem to have much disturbed the function of the heart. He would like to hear the grounds on which Dr. Lewis was able to state, from his electrograms, that a case of delirium cordis necessarily commenced in the auricle.

Dr. LEWIS, replying to Dr. Morison, said that the evidence for auricular fibrillation in man was a long story, and that he would content himself with a brief résumé of the main points in that evidence. The picture which obtained in cases of so-called delirium cordis was always of the same character; the records were always similar. He based this statement upon the electrical examination of sixty cases, and upon a large number of cases collected from other papers. With regard to the shape of the ventricular complexes, they were of normal outline, consequently it could be concluded that individual beats originated from a supra-ventricular portion of the heart—i.e., from the auricle or from the tissues which joined the auricle and ventricle. That contention would be accepted by those who were acquainted with electrical work. All signs of auricular contraction had gone, and they were replaced in all cases by a series of oscillations which ran throughout the whole curve. One knew that the large variations marked R and T were due to the ventricle, and one might ask what the oscillations were due to. Their point of origin of those oscillations was clinched by the employment of special leads from the chest wall. Where there was mitral stenosis, and the right auricle was against the wall of the chest, one could identify the point of origin in that area of chest wall overlying the auricle itself, whereas one could get uncomplicated ventricular curves from the parts of the chest which overlay the ventricles. The oscillations were not representative of co-ordinate auricular contractions, but were the total result of the whole electrical changes occurring in a chamber in which the muscular activity was continuous and inco-ordinate. It was practically a diastolic auricle, but the wall of the auricle was continually active; it was flickering all over. Again, the comparison of the clinical with the experimental curves produced by auricular fibrillation in the dog had demonstrated that arterial, venous and electric curves were identical in one or the other condition. Recently he had had several opportunities of seeing the same irregularity of the heart in the horse, and in one case of obtaining records from this animal; he had opened the chest and had seen the fibrillation on two occasions. The ventricle was beating rapidly (50 to 150 beats per minute, as opposed to the normal rate of 35 to 40 beats per minute) and extremely irregularly in the animals upon which these observations were made.

A Case of Acute Suffocative Catarrh of Pneumococcal Origin and Fatal (Pneumococcal Bronchitis); a Second Case of Pneumococcal Bronchitis, Unilateral, of long Duration and not Fatal; with other analogous Cases and Remarks.

By SAMUEL WEST, M.D.

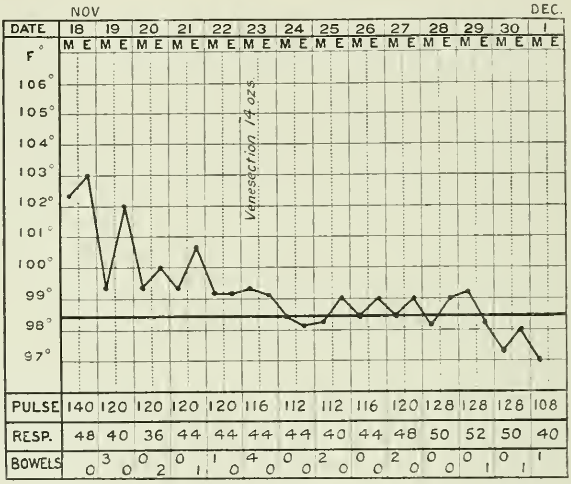
H. W., AGED 31, a carman, was brought into the hospital on November 18, 1910, with great dyspnœa. He was deeply cyanosed and very gravely ill. He had been in his usual health until November 14, four days previously, and then rather suddenly developed cough with abundant expectoration, which he said had a peculiar bluish colour. Though ill he continued at work until November 17 (three days), when he had some shivering and went to bed. That night he became delirious and the next morning he was brought to the hospital and admitted. He always considered himself strong and active, but he had pneumonia at the age of 17 and an attack of bronchitis at 24, for which he was a patient in the Chest Hospital. Eighteen months ago he was in St. Bartholomew's Hospital (Mark Ward) with another attack of bronchitis, from which he completely recovered. He remained well till the present illness, and was not considered between the attacks to have any weakness of the chest. He was a well-developed man and his general condition bore out his statement that he was usually in good health.

On admission the patient was deeply cyanosed and suffering considerable dyspnœa. The temperature was 102° F., and rose that evening to 103° F.; the respiration varied between 36 and 48; the pulse was 144. Physical examination revealed nothing except the signs of general bronchitis; the heart seemed sound, and there was no evidence of dilatation; the arteries were normal, and the urine contained no albumin.

The chief feature of the case was the profuse expectoration; this amounted to several ounces (12 to 15) in the twenty-four hours. Though frothy in parts the sputum was not simply bronchitic, for it consisted in the main of muco-purulent matter of a greenish colour and viscid, but containing no blood. It proved to contain large numbers of pneumococci, and on cultivation yielded an almost pure culture of pneumococci.

The patient was desperately ill, and seemed likely to die of suffocation. Oxygen was at once administered, stimulants given in the form of brandy, citrate of caffein and strychnine, and counter-irritation applied to the chest by hot applications of spirits of camphor. The night was a bad one, but the next day the patient seemed better, the temperature being a degree or so lower, the respiration having fallen to 40—36, and the pulse to 116—120.

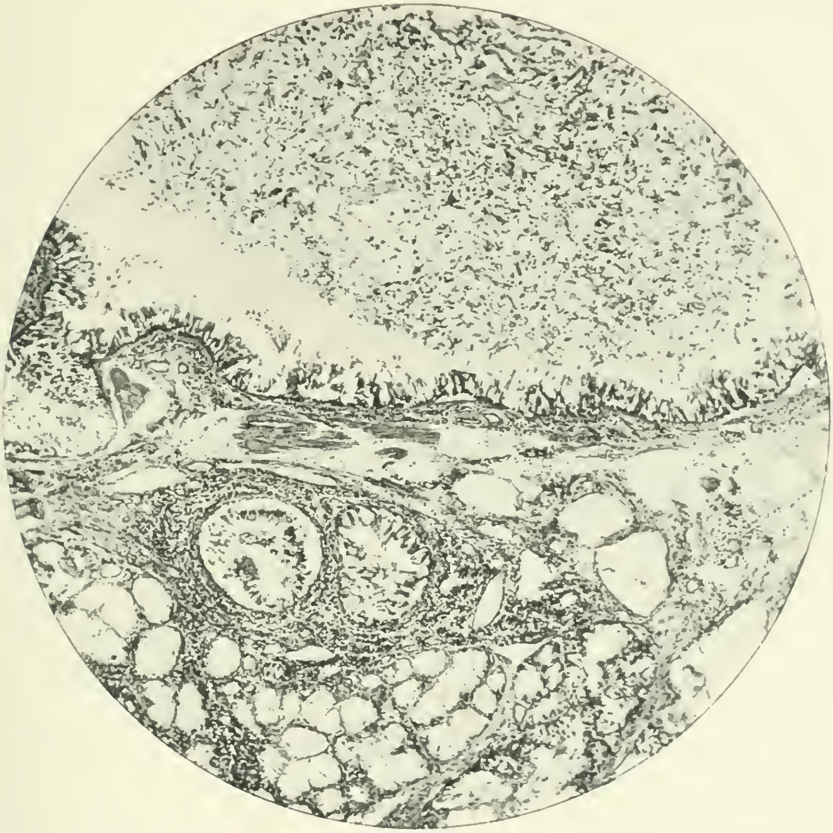
The case was diagnosed as one of acute suffocative catarrh of the adult, of pneumococcal origin. The physical signs and sputum remained the same for the next few days, the temperature gradually falling, until on November 21 it reached the normal; the expectoration diminished somewhat, but was still very abundant and of the same character, while the respiration rate was about 44.



Temperature chart of case of acute suffocative catarrh.

On November 23 the cyanosis became worse, and the dyspnœa was so severe that *venesection* was performed to the extent of 14 oz. This gave great relief, but did not reduce the respiration-rate. *Ten c.c. of Merck's pneumococcic serum* were now injected. It seemed to be without effect for good or ill. From November 23 to November 26 the patient seemed to hold his own, but on November 26 the pulse rose to 120—130 and the respiration to 48—52. The cyanosis and dyspnœa increased and the temperature showed marked daily fluctuations, dropping sometimes to 97° F., but not rising above 99·6° F. The patient's strength gradually failed, and on December 1 he died of exhaustion and suffocation, having been ill just fourteen days.

The case was diagnosed as acute suffocative bronchitis of pneumococcal origin—or, as it may be better described, acute pneumococcal bronchitis.



Photomicrograph of lung (2 × 50), to illustrate condition described in the text.

I recorded an exactly similar case to this, except that it recovered, in the *Proceedings of the Royal Society of Medicine* of 1908,[1] but in that case the germ discovered in the sputum was of an indefinite character, resembling the diphtheria bacillus.

For the complete and careful pathological report I am indebted to Dr. Gow.

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[1] *Proceedings*, 1908, i, p. 151.

Post-mortem Examination.

The lungs were bulky and emphysematous. The right lung weighed 35 oz. and the left lung 32 oz.; the whole lung tissue was acutely congested. No pleurisy and pneumonia seen anywhere. The greater part of the left upper lobe was in a state of hæmorrhagic infarction of recent origin; a similar change being present in both bases, but in smaller areas. The bronchi were thickened, somewhat dilated, and were full of thick pus, which could be squeezed out of quite small tubes on section. The heart weighed 14½ oz.; pericardium normal; the right auricle and ventricle dilated and the muscle of both chambers thickened; the tricuspid valve was stretched, admitting four gloved fingers; the cusps were not thickened; other valves natural; myocardium of left ventricle normal. The liver, the spleen, and kidneys were congested, and so also were the meningeal vessels.

Histological examination of the lung: The alveolar walls were stretched, and this especially in the area of the hæmorrhagic infarct; there was no microscopical evidence of pneumonia or of tuberculosis. The alveoli contained a small amount of granular exudate, largely not fibrin, but probably albuminous, and there was very little desquamation of the alveolar epithelium. The chief changes were found in the bronchi. The small bronchi—those devoid of cartilage—were in a condition of acute and severe bronchitis; their lumen was blocked with desquamated epithelium and polymorphonuclear leucocytes; their walls showed marked inflammatory infiltration, composed mainly of small round mononuclear cells, the polymorphonuclears being comparatively few in number. In the large bronchi the epithelium was less extensively shed, but even here the lumen was blocked by polymorphonuclear and epithelial cells and granular exudate. There was intense inflammation of the bronchial wall and mucous glands, but here again the infiltration was composed of small round cells in contrast with the polymorphonuclears seen in the lumen. There was great hyperplasia of lymphoid tissue around the bronchi and vessels. The pleura showed no thickening, nor was there any lymphoid increase beneath it.

Bacteriological examination: Bronchial contents—Films showed Gram-positive diplococci, capsulated and with pointed ends, indistinguishable morphologically from the pneumococcus; no other organisms seen; culture gave a pure culture of pneumococcus; the same result was obtained during life from examination of the sputum. Heart's blood—No organisms were seen in films, nor was any growth obtained.

Meningeal exudate—Films and cultures negative. Besides the presence of hæmorrhagic infarcts of recent dates, and the absence of pneumonic consolidation, the post-mortem examination revealed nothing except severe bronchitis, of peculiar character, in that the secretion was less mucous, and consisted of albuminous non-fibrinous exudation, in which polymorphonuclear cells abounded, and which gave a pure culture of pneumococcus. The pathological report thus confirmed entirely the clinical conclusions.

The following is a somewhat similar case, but of much longer duration and less severe intensity, and not fatal :—

B. H., aged 55, was admitted into St. Bartholomew's Hospital, complaining of pain in the right side and spitting of blood. She had been in her normal good health until November 10, when she was seized suddenly in the night with pain in the right side. Shortly afterwards she spat up blood, frequently, but never more than a teaspoonful at a time. She kept at rest for a few days, and getting no better sent for a doctor, who diagnosed "bronchitis and gastric ulcer." After ten days' illness at home she came to the hospital and was admitted on November 20. She was very weak and ill and was carried into the ward. Temperature, 98° F.; respiration, 32; pulse, 88. She was rather dusky and had obvious dyspnœa. The tongue was thickly coated; the urine contained no albumin; and the knee-jerks were brisk. Over the whole right side rhonchus and sibilus were present, with a good deal of medium-sized crepitation, which in the mid-axillary and mammary region, where she located her pain, might have been pleuritic. The sputum was frothy, muco-purulent and blood-stained; unlike ordinary bronchitis in being so viscid, and of a greenish colour. The blood was present in some parts only of the sputum, where it was rusty-red and uniformly distributed like pneumonic sputum, but not in small pellets. The sputum was neither typically bronchitic nor typically pneumonic in character; the total amount expectorated was considerable —several ounces in the twenty-four hours.

The patient was obviously very ill, yet the general condition and temperature was not like that of pneumonia, nor were the signs those of ordinary bronchitis, for though the signs of bronchitis were present over the whole right lung, they were absent from the left lung, except for some crepitation at the base behind.

On December 1 films of the sputum were stained and showed numerous pneumococci, and a few staphylococci and streptococci.

Subsequent examination showed the presence almost exclusively of pneumococci. On December 2 the percussion note was impaired over the whole right side, though there was no absolute dullness anywhere. The areas of dullness varied from day to day, and the conclusion arrived at was that there were some disseminated patches of consolidation of the broncho-pneumonic type, with transient and varying collapse of the intervening parts. The temperature remained high and the condition did not improve, and on December 16, 10 million dead pneumococci were injected *sub cutem*, but seemed to have no effect for good or bad. A second injection of 30 million was made three days later without obvious effect. The expectoration continued profuse and of much the same character, except that less of it, and subsequently none of it, was rusty. The general condition was very indefinite, the features being the continued bronchitis on the right side with a little at the left base, the continued fever, and the copious and peculiar sputum. The dyspnœa and cyanosis were considerable, and at one time life seemed in actual danger from suffocation. The general condition varied in relation with the temperature; this gradually rose after admission till it reached 100·8° F. on November 22, and it continued round about 101° F., sometimes higher and sometimes lower, till December 6, seventeen days from admission. Then it took a lower mean, but did not fall to normal till December 28, and remained unsteady for a few days, after which it became subnormal, and convalescence began. The respiration-rate throughout was 24 to 32, and the pulse-rate about 90. The sputum, when examined bacteriologically, showed practically nothing but pneumococci.

The case was one of pneumococcal bronchitis, limited to the right lung, with small secondary scattered patches of consolidation in various parts. The illness was grave and lasted about six weeks, during the early part of which, though the fever was not high, the patient was very seriously ill, and in considerable danger. It was quite an unusual case clinically, and belongs to the same category as the preceding case, from which it differed in being not general, nor so acute, but of much longer duration.

A somewhat similar case to this has recently come under my observation after severe measles. A young lady, aged 18, had a sharp attack of measles, the rash being of a dark purple colour, though not actually hæmorrhagic. Two or three days after the rash appeared the temperature rose again, and acute bronchitis set in. Clinically it appeared to be a case of secondary pneumonia, but no definite physical signs of consolidation could be found. The expectoration was profuse, and was in parts rusty. The bronchitis was extreme; the patient became rapidly cyanosed, and suffered greatly from dyspnœa. She quickly passed into a hopeless state, but owing to her youth and vigour life was prolonged many hours after all hope was given up.

An almost identical case, except that it was of spontaneous origin, like an ordinary pneumonia, occurred in a man, aged 45, the subject of chronic phthisis. In him no definite signs of consolidation could be made out, but from the commencement the general signs of bronchitis predominated, though clinically it was obviously a case of pneumonia.

The association of bronchitis with pneumonia is always serious, but the gravity depends upon the relation between them. Pneumonia occurring in the course of an ordinary bronchitis is an awkward complication, but not necessarily fatal. If, however, the signs of bronchitis develop in the course of pneumonia, the result is almost invariably fatal. The meaning of the bronchitis in the two cases is different. In the first it is merely the association of two independent diseases; in the second the bronchitis is a sign of pulmonary failure, the result of the strain thrown on the lungs by the pneumonia, the lungs having done all they could to cope with the pneumonia, but failed. The signs of the failure are those of bronchitis. This is sometimes called secondary congestion of the lung. But this congestion of the lung may be primary. It may be coincident with the onset of pneumonia, and produce very grave symptoms. It may even be associated with hæmoptysis, which is sometimes considerable. Yet as the consolidation develops in some part of the lungs the primary general congestion may subside, and the physical signs gradually limit themselves to those of ordinary consolidation.

This may be compared to the widespread initial swelling which occurs in the subcutaneous tissue precedent to the development of a boil, and which rapidly disappears so soon as the infection localizes itself. If when the pneumonia localizes itself this general congestion still continues, it constitutes a very grave complication. This general congestion, bronchial as well as pulmonary, may also exist independently of any or of anything but slight consolidation. It is then, as these cases show, a condition of the greatest gravity.

The case with which I introduced this paper, as well as that brought before the Section in 1908, were instances in the adult of Laennec's acute suffocative catarrh. In the earlier case I suggested that this affection was really an acute bacterial infection. The second case now recorded confirms this view, and the infective organism is the pneumococcus, as is also the fact in the next case described. These cases might, then, be more accurately described as acute pneumococcal bronchitis, but of course it does not follow that in all the cases of acute suffocative catarrh the infective organism will prove to be the pneumococcus, though probably this will turn out to be the most common. So far we seem justified in holding that pneumococcal infection of the lung may present itself in different forms, either as massive consolidation, the usual form in the adult, or as disseminated patches, a common form in children (primary multiple broncho-pneumonia), and lastly, as these cases show, as general bilateral or even unilateral bronchitis. If this be so, it is obvious that we must greatly widen our conception of the pneumococcal infection of the lungs.

Medical Section.

April 25, 1911.

Dr. J. MITCHELL BRUCE, President of the Section, in the Chair.

The After-history of Cases of Albuminuria occurring in Adolescence.

An Address Introductory to a Discussion on the Subject.

By J. F. GOODHART, M.D.

I AM here this afternoon, Sir, by your command. Our President, Gentlemen, appears to be a sort of He who must never be disobeyed, so I responded, not because I knew much about the subject, or have anything new to tell, but because I think the time may have arrived for a further expression of opinion upon the after-history of adolescent albuminuria, which this meeting may very well give. To say that I know little about the subject is to give an opinion begotten of the humility that cometh after the fall, that is said to follow upon the exercise of pride; for I had thought that I did know something about the albuminuria of adolescents, but now that I have gone back to my notes I realize how scrappy and wanting in continuity of observation they are. However, my function is to unloose the tongues of others, and what I have to say will no doubt be sufficient for *that* purpose.

I shall take the liberty of not confining myself exclusively to the *old age* of my subject, for there are certain points in the history of its *youth* that bear upon the after-history, and in some measure determine the opinion one is disposed to form. I have based what I have to say upon all the cases I can find in my private case-books for the last twenty-seven years. These amount to 237 cases of all sorts that needed some consideration. This leads me to say that I had thought the condition to be an exceedingly common one, and so I believe it to be, for there is many a urine examined where the condition exists, but where the albumin is not present at the particular time of examination. I have had to exclude a good many of the 237 for various reasons; 206 were probably straightforward cases, 29 were cases the nature of which was doubtful, one or two of them being mature adults in whom the trouble had extended over some years with no apparent injury to health; 10 were probably hæmoglobinuria, &c. So that out of a total, say, of some 30,000 cases of all sorts of disease, I have not met with adolescent albuminuria so very often. Many times more often I must have looked for it, expecting to find it, and have not done so, and *only* not *done* so because I did not happen to hit the physiological moment. The *frequency* of the *disease* is one of importance with reference to after-histories, because if so very common, and you cannot show that in any number of cases it passes on into actual renal disease, or even into ill-health, an argument is supplied in favour of it being an unimportant condition.

My 237 cases include what has been understood by the several names—cyclic, intermittent, postural, functional, &c. This form of albuminuria may occur at any age. Of 192 cases, 32 were of 10 years and under; 65 were of 15 years and under; 61 of 20 years and under; 27 of 25 and under; and 7 under 30. I have at least two others, a man and a woman, older, already mentioned, but I have not included them, as they might seem too open to dispute.

But I like the term—I think it was Dr. Pavy's original one—"Cyclic," for I doubt if the after-breakfast or mid-morning periodicity of the albumin is to be explained purely by posture, or even by exercise superadded. Many have noted—I have seen it myself—the appearance of albumin after food later in the day. Of the influence of exercise there can be no doubt after Dr. Collier's, to my mind, splendid piece of work upon functional albuminuria in the *athlete*. And this opens up the cause of adolescent albuminuria. My own cases do not, I think, help much towards a solution of this question, so many of them have only been seen once or twice, and I have heard no more of them, but I think that as nearly everyone interested in the matter has noted, if you want to catch one of these cases *flagrante delicto*, of all times in the day the mid-morning hours are the time to choose for an examination. And it is astonishing, as others have also noticed, what large quantities of albumin may be exuded, if I may use such a term without prejudice. I have several times found as much as a half settle down in the test-tube, and yet at the next micturition there may be the barest trace or none at all. I am not quite sure if it has ever been proved that posture alone will determine albuminuria in these cases. Posture plus breakfast undoubtedly will, time after time; but what effect has an empty stomach in controlling it? But in addition to this mid-morning albuminuria the urine possesses other features that surely add strength to its cyclic character. All natural functions are *cyclic*, they have their time to laugh and their time to weep, a time to rest and a time to act. *Micturition* is *cyclic*, and we are not all set to the same time. But it happens to most people that that function is active at this particular time, and all sorts of strange things may be found in the urine then. It is a time when the urine may lose for a while its proper acidity, when a quantity of phosphates may appear, when urates appear, sugar also. Yes! I think that the mid-morning urine reveals a good deal of one's inner life if only the spirit of the artist be there to recognize and fix, not only the bones of a man but his being.

These cases of albuminuria are often associated with a full, or even high, specific gravity, and a ready deposit of urates on acidifying. Of 63 cases where this point was specially noted, 38 were of high specific gravity; in 13 of these urates were specially noted as being in excess. In nine others phosphates were passed. I am therefore quite in accord with those who think that the fault is largely a digestive one, that defectively prepared products reach the kidney, or even acceptable material in too great an excess for the secreting cells to deal with. I am even disposed to think that this may apply to Dr. Collier's cases after severe exercise, already alluded to. I gather that Dr. Collier has come to think, as have I also, that the condition he describes is an unimportant one as regards the duration of life. I cannot think that, if it were due, as has, I believe, been suggested, to stress upon the small vessels during the exertion, a mechanical exudation such as that could be otherwise than harmful: it ought, one would suppose, to be accompanied in no small number of cases by the appearance of blood in the urine also. A sudden flux of digestive products in which the sentinels at the door are crowded out for the time is, to my mind, a much more likely course of events, and one that in the ordinary course of their workaday life the renal cells must frequently experience, be prepared for, and take no hurt from.

And may not posture tell in a similar direction? A good number of these subjects are pale, flabby, overgrown, and highly nervous subjects.

with splashing stomachs, and perhaps slow digestions. It has been contended, and as I think most reasonably, that the albumin appears in the urine owing to some want of the proper vasomotor control. A condition, in fact, that in older subjects tends to bring about that general proptosis of the viscera that is called Glénard's disease, and which, I should suppose, would virtually make a sort of diet question for the kidney by supplying to the secreting cells a pabulum temporarily in excess of their powers of disposal.

Then may I say just a word or two upon albuminuria as *intermitting*, because it seems to me to have been sometimes quoted as almost in favour of the existence of disease in these cases? It has been quite properly said that it is no proof of a sound organ, and it is well known to occur in some degree in pronounced disease of the kidney, both acute and chronic. But I incline to think that it is still more true to say that it is no proof of a structurally diseased one. Is it not true that we are ever ready to allow a large functional element in the maladies, and even the gross diseases of other organs—the brain, the heart, the lungs, and the liver—but that albuminuria has always lived, since the days of Bright, under the protection of nephritis and little else? Even now, when we are opening out our minds in undreamt of ways, it has but added to and intensified its sombre character by taking to itself an extra garment that is called arterio-sclerosis. I do not know much about rigidity of arteries and high tension in boys—I hope to hear more to-day—but I have been wont to attribute such cases as have been ascribed to those conditions to another wide group in general medicine, that of intermittent claudication; but that is a subject I must not touch on now.

Everyone knows quite well that even in the kidney there is a large functional element in its diseases if we do but look for it, and I have had to exclude from my cases a considerable group because on the whole the cases lean, especially at their commencement, to the side of a true nephritis, although they had also a markedly intermitting character.

I am not, indeed, sure that I have done right in excluding them, or that I ought not, on the contrary, to have put them before you as a special feature for discussion, for they certainly seem to me to indicate that a markedly intermitting appearance of the albumin, even when seemingly associated with considerable evidence of renal disease, may be too readily associated with the *disease* of the organ, instead of with the *health* that remains to it. I do not think that marked intermittency of the albumin can justly be considered a *characteristic* feature of *nephritis*, either acute or chronic, although it may occasionally occur; therefore, when I find it, it always raises the question in my mind, In what condition is the kidney? And, as far as my memory serves me, such cases have mostly been what might not unreasonably be grouped together as of toxic nature; cases, for instance, of uric acid and albumin replacing each other in storms quite gouty in their violence; others, where, after a few weeks of failing health, the hitherto healthy urine suddenly becomes loaded with albumin, which may, or may not, subside, and so on. Such cases mostly sail under the licence of acute nephritis, and such they may become; but the very suddenness of their onset is much more in accord with some toxic state that has determined some actual, but possibly only temporary, damage to the integrity of the secreting cells.

Any marked oscillation in the output of albumin is then no more than an evidence of disturbance of function, however much disease there may be behind it. I make so much of this point because it brings all the intermitting albuminurias into line. At one end of it there are the *severe* cases, that *being so*, may readily, and no doubt often do, lead on to structural change and destruction of the organ. At the other end are the conditions of surfeit or subtoxic states—of adolescent albuminuria—so far as it is renal. These *come* and *go*, as I would maintain all experience teaches, without any risk to the renal structure; and they *leave the subject after a time almost invariably sound*. And I would, indeed, further maintain that the extreme variations of albuminuria so often seen, from *large quantities* to *none at all*, within such short periods of time render any existing structural fault in the kidney highly improbable.

The albumin appears in nearly all cases of adolescent albuminuria to be a serum albumin. I once had hopes, following Sir Lauder Brunton's suggestion, that something might come out of a differentiation of albumins. Once, many years ago, the analysis of the albuminous urine in a girl, aged about 7, came back to me as a case of peptonuria. Now and then it has been more a mucinuria than albuminuria.

Dr. Penny, of Marlborough, who I hoped might have been able to add his knowledge to the discussion, writes pertinently on this matter: "One 'old boy' whom I saw only a fortnight ago told me that he had just been passed for life assurance; and ten years ago he had a large amount of albumin in the morning specimen, but the microscope showed multitudes of spermatozoa; and I imagine that the albumin did not come from the kidneys." I have always thought it worth consideration that just as in old men one occasionally meets with sudden and even profuse hæmorrhage from the prostate that may be repeated time after time from what one may suppose to be a prostatic hæmorrhoidal state, so in the turgid condition of early sexual development the prostatic secretion is likely to become charged with unnatural exudates, and the urine to be found, in consequence, more or less albuminous, but it is not easy to single out this group. I have thought that a few of my cases were probably due to seminal emissions or to masturbation, 14 in all out of 185 cases, but I agree with Armstrong that the group is not large or important; moreover, if it were, one might suppose that it would sensibly affect the very early morning urine, which, it is clear, is not the case, as all are agreed that the early urine passed on rising is nearly always free. Of other local states that may well claim a space in the production of the albuminuria are the worry or wounding of the passages by the transit of oxalates, uric acid, and phosphates. This may quite possibly be a frequent source, but how frequent, there are, I think, no means of deciding.

There is, however, another condition of young life and adolescence that has, I believe, a very largely determining influence, and that is, rapid or inordinate growth. I have taken the average weight for the age in 124 of my cases—48 were under average, and 76 over it, many of them much so. Or if we take the height: out of 106 cases, 23 only were average or under height, and several of these are noted as having grown rapidly of late; 83 were over the average, and maybe for as much as from 3 in. to 8 in. Food-bolting has also, I suspect, an influence in the promotion of intermittent albuminuria, and it is to these two conditions that I personally look in chief for the instigation of the trouble. It is probably an affection largely of males, as has always been very generally asserted. I have notes of only six cases in females out of 206. Several reasons may be suggested for this. The urine itself is less often examined; there is the monthly periodical loss, that may well have a controlling influence in various ways; then girls hardly grow with such all-round rapidity as do boys, and they are much more moderate feeders, and probably less often food-bolters.

I must now finally turn more directly to the after-histories, and in doing so I may say that my own practice has been almost invariably, on finding albumin, to take general stock of the physical condition so to speak, and then to write to the local medical man, telling him what I have found, and what I suspect to be the nature of the case, and asking that the urine should be examined three or four times on each of two or three consecutive days, that the sediment shall be examined for casts, and that the individual should be watched. But for all practical purposes I hold it to be sufficient for diagnosis that the albumin shall be established to vary considerably at various times from none in the early morning through more about 11 a.m. to less again at 5 p.m. and bedtime. I like to hear that albumin is definitely absent some time during the twenty-four hours. The collected sediment should, however, be examined for casts. The presence of cylindroids are of no importance. Our present-day methods of physical examination are sometimes too perfect for health to stand the glare. I shall even confess to having occasionally felt obliged to ignore a solitary granular cast. I allude to all these points of procedure because I rely upon them to support a point in regard to the after-history of these cases. It has been said, I believe, that happy is the man who has no past! Happy also he whose maladies have no future. And notwithstanding the high authorities who think otherwise, I have long held, and still do so, that the *albuminuria of the adolescent has no sinister effect upon health, or upon the duration of life, and that with due circumspection such cases ought not to be excluded from the advantages of life insurance, or from clerkships in banks and other public and private offices.* But I am ready to admit that the requisite evidence that would absolutely establish the truth of this opinion is very elusive, and possibly it is only out of the mouths of many witnesses that it can become established conclusively, if at all. To quote Dr. Penny again, he writes: "My ignorance of the after-history of these cases is almost complete, they pass out of my sight almost entirely." My own experience is not so very much better, and thus it comes, that after looking over my notes, and writing to as many doctors in charge as I could identify, I have only been able to collect an after-history of sorts from 38 cases out of 202. But I am not prepared to allow that the negative evidence of the other 164 is of no value. On the contrary, I think it should be turned to account as I foreshadowed just now. The great majority of the 164 have been committed to the charge of a competent observer, and I think that if any bad result, or even persistent albuminuria, had been the common result I should have heard of it, in at least some small proportion of cases. On the contrary, I have heard nothing, and I have repeatedly asked practitioners whether they knew of any ill-results from intermittent albuminuria—and it is a condition with which most men are familiar—and I have been able to hear of none. Nor do I remember in the course of all these years embraced by my notes to have come across a single instance of any case I have seen passing on into Bright's disease. I have, of course, seen several where the actual condition was at first in doubt; I have abstracts of several such before me, but not one of them but would at once have suggested further observation, and they would have been passed into the group of doubtful cases.

As part of the after-history of these cases it may be said, as is well known, that the condition may spread over long periods of time. I find seven that owned a history of several years; a year or more is quite common. During this course the albumin will be quite erratic in its appearance; it may frequently come and go, or it may be entirely absent for weeks together, and then come back for a time. There are those who think it is an evidence of ill-health and that it goes by the administration of a good tonic. And I am not prepared to say this may not be so with perhaps a good number of cases, but, on the other hand, I have been repeatedly surprised at the tenacity with which it hangs about even in conditions of fair health; and herein is the great difficulty of getting really reliable information as to the patient being really well. Many of my cases were well in health and free from albuminuria, when last examined, but that is no sufficient guarantee that the tendency to discharge albumin had ceased to exist; this, however, is the sort of after-history that in too many cases one has to be content with. Of the 38 cases, 36 males and 2 females, the last observation or report is separated from the first in most by many years, from thirty down to three and two, 28 having more than a five years' after-history.

As regards the cases, a short abstract is subjoined of each case which conveys the length of time over which it has spread, and the state of health of the patient as far as it can be ascertained at the present time, or as late as possible, and this last report is seldom anything but excellent as regards general health, although, unfortunately, it too frequently still leaves us begging for an actual examination of the urine.

The After-histories of Albuminuria of the Adolescent.

No.	Sex	Age	Remarks
1	Male	20	Twenty years after seeing him his doctor reports him to be in rude health and all albumin gone
2	Male	21	Seen in 1906; for some time after I saw him he occasionally passed albumin, but after that he became quite fit and well, and he remains so—1911
3	Male	11	Seen first in April, 1908; his urine contained one-sixth albumin, his general health being good: his heart was supposed to be at fault. January 24, 1911: He has been well since, and is so now, but his urine still contains a good deal of albumin
4	Male	20	Seen first on October 4, 1907; he is a nervous fellow, and his urine, after a game of tennis, contained a thick cloud of albumin; then the usual history—much after exercise, none after lying down: urine, sp. gr. 1020. The laboratory report runs: "The analytical data, microscopical and chemical, prove that there is a definite lesion of the kidney which has permanently damaged the renal tissue"; a second examination gave an equally dogmatic report. The renal symptoms all subsided into insignificance. April 25, 1911: Quite well a long time
5	Male	$15\frac{6}{12}$	Seen in June, 1906; albumin one-sixth, sp. gr. 1030, urates; daily examinations showed sometimes none, sometimes little, sometimes much and urates. June, 1908: Sp. gr. 1018, no albumin; he left school the end of 1909 and is now (April, 1911) studying in Germany and never better in his life
6	Male	$7\frac{9}{12}$	A fine, healthy, rather pale boy, with albuminuria; when seen two to three years after, when under observation for a febrile attack of somewhat long duration, his urine was always perfectly healthy
7	Male	14	Seen with albuminuria some four or five years ago; dark, pale, and an excessive neurotic; he is now in the best possible health, sports' master at a school and plays hockey for his county
8	Male	$16\frac{8}{12}$	In good health, but tall and weedy; rejected at Bank of England for albuminuria; he has never been ill, and now, several years after, he is farming and in the best of health
9	Male	15	Six years after the discovery of albumin; he is still of poor physique, but his urine is free from albumin
10	Male	$14\frac{6}{12}$	Seen first in 1897; six years later he is said to have been well
11	Male	14	Has been under occasional observation since April, 1904; during this time he has passed, at times, albumin from a trace to as much as one-half. April, 1911: He seems quite sound and well; he has grown enormously, but his urine is still albuminous
12	Male	$13\frac{6}{12}$	Under observation since 1898; has had a slight trace of albumin as late as six months ago; was free a month later and also as late as three weeks ago; he has grown into a tall, healthy young man

The After-histories of Albuminuria of the Adolescent—(*continued*).

No.	Sex	Age	Remarks
13	Male	24	Albuminuria, which came and went since the spring of 1896; this still so as late as July, 1899. In June, 1908 (ten years): He has been in India for eight and a half years, and is fairly well; his urine is healthy
14	Male	15	Seen in 1896. In 1908: He is quite well; no disease of any kind
15	Male	19	Seen with intermittent albuminuria in 1898. Five years later: He is quite well, save enervated by the Indian climate
16	Male	17	Seen originally on September 12. 1904. Again in 1911: Height, 6 ft. 2 in.; weight, 13 st. 9 lb.; urine quite healthy
17	Male	11	Seen in 1901 with intermittent albumin. In March, 1911, is reported to be a fine, healthy specimen; he is in India in the army
18	Male	$6\frac{6}{12}$	Seen with adenoids in 1903, but otherwise healthy; he started to grow enormously in the summer of 1906; his urine contained albumin and much urates. He was seen at intervals, for a year and three-quarters, by which time he is noted to be an excellent specimen, and his urine is healthy
19	Male	18	Seen in 1895; has asthma; has grown very fast; has been under observation more or less since then. In 1910: The albumin still occurs in traces now and again, but in all respects, except asthma, he is well; his pulse tension by Riva-Rocci is 116
20	Male	6	Seen in 1896; is said to have had a trace of albumin in the urine persistently, with uric acid crystals, but no casts. He has been seen once since, three and a half years later, when he was quite well and free from albuminuria; I have seen other members of the family more recently, and hear that he is quite well
21	Male	12	An intermitting albuminuria since a sore throat and hæmaturia in December, 1889; he never had any other evidence of renal disease, and the albumin was almost always absent on rising. March, 1911: I hear that he got quite well and remained so until he was drowned some years later
22	Male	17	Seen May, 1889. Fourteen years later he was well, and his urine was free from albumin
23	Male	17	Seen with urine full of albumin; four years later, less; fifteen months later, urine sp. gr. 1028, no albumin or sugar
24	Male	22	Seen December 2, 1901, with intermitting albuminuria; he wanted to get into the Militia; the condition was certified as functional only, and he got into the Militia; four months later he was quite well; he has not been seen since
25	Male	$11\frac{4}{12}$	Seen on October 7, 1901, with intermittent albuminuria of some seven months' standing; there have never been any casts; acid, sp. gr. 1028. April, 1911: He has grown a huge fellow; his urine has been free from albumin or sugar for some years
26	Male	8	Seen March, 1898; urine examined many times; a trace of albumin in most specimens with phosphate. Dr. Earle writes in 1911 that the albuminuria entirely cleared off, I think, by the time he was 15; he is now in the Army in excellent health, quite a fine specimen of a man

THE AFTER-HISTORIES OF ALBUMINURIA OF THE ADOLESCENT (*continued*).

No.	Sex	Age	Remarks
27	Male	17	Seen on August 13, 1898; examined for life insurance, and albuminuria discovered; since then sometimes a trace, then again as much as ever; his urine is said never to have been free; he is sturdy and in good health; urine, sp. gr. 1020, a thin cloud of albumin: his urine was to be examined repeatedly, and the case watched, as I believed it to be an adolescent case. April, 1911: Dr. Rake writes he is now a robust man; there is no albumin in the urine, *and* he has been accepted for life insurance
28	Male	$28\frac{3}{12}$	Seen first September, 1891, having been refused for insurance. It was a typical case of intermitting albuminuria; I saw him at intervals during seven years, as he was always, as he expressed it, "being upset by the insurance fellows again"; the last time I saw him, except that he was nervous and depressed, he was in good health. The urine passed at night after food containing a definite ring of albumin, that in the morning being free; the sp. gr., 1022, of each being good
29	Male	15	Have known him over a period of nineteen years; for eighteen months or more there was marked intermitting albuminuria, as much as one-half when boiled and settled; he has enjoyed fair health all along, and his urine taken lately after breakfast was sp. gr. 1025, acid, no albumin; deposited urates on standing
30	Male	23	In March, 1891, at school, he broke down with headaches, and albumin was found in his urine; the albumin was 1 grm. per litre (Esbach) without renal elements, and was considered to be a case of functional albuminuria; he had been well since, urine being found from time to time often with phosphates; as the albumin has subsided, the phosphates have increased. I examined him in August, 1897; his urine was then free in the morning, contained a trace in the afternoon, and none in the evening; the specific gravity being 1020—1022. Lost sight of.
31	Male	23	In 1903 suffered from intermitting albuminuria which varied in quantity and was regarded as adolescent; was examined for life insurance and albumin found, traces only; sp. gr. 1027; he was passed at a slight increase. He was sound when I saw him soon afterwards
32	Male	7	Seen in 1895 with a urine of sp. gr. 1024 and a trace of albumin. Notes fail of this case, but I learn (March, 1911) that he has passed through school and college life and is apparently in excellent health
33	Male	9	Seen in June, 1896; he looked well, came of nervous stock, and his urine contained a good thick ring of albumin; there was never anything definitely wrong with him, but supposed to be delicate in the chest and he wintered abroad. I saw him in 1907; he was then 6 ft. 1 in., weight 10 st. 2 lb., quite well, and his urine free from albumin
34	Male	12	History wanting. Seen in 1896: a case of adenoids; urine albuminous; four months later this had gone. A year later he remained well, and in 1903 I note that he is now in the Army
35	Male	$11\frac{6}{12}$	Seen in 1896; schoolboy; so easily tired, and headaches; overgrown. Two years and seven years later he is in good health and the urine is free from albumin

THE AFTER-HISTORIES OF ALBUMINURIA OF THE ADOLESCENT—(*continued*).

No.	Sex	Age	Remarks
36	Male	$15\frac{6}{12}$	History wanting; comes for fainting and giddiness; has a little albumin in the urine which I considered to be the albuminuria of adolescence. Eighteen months later I note that he has been well ever since
37	Female	14	Seen April, 1888; has had albuminuria off and on since 1881, but never when she is in bed; up to 1888 she was watched carefully and the urine often examined. She has been known to me ever since, and is alive and well and active (March, 1911)
38	Female	18	Had scarlatina in 1889, with carboluria and albuminuria after; the albumin did not quite clear up, and ever since, more often than not, a faint or sometimes a thick cloud of albumin. On July 24, 1895, her urine, examined by me, was sp. gr. 1030, with much uric acid and a mere trace of albumin; in another specimen there was neither uric acid nor albumin. April, 1911: Dr. Tait, of Highbury, writes that he has examined the urine from time to time, and for many years past has never found any albuminuria, and she has grown up strong and active
39	Male	20	Seen in 1906: albumin sometimes present, sometimes not; refused at a bank in consequence. April, 1911: In Calcutta two and a half years, full of energy and playing all games

The PRESIDENT (Dr. Mitchell Bruce) indicated the conditions under which the debate would be carried on, and intimated that on account of the number of gentlemen who had expressed their wish to speak, and the possibility of the adjournment of the discussion, it was his intention to call upon speakers from the provinces first. Though he did not intend to be strict as to time limit for speeches, he trusted that ten minutes would not be far exceeded, in order that as many as possible might give their views. He also expressed the hope that the actual subject indicated by the title would be adhered to, except incidental references to the disease itself and the causes of its different forms. The intention of the discussion was to compare notes as to the outcome in later life of the albuminuria of adolescence, particularly for prognostic purposes.

Dr. SAUNDBY said it gave him great pleasure to take part in the discussion, as it was a subject in which he had taken an interest for a great many years. As long ago as 1884 a society in Glasgow organized a discussion on the question, and although at the last moment he was prevented from attending, he published a paper in the *Glasgow Medical Journal*[1] for that year, setting forth his views at that date. And although that was twenty-seven years ago, and he had maintained his interest in the subject since, he could only say that the favourable opinion he then expressed as to the ultimate result in those cases was the same as he held to-day. Dr. Goodhart had not attempted to define what he meant by the albuminuria of adolescence, but he took it to mean cases of young people who had albumin in the urine and in whom the most careful investigation failed to reveal any other signs or symptoms suggestive of Bright's disease. That was a definition which involved a great deal. It meant that in each case one had to make a most careful investigation before one was justified in speaking of a case as one of albuminuria of adolescence. Consequently it very much limited the number of cases which he could bring forward in a discussion of this kind. He had seen many patients, or at least persons, whom he thought were instances of albuminuria of adolescence, as he had had to examine candidates for one of the large banks, many of whom had albuminuria, but he had not been able to follow them, and even if he could he had not kept the notes, which were passed on to the bank. But assuming that that definition was accepted, he could not agree with Dr. Goodhart's suggestion that albumin should be absent in all cases at some part of the day; he believed in many cases the albumin did not disappear at any time, or under any influence. Although there was a group of cases in which posture had an influence, there were others in which it had none. So one could not make the definition more stringent than that these were cases of albuminuria occurring in young people of both sexes in whom most careful examination failed to reveal any other indication of Bright's disease, no casts, no cardiovascular changes, nor any retinal degeneration. It was difficult to classify the cases, although, no doubt, the postural type was well defined. This was first described by Dr. Morley Rooke, of Cheltenham, who observed it in a young lady with whom he could make the albumin disappear by getting her to lie on a sofa, and to reappear when she got up and walked about. That answered Dr. Goodhart's question as to whether there were cases which were strictly postural and not influenced by diet. In Goetzky's book[1] the question is discussed at length, and the experiments of Erlanger and Hooker, by inflating a pair of pneumatic trousers, proved that by increasing the external pressure to 50 mm. Hg. the albuminuria can be prevented from appearing, but recurs when the pressure is reduced.

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[1] *Glasgow Med. Journ.*, 1884, xxi, p. 413.

With regard to the cyclical albuminuria of Dr. Pavy, one knew what it meant, but he did not think it was what the name suggested—i.e., something connected with time. The albumin was more frequently found, and in greater quantity, after breakfast, but if the patient were left in bed until after midday, the albumin was increased in the afternoon. Many of the cases which were formerly called cyclical were no doubt postural. There was also a type in which the albumin came on after food; another after violent exercise, which everybody recognized. In the latter it was not a very uncommon thing to see actual hæmorrhage. Dr. Goodhart said it ought to come, and it did. He had seen boys have hæmorrhage after football, and medical officers of schools would no doubt be able to confirm the observation. There were other cases in which there was a plentiful excretion of oxalates, and the cases to which the opener referred where the urine is of high specific gravity, depositing urates, and generally associated with a gouty constitution and over-eating. Boys who came home from school where, perhaps, they were on a moderate regimen, would eat a heavy breakfast, a meat luncheon, and a full dinner; their urine was of high specific gravity, and contained albumin, but if they ate more moderately the albumin disappeared. Then there were cases with a more or less definite history of some infectious disease, possibly scarlatina, yet they ran the course of the albuminuria of adolescence. There was no permanent nephritis, they recovered in time, and the subsequent history was good. He knew many such cases, and could speak with confidence, because he was old enough to have seen them grow up in good health and marry, even though they had post-scarlatinal simple albuminuria lasting for some years. Therefore, in his opinion, the prognosis in all these cases was good, and where one could satisfy oneself that there was nothing except simple albuminuria, one was justified in giving a favourable opinion.

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[1] "Orthotische Albuminurie," Berlin, 1910.

For the purposes of this discussion he had tried to trace his cases, and to give the meeting an account of their condition as it was found to-day. But that was very difficult to do. He was only able to trace twenty-four cases in which he could get a definite report, and they extended over thirty years. None were of shorter time than five years ago. Of those twenty-four only one had died, the cause being acute myelitis, the doctor stating that several years before the onset of that disease the albuminuria had disappeared. Another patient with "cyclical" albuminuria was seen thirty years ago, he was now 45 years of age, and epileptic, but had never developed anything like Bright's disease. Twenty-nine years ago a little girl had post-scarlatinal albuminuria; she was the daughter of a doctor. She was now married, and the mother of a family; there were no casts nor any other sign of Bright's disease, except albuminuria. Another case was that of a doctor's daughter also, who had been watched carefully, and had got quite well. When he saw her first she was a child; she was now a woman in good health. Another was that of a young man seen nine years ago who had temporary albuminuria after working for a competitive examination. He had seen several cases following such events. The patient reported that he was not strong, and six months ago he had to consult a doctor, when albumin was again found, but he had not developed Bright's disease. Of the remaining twenty the average duration was 13·6 years since they came under his observation. Many were well known to him, and about others he had had reports from their doctors, and in no case was there any question that their present health was otherwise good. He did not think the condition under discussion ought to be mistaken for Heubner's Bright's disease of children if the definition he had laid down were accepted. He did not think that in England there was any disposition to minimize the importance of albuminuria; matters were rather the other way, for the tendency was to jump to the conclusion, when albumin was found, that the case was one of Bright's disease. If each case of albuminuria was, as he thought it should be, carefully studied, and only set down as albuminuria of adolescence when all means of investigating it had been exhausted, he did not think we should do wrong in giving a good prognosis.

Dr. H. G. ARMSTRONG expressed his gratification at the opportunity of taking part in the discussion of a subject which, for many years, had been of interest to him as medical officer of a large public school. As the opener of the discussion and the speaker who followed him had departed from the rules which the President had laid down, he assumed he might do so also. His purpose was to try to put before the meeting the types of cases of which he got a good many at Wellington College. He did not think there was any value in statistics on the subject, because of the great intermittency of the appearance of the albumin; one could not say what boys would be albuminuric, unless one daily examined the urine over a considerable time. But, if a urinary census were taken on any one day, of a large public school, like Wellington College, where there were 500 boys, 15 per cent. would be found to be passing albumin. He got that impression from his examination of new boys. He had examined various types of boys who presented that condition, especially with a view to determine the reasons for the varying symptoms which they presented. He need scarcely refer to the literature of the subject, but they were acquainted with the fact that the number of opinions which had been expressed on the matter were many, and some of them were contradictory. Edel, and Erlanger and Hooker, said that the albuminuria was associated with marked changes in the blood-pressure, while Oliver stated that there were none. Some authorities stated it was a phenomenon only of adolescence, and others that it might be continued into later life. Perhaps the contradictory statements were due to the varying causes of the albuminuria of adolescence. He did not agree with Dr. Saundby's definition. What he would suggest was, that it was a complaint which was *entirely* dependent on the assumption of the upright posture. On assuming the horizontal position the albumin entirely disappeared, and would not be produced by either diet or exercise as long as the person maintained the horizontal posture. That was the essential element of the subject under consideration; it was common to all types. If he found a boy in whom the albumin did not disappear entirely upon resuming the horizontal posture, he would conclude that he was not a functional albuminuric, but that he had some organic disease. He proposed to divide the adolescent albuminurics into four classes: the athletic, the emotional, the functional, the lordotic.

With regard to the first, his observations were drawn from boys who took part in the long-distance cross-country runs, a certain proportion of whom passed albumin after the race. But the albumin was always associated with evidence of temporary heart failure, as shown both by the physical examination of the heart and by the character of the pulse and the blood-pressure. He took the blood-pressure of every boy the day before he went for a long-distance run, and of as many as possible an hour and a half afterwards. If there was definite lowering of blood-pressure—it was sometimes 25 mm.—albumin was present; if there was no lowering of the blood-pressure there was no albumin. Albumin continued to appear as long as there was the lowering of blood-pressure. In these cases the blood-pressure often returned to slightly above what it was before the boy took part in the race; there was a slight compensatory hypertrophy—the making of the "athletic heart." The young athlete past adolescence got the same thing when he indulged in violent exercise.

As to the emotional kind, he did not think it was recognized how frequently extreme emotion, especially fear, would cause albumin to appear in the urine. He had seen many examples of that. If the urine of the extreme micturition which attacked a boy, when about to receive a promised flogging, could be examined, he felt sure albumin would be found in it. He could relate marked instances of that.

With regard to the functional—to that class belonged the great majority of true adolescent albuminurics. He held that it was a definite neurosis affecting the cardiovascular mechanism, the exciting cause being the assumption of the erect posture. He agreed with Dr. Saundby that, at whatever time the patient got up, it was *then* that the albumin appeared; it was the getting up which caused the appearance of the albumin. Heredity was a very important factor, one or both parents being of a neurotic type; and it would often be found that the father exhibited similar symptoms during his youth. The circulatory phenomena were cardiac dilatation, a rapid and varying pulse, sometimes soft blowing murmurs at the base. On examining the after-breakfast urine, albumin would be found, often to a considerable amount, but he could assure Dr. Goodhart that the meal had nothing to do with it. One got cases of boys who fainted in chapel, and were taken to the hospital, when albumin was invariably found, even though the boy might not have breakfasted. There was this remarkable fact that all symptoms of vascular excitement and the albumin disappeared on the resumption of the recumbent position. Subjects of that type exhibited other symptoms of vasomotor disturbance, such as cold extremities, chilblains, and cyanosis. They were very liable to epistaxis and other hæmorrhages, and attacks of hæmoglobinuria were by no means uncommon. In those cases the blood-pressure followed the course to which attention had been called by Edel, and Erlanger and Hooker—i.e., instead of rising on the assumption of the erect posture and taking exercise, the systolic pressure fell. He had frequently observed a fall of from 10 mm. to 15 mm., the pressure rising towards the middle of the day and reaching the person's normal in the evening. In these also, as in the athletic and the emotional, there was evidence of back pressure on the splanchnic veins, and a retardation of blood-flow through the kidneys. The condition showed a tendency to intermission; attacks might recur from day to day for days or weeks, and then all symptoms disappear, to recur once more at intervals of weeks, or possibly months; with growth and increasing maturity the intervals between the attacks lengthened, and he believed they disappeared at about the termination of the adolescent period. The pure functional cases absolutely disappeared.

The next was the type which did not so certainly disappear, namely, the lordotic, the recognition of which was due to the work of the German investigators Goetzky and Jehle. The essential characteristic was a lumbar lordosis, causing pressure on the renal veins. The forward curve was undoubtedly an important factor in the production of albuminuria, and he had examined many spines, with the following results: If a line were drawn from the sacral prominence to the most prominent point of the lumbar curve, it would be found to make an angle with the horizontal of about 80° in a well-built youth. If that angle were less than 70° albuminuria would frequently exist, and if the angle approached 60° it was almost always present. The measurements were taken with the boy standing at attention. When standing at ease it increased by 3° to 6°. Goetzky regarded the critical angle as 40°, but there was no indication as to how the angle was measured; 40° seemed an impossible angle, except in a cripple. In those cases there was no intermission; albumin appeared from day to day; there were no symptoms of cardiovascular disturbance, nor alteration in blood-pressure. The functional and lordotic types frequently co-existed in the same individual, and then the variations and symptoms of the former type were met with. The lordotic type had, he considered, a tendency to persist into early manhood, or possibly later. Perhaps to this form was due the frequent faintings of young soldiers on ceremonial parades. It tempted one to speculate as to whether the extremely upright carriage of the body was the correct one.

Those were the types of adolescent albuminuria which he saw at a public school. In all of them there was retardation of the venous flow, produced in the first three by cardiovascular derangement; and in the fourth by mechanical obstruction, increased in the upright position by the weight of the column of blood. Another element in the consideration of these cases, to which he did not think enough attention had been given, and which he regarded as of even greater importance than its pathology, was its psychology. When young people were having functional albuminuria, they also exhibited symptoms of mental and moral decadence; they became careless, eccentric, untidy, forgetful, slack in work; by other boys they were regarded as "mad." It was not uncommon to have a boy sent to him, by one of his educational colleagues, to know whether his falling away from early promise was due to his being an albuminuric. Often it was so. But there was something more than those minor disturbances. From those moral decadents came school-boy criminals—the liars, the pilferers, possibly the incendiaries, and suicides. No doubt the same applied to girls, and he believed the so-called hysterical manifestations of the adolescent girl were really due to her albuminuric condition. Dealing with such cases was one of the most difficult of educational problems. If there was an absence of correction the apathy became worse. But if the boy was constantly nagged at and punished for what he felt he could not help, his despondency was increased, and his neurosis was aggravated. He found that a few words of explanation to the boy either from himself or his tutor, with the assurance that *his* was not an uncommon case, was a palliative, and sometimes a cure. Efforts should be made to help parents and masters to understand that aspect of the case. This inquiry as to the persistence of albuminuria would not be complete without a consideration of the far-reaching effects of that mental condition, effects which, if unappreciated and not dealt with, were likely to lead to permanent mischief.

Dr. WILLIAM COLLIER desired to thank the Council for the opportunity they had given him of listening to Dr. Goodhart's interesting paper. He would set a good example by keeping to the time limit which had been agreed on. During certainly the last twenty years he had had opportunities of seeing many cases of adolescent albuminuria, and he had often been asked by London physicians to watch such cases. He referred to cases in which albumin was absent in the early morning, and present at some time in the day. The conclusion he had come to after some years' observation was, that if the patient was in obviously good health, the best thing was to leave him entirely alone; no good would be done by attempting to treat him. This view differed from the view held a few years ago, which he would instance by one particular case that lived in his memory. A rich and anxious father had an only son, whom he asked Dr. Collier to look after at Oxford. The father had consulted three very eminent physicians about his son—the late Dr. Roberts (Manchester), Sir George Johnson (of King's College Hospital), and Dr. Dickinson. Sir George Johnson took such a serious view of the case that, for three consecutive years, he had sent him to the South of France for six months of the year. The youth came to Oxford at a later age than usual, and Dr. Collier looked after him for four years. In the last week of his residence at Oxford he passed as much albumin in his urine as in his first week. A few years ago he wrote to the man's father, and ascertained that at 30 years of age the albuminuria had entirely disappeared, and had remained absent for at least ten years; the man himself was in good health, and had married.

He would touch on another kind of albuminuria, which he thought would throw some light on the cases of functional or postural albuminuria—viz., athletic albuminuria. For many years he had been accustomed to examine undergraduates between five and seven o'clock—i.e., after their afternoon's exercise. Many of the men had come and asked him if they were fit to row in the races. Many times, when examining the urine of men under these circumstances, he found they were passing large quantities of albumin; with the cold nitric acid test there was a ring such as was found in cases of large white kidney. He told these men that there was obviously some weak point in their kidneys, and that if they went in for these contests they would throw a great strain on the kidneys, and that the leakage of albumin would be increased; so his advice was that they should avoid the violent strain of racing. Many young men regarded rowing as the most important thing in their lives. They were often not satisfied with his advice, and he frequently suggested that an independent opinion should be sought. They would go to London for the purpose, and he had the records of the opinions of many London physicians; and in every case, without a single exception, they had confirmed his view and his advice. Some of the men disregarded this advice, and they seemed no worse for having done so. He thought he would find out what really happened in rowing, and so, with a friend, in 1906, he made arrangements to get the urines of many of the college crews: 156 cases were examined of men who were in training for the Lent races. His friend examined with heat and acetic acid, and he himself examined the same specimen with the cold nitric acid test. Out of these 156 men 130 had albumin in the urine—some in large quantities, some only in small traces. He had the opportunity of examining the urines of the Oxford and Cambridge University crews when practising for the race, and there was albumin in the urine of every man—in some a large quantity. Next he examined the urines of runners, and in all there was plenty of albumin after a race. From his medical friends at public schools he got thirty-eight reports, and in thirty-seven of them there were traces of albumin. In regard to rowing, it occurred to him that the urines were examined only after the men's practice, not after a race. But in the former case the amount of work done was often not very great. So in 1907 he examined and took the urines of five crews who had actually raced. Those he sent to the Pathological Department at Oxford, and the report he got back was to the effect that in the first crew the urine of every member of the crew showed albumin in some degree, with casts and oxalate crystals in every case. In the second crew there was albumin and casts in every case, with spermatozoa in addition in No. 3. In the next crew there was albumin in all. In short, every one of forty men examined was passing albumin, and many of them passed casts, some of which, he believed, were granular casts. He would examine similar specimens this term and find out the exact nature of the casts. He did not understand why so many men were passing oxalate of lime crystals. In athletic albuminuria, what was the cause of the albumin? He agreed with Dr. Armstrong, it indicated an acute passive congestion of the kidneys. If one examined, as he had done, the urine passed by the men two or three hours after rigorous exercise, and then some hours later, it would be found that the albumin disappeared altogether five or six hours after the race. It certainly was not due to muscular work. Eighteen months ago he had the opportunity of examining specimens of urine from eleven men who had just completed a twenty-four hours' walk, none of them having covered less than a 100 miles. One of them was a gentleman aged 66. In only three instances was there albumin, and then only in a small quantity. What was the lesson to be learned from athletic albuminuria? If the line was so narrow between urine which did and that which did not contain albumin, one could understand how, in cases where it was not the result of severe exercise, but merely change in posture, this form of albuminuria probably really did not mean much. That, he considered, was the lesson to be learned from the facts recorded. He did not think there was any sound man who ran or rowed at top speed for two or three minutes who would not pass albumin; and he thought those boys referred to by Dr. Armstrong who did not show albumin could not have run themselves out. In school runs there were many who dropped down to a walk for a good portion of the run.

Dr. CLEMENT DUKES said it was only at a meeting such as the present that one learned the virtue of growing old, for it was forty years since he began to plod over these cases that were now under discussion, and he had continued that interest ever since. During a series of years, whenever he had such cases of a marked degree he used to examine the boys' urine all day long, and went on with the work for a series of terms and a series of years. Many of the boys left his care passing large quantities of albumin. He concluded he was witnessing the earliest stage of organic disease of the kidneys. But in course of time, as experience extended, he recanted his opinion, and he held a very different one now. In 1897 the Governing Body of Rugby School, at his request, made provision for the physical examination of every new boy as he entered the school. In eight years he had examined 1,000 boys, with a clear record of all of them. Of those boys, between the ages of $13\frac{1}{2}$ and $14\frac{1}{2}$, 157 were passing albumin on their entrance on public school life, and during the course at school certainly the number was considerably increased. During his period of office, therefore, about 5,000 new boys entered Rugby with probably 785 suffering from albuminuria. The question he had many times asked himself was, what became of these boys? It was clear that many of them still passed albumin when they left school. If still suffering, they would transfer themselves to the care of physicians in the various large towns when they reached adult age, and there should be strong evidence as to the ultimate issue of such cases. Whilst he was growing old in his work, boys were leaving and going to different parts of the world. Sometimes they returned and visited their old school, and on such occasions he did his best—by invitations to luncheon or to tea—to get them to let him have a specimen of their urine; in fact, he resorted to various manœuvres to gain his end. In not one instance of a former albuminuric who had grown to man's estate did he find the albuminuria still persisted. Therefore he now felt fully convinced that such adolescent cases do not lead to organic disease, but are of a temporary character during growth. In 1905 he read a paper before a meeting of the British Medical Association, in which occurred this paragraph: "I enter a decided protest against these albuminurics having their education interfered with, their games prohibited, their food supply curtailed, with consequent insufficient nutriment for growth, and their life insurance deferred."

Dr. Tyson said his interest in the subject dated from 1874, when he himself had albuminuria. He wished to say a word about something which had not so far been mentioned in the discussion. The remarks had been principally in connexion with children, or at least young adults, but he would refer to cases where albuminuria started in the twentieth year and persisted for twenty, thirty, or forty years. Last Sunday he met a friend who, twenty-five years ago, he examined for an insurance office, and who, fifteen years before, was refused for a London office by, he believed, Dr. Radcliffe, Physician to Westminster Hospital. Twenty-five years ago he (Dr. Tyson) passed him on condition that the policy were loaded, and that if he lived an allotted time he should receive the full amount of his policy. When he saw him last Sunday he was practically well. He could relate several similar cases. The cases he was referring to were not instances of intermittent albuminuria, as he had never found the albumin absent. He had constantly found hyaline casts, and he could not say, not even after hearing Dr. Goodhart's illuminating address, why they were passing albumin. They were not high-tension cases, they were not particularly ill—though they were not robust either —and they did not die. He did not think such cases were recorded in books. The whole history of such cases required re-writing: and even what had been said that day did not give him much help. The cases, as Dr. Clement Dukes said, got well. His opinion was, that if any good was to be done in regard to such cases, a man should begin to observe them when he was 20 years old, and when he reached 80 he should write a book.

Mr. R. Clement Lucas apologized as a surgeon for speaking at a medical meeting, but he was associated with his friend, Dr. Moxon, in life assurance work, when that gentleman described the albuminuria of adolescence.[1] It was so distinct a kind of albuminuria that Dr. Moxon's name might, he thought, after that long period of years, be associated with it. Dr. Moxon described its intermittent character in a volume of the *Guy's Hospital Reports*, and he threw out, very obscurely, his view as to the probable cause in a large number of such cases. He (Mr. Lucas) felt no doubt that in a large number of these cases the albuminuria was associated with sexual excitement—he would not say of necessity with bad habits. But it was found not infrequently, as some speakers had said, going on from young adult life to 24 or 30 years of age, and it was often associated with long matrimonial engagements. He would mention one patient whom Dr. Moxon took for life assurance after he had been for some time under the care of Dr. Hilton Fagge and had been refused by two or three life assurance offices. When he came to the life office he was 24 years of age, and Dr. Hilton Fagge sent a report as to his having been under his care for albuminuria for some time. Dr. Moxon learned from him that he was engaged to be married, and told him he would take him at twelve years extra if he liked to accept it, and he should come again after he was married for further examination, when he believed he would pass an average life examination. That showed what Dr. Moxon's opinion was. The man was taken at twelve years extra in 1881. He came two years afterwards and was taken as an average life, and dropped his former policy. He was now, thirty years afterwards, living and in good health. The cases which one generally saw were in young persons between 15 and 19 years of age—Dr. Moxon discovered the condition by examining young gentlemen candidates for clerkships in a bank, and 10 per cent. of these youths had albuminuria. There are associated symptoms in the majority of the cases which point to sexual excitement; the face is generally pale, and often spotted with acne, the eyes are hollow, the pupils dilated, and the throat congested, often associated with enlarged tonsils, adenoids, and a liability to catch cold. There is also an excitable heart, the very sound of which is sufficient, in many cases, to indicate that the boy has albuminuria. It is a very distinct class. Dr. Armstrong and Dr. Collier had alluded to a class of athletes who had albuminuria. He (Mr. Lucas) would separate them off entirely. The cases to which he was alluding were not athletes, but were very liable to late rickets, and some years ago he ventured to describe that form as the "rickets of adolescence,"[1] to show its connexion with the adolescent albuminuria of Dr. Moxon. Another point to which Dr. Moxon alluded was that in many of these cases the albumin was very slow to deposit. Placed on top of nitric acid, it might be some minutes before it would appear. Dr. Moxon said that was characteristic of this form of albuminuria. He asked whether others had noted that fact. He agreed with those who had spoken that the albuminuria of adolescents was seldom of lasting importance. He had alluded to a case where good health was still preserved thirty years afterwards. Some of the cases ended in a way which suggested that there might be weakness of constitution, and perhaps also some mental defect. One of the cases which was seen in 1884 died between 50 and 60 years of age on suspicion of having committed suicide. Another case died of general paralysis of the insane, and another succumbed to phthisis thirty years afterwards. Not one, however, to his knowledge, had died of chronic albuminuria. The athletic form should be taken out from that category, for the cause and effect were obvious. The form of albuminuria of adolescence which was most characteristic was that associated with the symptoms he had mentioned, and was exceedingly liable to occur in connexion with late rickets.

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[1] *Guy's Hosp. Reports*, 1878, 3rd ser., xxiii. p. 233.

(The discussion was adjourned until May 2.)

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[1] *Lancet*, 1883, i, p. 993.

Medical Section.

May 2, 1911.

Dr. J. Mitchell Bruce, President of the Section, in the Chair.

Discussion on the After-history of Cases of Albuminuria occurring in Adolescence.[1]

Dr. Samuel West: The remarks made at the last meeting show the necessity of exactly defining the group of cases to be dealt with. Albuminuria is a symptom and not a disease. The causes of albuminuria may be classified, as I suggested some years ago, into pre-renal, renal, and post-renal. In the post-renal group the albumin is added to the urine after it has left the kidney, and may be termed accidental, as being due to accidental contamination. It is in consequence of the greater ease of excluding such accidental contamination that males are usually selected for statistical purposes rather than females, but I believe that if in females the urine was as systematically examined, functional albuminuria would be quite as common, if not even more frequent. I may, in passing, say that masturbation and sexual excitement may practically be disregarded as causes. In the pre-renal group in some cases the cause is obvious, as in the venous congestion of morbus cordis, or of pressure upon the renal veins. So in the renal group the causes of the albuminuria may be obvious or not.

Functional albuminuria may be described as albuminuria of which the cause is not obvious. It is, however, never physiological, but always pathological, though not necessarily renal. The diagnosis is not difficult, but necessitates care and prolonged observation, so as to exclude as many causes as possible, which though not obvious may still exist.

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[1] Adjourned from April 25.

Albuminuria of adolescents limits still further the class of case to be dealt with.

The general curve of frequency of albuminuria is at its lowest at the age of 25, being considerably higher in the few years immediately preceding, and gradually rising afterwards as age advances. This being so, it follows that much of the albuminuria of adolescents must disappear before the age of 25. The statistics of insurance offices deal chiefly with the period after 25, but they show distinctly that among the cases accepted with albuminuria (but of course without evidence of renal disease, for then they would not have been accepted at all) the mortality rate is higher than among those without albuminuria. The significance, therefore, of albuminuria without obvious cause is greater after 25, and increases in gravity with every year after that age, depending largely, no doubt, upon the increasing frequency of granular kidney. The most valuable statistics would be obtained from banks and business houses, who require medical examination of their young clerks before appointment, or of the insurance offices who insure them. I see a considerable number of young clerks between the ages of 16—20 in this way, but my figures do not show any great frequency among them—a little less than 2 per cent. Yet the frequency of functional albuminuria is put as high as 20—30, or even 50 by some observers. This is due to two facts: first, the inclusion of cases in which albumin follows muscular exertion; and secondly, to the methods of detection employed. In a very large number the trace is infinitesimal and would be overlooked on ordinary examination. Dr. Collier has shown that in athletes after violent muscular effort albumin is constantly present in the urine, and often in considerable amounts, but here the cause is obvious, and these cases should be excluded. If that were done the frequency-statistics given in books would be very greatly reduced, certainly to below 10 per cent. and probably less.

The class of case that usually presents itself in practice is that in which a youth at school, or applying for an appointment in an office, has been found to have albuminuria, which there was no reason to suspect. The questions are at once raised: What can it be due to? Must he be kept from school and made an invalid of, or must he be condemned as unfit for appointment? The general answer to these questions I may give at once. If it be a clear case of functional albuminuria, none of these extremes need be resorted to. Most of the cases lose their albuminuria as they grow up, and if in some few cases they do not, no evil results seem to follow. But everything depends upon whether the diagnosis of functional albuminuria be correct, and that is just the difficulty, the diagnosis can only be made after prolonged observation and careful study of each case individually.

In functional albuminuria it may be stated as a general rule that the albumin is not always present, varies greatly in amount, often being very small—varies, too, in relation to food, posture, &c. Unfortunately all these peculiarities are observed in convalescence from acute nephritis, before the kidneys have quite recovered, so that these points are in themselves no proof that the kidneys are sound. And again, where the kidneys are known to be diseased, as in granular kidney, the albumin may vary much at different times or even be altogether absent for long periods together. Thickened arteries where not obviously due to athleticism would raise serious questions in my mind, for granular kidney begins much earlier in life than is often assumed, and well-marked—i.e., advanced—cases are not by any means very rare in the young.

The diagnosis then resolves itself into the question, Is there or is there not any evidence of renal disease, or is there any other obvious cause with which the albuminuria may be connected? Where it stands in relation only to muscular exertion, the albuminuria is almost physiological and constant, and its significance *nil.* Upon what evidence may we rely to infer renal disease? Where there is a clear history of acute nephritis in the past, it is probable that the kidneys have never quite recovered. It must be borne in mind also that recovery may be complete in nearly the whole of both kidneys, and yet, as autopsies show, there may be smaller or larger patches in which degenerative lesions are present, sufficient to account for the albuminuria, yet compatible with perfect health. Thickened arteries are not conclusive, for just as in athletes the heart hypertrophies so do the arteries, and physiological or functional hypertrophy of the arteries must be borne in mind when considering the possibility of granular kidney. Yet, as a rule, these cases are easily excluded.

Renal Derivatives—Cast and Renal Cells.—We do not respect casts as much as we were wont to do. Cylindroids are of no significance. Hyaline casts are common enough in otherwise healthy persons, and so even may be an occasional granular cast. Blood-cells raise more important questions, for though one speaker stated that hæmaturia might occur in functional albuminuria, such a statement should be received with caution. It need not be due to renal disease, but oxalates, and uric-acid-sand or -crystals are not infrequent causes both of albumin and of blood in the urine.

We may conclude that though functional albuminuria or the albuminuria of adolescence is not a grave condition and requires neither rigid treatment nor invalidism, still the diagnosis is the difficulty and this requires great care and observation, together with some skill and experience.

Dr. NESTOR TIRARD said there was much in what Dr. West said with which he was in cordial agreement, and that he was in more cordial agreement with that gentleman's remarks, on the whole, than with what was said by Dr. Goodhart in his opening address. At the last meeting speakers seemed to have been referring, under one heading, to many different things; under the heading of the albuminuria of adolescence they had been appealing to different types of albuminuria as illustrations of the albuminuria of adolescence. Naturally enough, the speakers at the last meeting drew largely from the special circumstances of their own practice. Dr. Collier had given an admirable account of a well-recognized form of albuminuria, that due to overstraining, which he called passive congestion of the kidney. That form had long been recognized, but he took it that in speaking of that form they were dealing with something quite different from the type of albuminuria which came under the observation of those engaged in examining for insurance offices, where one's concern was with people of a more advanced age than those who were at the Universities; and that the types seen at insurance offices were totally distinct in broad lines from those seen in the ordinary consulting room. In the past few years he had had all those types under his observation, the insurance work showing one type, the young people connected with colleges showing another, and young men applying for clerkships showing yet a third. Those types were, he thought, to be grouped as functional, but certainly not as adolescent. Probably all three types were of no ultimate importance in the matter of prognosis. Prime importance should be attached to the diagnosis of the case, because if any single feature were taken alone, error might result; one could not lay stress on the small amount of albumin, or on the specific gravity of the urine, or on the age of the patient; every case should be taken in detail, every factor being duly considered before it was concluded that the case was a functional one.

With regard to age, only that afternoon he saw at the Hospital a young woman, aged 22, who was passing a small amount of albumin. There was no history of scarlet fever, nor of any condition to suggest that the albuminuria was serious, yet she was admitted primarily for retinal hæmorrhages, and for high tension pulse. This was the third case he had had under his care during the past winter, of young women being admitted with retinal hæmorrhages, high tension pulse, and indications of chronic interstitial nephritis. One was aged 22, one 26, and the third 28. The occurrence of three such cases in that time was a unique event in his experience. In one of them there was an opportunity of confirming the diagnosis post mortem, that of the woman aged 28. If importance had been attached solely to the age, and the small and varying amount of albumin, one might have gone wrong. It was the occurrence of such cases that made him very cautious in accepting a diagnosis of functional albuminuria. Such cases were met with in insurance work, and he was sorry to say his experience of them differed from that of Dr. West, for he would have said that Dr. Moxon's albuminuria in adolescence, in the sense in which he used that term, was a correct term. He had seen a large number of cases of people wishing to insure their lives for the purposes of a marriage settlement, in whom albumin was found in the urine: and in those he followed what he believed to be the safe rule, of saying that they could have their insurance, but at a price, that price being subject to revision at a later period. In nearly all those cases, on examining two or three specimens a year or so after marriage, albumin was found to be absent. That fact led him some years ago to attach a new importance to Dr. Moxon's paper; and he had had a very interesting confirmation of the accuracy of that in a friend of his, who was declined by the medical officer of an insurance office because he had a displaced apex beat, a high tension pulse, and albumin in his urine. The albumin varied in quantity. He told the friend that, in spite of his having been declined, if he got married he would probably lose the albumin. From time to time, for three or four years, he came to see him (Dr. Tirard); he examined his water, and on one occasion found albumin was absent. He followed the case with interest, and although the man was very reticent, it was discovered that in the meantime he had made a runaway match, and after he married, the albumin disappeared. This absolutely fulfilled the prognostications which he (the speaker) had made. One needed to be cautious in saying that the presence of albuminuria was of great importance. In many cases in young men it meant very little, yet in some it was of importance, and led to early claims. It was necessary by pulse tracings, by the position of the apex beat, and by constant examinations, to ascertain the actual cause of the albuminuria. He did not think much was to be gained by grouping them all as adolescents, when they were not all adolescents, for the albuminuria of that kind might occur at almost any age.

He did think that out of a discussion of that kind the difficulties of insurance officers might be somewhat increased, that the number of supposed cases of functional albuminuria coming before them might increase. One was familiar with the candidate who, on being told there was albumin in his urine, said he had had eggs for breakfast that morning; or if there was sugar in his urine, said he had had marmalade for breakfast. These were the evasions by which people who knew themselves to be weaklings attempted to secure a future for their widows and families.

Dr. F. Parkes Weber said he would practically confine his remarks to the so-called orthostatic albuminuria, which he believed to be by far the most frequent, if not the most important kind of "albuminuria of adolescence." His knowledge of such cases was derived from young men whom he had examined as medical officer to a large insurance company, from candidates for clerkships in a London bank, and from a few patients observed in hospital or private practice. Several young men with orthostatic albuminuria had been accepted for life assurance or for clerkships, and he had never heard of any bad result following acceptance in any of the cases he had examined, nor had he heard from other doctors, or from a study of the extensive literature of the subject, of any single bad result in a case of typical orthostatic albuminuria. Considering the enormous number of cases, including those referred to in the literature of the subject, that was very significant. A case, which he had frequently examined and knew better than the others, was that of a man aged 28, who was fond of hunting and open-air exercise of a very active kind. Typical orthostatic albuminuria had been observed in him at least since 1907, but had been probably present much earlier than that. Sometimes his urine had contained a few hyaline tube casts, and sometimes there were granules or cells in the hyaline casts. This young man felt well and might even gain in weight when he was able to live the active, open-air life which he loved. He was of the usual tall and rather thin type, and, as usual in such cases, he had grown rapidly in height during early adolescence. On the last occasion that his urine was examined there was still orthostatic albuminuria present. The prognosis in orthostatic albuminuria was so good that no candidate for an appointment or for life assurance should be rejected on account of its presence, that is to say, in the absence of other points against him.

With regard to what he meant by orthostatic albuminuria, he would now say a few words. The main diagnostic character was the occasional, but not invariable, presence of ordinary urinary albumin—i.e., serum-albumin—(sometimes little, sometimes much) in the urine passed when the patient was up, but not in that passed in the recumbent posture or immediately after the patient got out of bed. He (Dr. Weber) laid great stress on that, because the doctor often asked for the "morning urine," and consequently might be given the urine passed at ten or eleven o'clock (precisely that most of all likely to be albuminous), instead of the urine passed immediately on getting out of bed. It had been experimentally shown (in some cases, at all events) that posture, not the time of day or the time of meals, was the essential exciting cause in the production of orthostatic albuminuria. Often calcium oxalate crystals were present in the albuminous urine, and occasionally a few red blood cells might be found. The presence of these red cells might turn out to be an occasional genuine feature of orthostatic albuminuria, but might, on the other hand, be merely connected with the concomitant oxaluria. The urine of patients with orthostatic albuminuria might likewise contain a little of the protein (formerly thought to be nucleo-albumin) precipitated by acetic acid in cold.[1] Usually tube casts were absent, but, by the help of the centrifuge, a few hyaline casts and even one or two granular or cellular casts[2] might be detected. The administration (as a "test") of calcium lactate (suggested by Sir A. E. Wright) seldom sufficed to remove the albuminuria, even temporarily, in cases of orthostatic albuminuria. This type of albuminuria of course occurred in girls as well as boys, but from certain hospital observations he would suggest that it occurred at an earlier age in girls. In hospital in-patients the presence of orthostatic albuminuria was frequently overlooked, because the specimens of urine examined were those passed whilst the patient was confined to bed. He did not think that albuminuria occurring in apparently healthy adolescents could justly be regarded as belonging to any other class than orthostatic albuminuria, unless it could be definitely ascertained that the urine passed immediately on getting out of bed after the night's rest contained albumin.

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[1] A case in point now under Dr. Weber's observation is that of a young man aged 18, a specimen of whose urine, passed immediately on getting out of bed in the morning, is free from albumin of any kind. But the urine passed at ten o'clock in the morning contains both ordinary albumin and a protein precipitated by the addition of very dilute acetic acid in the cold.

[2] Dr. Weber would rather describe these casts as "hyaline casts containing cells or granules," and he thinks such casts should be distinguished from typical "cellular" and "granular" casts.

For practical purposes he thought the so-called "lordotic albuminuria" should be regarded as a variety of orthostatic albuminuria, in which the lordotic position was the essential exciting cause of the albuminuria, as it could be experimentally proved to be in some children. Much had recently been written on that subject in Germany and on the Continent. In cases of lordotic albuminuria even when the patient was kept lying in bed the urine could sometimes be made to temporarily contain albumin by placing a bolster under the small of the patient's back, so as to produce an artificial lordosis. A somewhat asthmatic girl, aged 12, was under his observation with orthostatic albuminuria. Her urine was free from albumin when she was kept lying in bed, except when a bolster was placed under the small of her back so as to produce an artificial lordosis. This was tried for about an hour on two occasions, and as a result on each occasion albumin appeared in the urine. He did not think the temporary albuminuria caused by running or rowing should be regarded as specially connected with adolescence, and he did not think that it should be discussed in the present connexion. Probably albuminuria of the orthostatic type discovered in the urine after scarlet fever had been sometimes incorrectly taken as evidence of the presence of scarlatinal nephritis. There was, however, no reason to suppose that orthostatic albuminuria might not sometimes accompany albuminuria due to actual nephritis. Naturally very few kidneys of patients known to have had orthostatic albuminuria had been microscopically examined. In one such case, however, slight kidney "changes" were found, but in the light of recent researches it appeared that slight so-called "changes" in the renal cortex might be found by microscopical examination in children who had never had kidney disease. At any rate, orthostatic albuminuria could not reasonably be attributed to the presence of such slight microscopical "changes."

In conclusion, he wished to draw attention to the analogy between renal symptoms and cardiac symptoms, from the point of view of prognosis. Just as grave myocardial changes might exist without obvious clinical symptoms, so also organic renal disease might be present in people who seemed to be in good health, and whose urine was sometimes quite free from albumin. Just as young persons may present cardiac murmurs, palpitation, &c., in the absence of organic heart disease, so also they may pass albumin in their urine in the absence of organic renal disease. And just as an organically diseased heart might regain its functional activity and do all the work that it was called on to do, so in cases of parenchymatous nephritis with dropsy of long duration one might sometimes observe a kind of "urinary crisis," during which, in the course of a few days, the dropsy entirely disappeared, owing to the kidneys having recovered their functional activity, although remaining, of course, anatomically diseased.

Dr. GOSSAGE said he had had much experience in examining males in the adolescent period, and had found a good deal of that class of albuminuria. His experience of the proportional occurrence of albuminuria had been somewhat larger than that of Dr. West. Out of the last thirty cases albumin was found in five. The reason was that a considerable number of his cases were young men who had travelled up some distance to be examined, and some of them had travelled all night, just before examination. Amongst these the proportion which showed albumin in the urine was very high. He did not know why a railway journey should cause the excretion of albumin, though of course many of them would travel in only moderate comfort. In a considerable number of the cases the albumin disappeared almost directly afterwards, at whatever time of the day the urine examined was passed, the patient still pursuing his occupation. In a considerable number also the albumin apparently disappeared permanently. He could not think marriage had any influence in causing the disappearance of albumin; anyway, it frequently disappeared permanently in bachelors. A certain number of his cases bore out the truth of what Dr. Parkes Weber had said as to the conjunction with albuminuria of an excessive amount of nucleo-albumin. In two cases which he was able to follow out the amount was so large that the urine became almost solid on the addition of acetic acid in the cold. One of these patients he still had under observation. In both the nucleo-albumin and albumin absolutely disappeared: in one the next day, in the other after a few weeks. One of the main points in the discussion was to ascertain what became of the cases eventually, and that was just what was so seldom possible; as to what was the condition at 40 of a person who had albuminuria at 20. The fact that the albumin disappeared was known already, and it was the essential definition of the class that, from 20 to 30 years of age, those people should be comparatively healthy. One wanted to know what proportion of the cases were affected with renal disease in middle life, but there were not sufficient statistics available to determine the point. He, personally, had only had the opportunity of following out any of these cases for about seven years. Two or three young men he had known for from two to seven years, and they had kept in perfect health during that time, and the albumin had disappeared. A certain proportion of the people were pale and flabby, and some exhibited dilatation of the heart and functional murmurs with rapid heart action—that other bugbear in the examination of young males. He had not noted any special association with a particular build; tall and rapidly growing youths did not seem to have the condition more than thick-set, short ones.

The President (Dr. Mitchell Bruce) desired, before offering a few remarks on the subject of the debate, to convey to Dr. Goodhart the thanks of the Section, and particularly of the meeting, for the immense amount of trouble that he had taken in preparing the address with which he had opened the discussion. That was not Dr. Goodhart's own proposal, but that of the Council of the Section; and it was extremely good of him, in the midst of all his work, to have gone through his case-books and abstracted hundreds of cases in order to arrive at the results which he embodied in his paper. The Council were also pleased to find that some of the provincial Fellows had come up to take part in the discussion. He had always said that the Society was particularly anxious to welcome them.

With regard to the subject under discussion, he would offer a few remarks based on his own experience, and try to answer the question which Dr. Gossage had put, namely, What would be the outcome if they were followed up for a sufficient number of years? He had gone through a certain number of his case-books, and he had found thirteen cases of what he might call typical albuminuria of adolescence—though he agreed that there was a divergence of opinion about the definition of it—and he had written to all of them, and heard from ten. Two he could not trace; one had died. The ten were quite well, after an interval ranging in length from eleven to thirty-one years. Not one of them had albuminuria now. The one who died, died of chronic Bright's disease; and he would say a word about that case, because the diagnosis at first was based on an error on his part, which he corrected when seeing the man a second time. At that time he considered that masturbation and spermatorrhœa had something to do with these cases. That man gave him a full confession of very bad masturbation, and no doubt that misled him. But even then there was no doubt the case was one of early Bright's disease. The albumin never disappeared: four or five years afterwards the patient was obviously the subject of chronic Bright's disease, and he died a few years ago. So that of the unquestionable cases of that series all remained free from albuminuria. Therefore his conclusion was that the albuminuria of adolescence was an innocent disturbance of health in respect of its ultimate results. He was particularly interested to hear Dr. West dwelling on the difficulties of diagnosis, one of the most important points to be considered. He would refer to two points in addition to those brought forward by Dr. West. One sometimes came across tuberculosis of the kidney, which for a time had been regarded either as the albuminuria of adolescence or as early Bright's disease. That mistake would probable be seldom made now, because there was a bacteriological examination of all doubtful cases. And sometimes one might mistake *Bacillus coli* infection for the albuminuria of adolescence. There were cases of the kind which one met with after they had been going on for perhaps twelve months or more without their nature having been discovered. Another point of difficulty was in connexion with the examination of the heart and pulse as a test of the presence of false or true Bright's disease. It was known that youths had relatively larger hearts than adults; and the same difficulties which had been mentioned with regard to murmurs applied to the size of the heart. Therefore, that was not a test which one could always trust to. Again, he had noticed that the second sound of the heart was often relatively loud in those subjects, so that for a moment one feared they might have Bright's disease, but closer observation showed that the second sound only seemed to be loud because the first sound was relatively weak. There were two other points which, though they might not be important, he had noticed in his study of these cases. The first was that in a certain number of them there was left-sided varicocele, leading one to wonder whether this was another evidence of obstruction of the left renal vein. That carried one back to the cases in which the venous stasis in the kidney was believed to cause the albuminuria. With regard to orthostatic albuminuria, he had noticed that the headache of which these patients often complained was especially severe when they stood up and disappeared when they lay down. That appeared to conform with the orthostatic explanation of those cases.

He felt altogether satisfied with the discussion; it was one of the most interesting and successful which the Section had held for some time. And, in spite of what Dr. Tirard had said, he thought they might conclude that the kind of cases under discussion did not ultimately end in Bright's disease, and that they might safely be accepted for assurance.

Dr. GOODHART, in reply, said: In opening the discussion I hoped to elicit an opinion that the albuminuria of adolescents is a condition involving no prospect of a shortened life, or much stigma even upon health, and that there is little reason for subjecting those affected by it to any disability as regards the choice of a career, and I think that opinion has upon the whole been given. Why this form of albuminuria should prove so harmless several interesting suggestions have been offered, and all of them are well worth consideration, but I am surprised that posture should have assumed the prominence that it has done. In opening I said I ventured to doubt if posture had yet been proved to be an essential cause, although posture plus food seemed to be sufficient. I did not by that mean to convey that there was not plenty of evidence to show that posture was an important initiating influence, but I did mean to demur to any hypothesis of simple mechanical stasis, such as seemed to be implied by it. I certainly am not prepared to think that the mere tipping of the adolescent machine from its back on to its toes is in itself an adequate explanation of albuminuria. One must be careful not to assume too readily that the kernel of such a subject has been reached. I rather think there is a suspicion of this here. And I was rather confirmed in this opinion by listening to the very interesting remarks of Dr. Armstrong. Posture, and birchings, and empty stomachs, and low blood-pressures were all alluded to by him as causes, and all his remarks were exceedingly pertinent to the subject, but how explain them all?

Let me take one group of Dr. Armstrong's cases where he described the albuminuria with low pressure; how that for the time this kind of patient went off colour, with blue hands, slow circulation, a tendency to faint. He did his work badly, and even might become temporarily a moral reprobate. If I understood him correctly, Dr. Armstrong would associate the albuminuria and all the evils he so well described with low blood-pressure. I agree with the clinical description, I have seen many such cases, and, in the past, they have been too little recognized by masters at schools. The report comes back from school: "Has strangely gone off in his work; is lazy; could do better if he liked." These cases nearly always occur in rapidly growing and developing boys. And I have known some to become so addle-headed, absent-minded and sleepy for a time, as to fall asleep in the street, wander away and become lost; and sometimes to raise the question whether they were not epileptic. And I wonder that not more has been made in the discussion of the overgrowth of these adolescents. My own explanation, as I said in my opening, is: not that they are albuminurics; not that they are cases of low blood-pressure only, but that they are machines with their petrol exhausted, their nervous energy all gone for a time; and these unnatural signs are an indication of the upset of function caused thereby.

Adolescent albuminuria is largely an affair of growth and development. Not entirely so, for I am sure, with Dr. Armstrong, that an emotional temperament is often behind it. These things I take it act upon and lessen vasomotor control and link up the condition with such phenomena as flushing and blushing, a poor circulation as it is called, and blue hands and blue brains, and with—not renal disease—but that large subject of angio-neuroses and intermittent claudication suggested in my opening. And it is from this point of view that such cases as those mentioned by Dr. Tyson may claim to be considered as well as others, of which I have notes, of intermitting nephritis and dropsy. I think it may well be that there is low blood-pressure in these cases, but here again I am caught in the doubt whether we have as yet made much way with the clinical investigation of blood-pressure. Is the index of the pressure at the wrist, or at any *one* part, a correct indication of the blood-pressure in other parts of the body? I remember long ago seeing a manometer fixed to a man's wrist, and a record of his blood-pressure taken on a drum, and while this was being done he was put to work out some small calculation in his head, and immediately on this being commenced his pressure at the wrist fell. Then and now I associate that experiment with the remark of the "Autocrat of the Breakfast Table," that "I like those people whose feet grow cold when they are writing, whose blood all runs into their head as mercury into the ball of a thermometer." I hear talk of reducing blood-pressure as if it were the easiest thing possible. All I know is, that I do not get the results that others claim to do, and *permanently* to reduce the blood-pressure of any particular man is one of the most difficult things to accomplish. This correlation between the various organs of the body must make it very difficult to be sure of. Tension, like this form of albuminuria, is intermitting. It is up and down, now here, now there, and thus I doubt if it can be considered as an essential cause. There is still something behind it, and what that is, in my opinion, I have already indicated.

Medical Section.

May 23, 1911.

Dr. J. Mitchell Bruce, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

In accordance with an honoured custom of recording to-day the state of the Society, and in particular of the Medical Section, may I not say that "The number of its members has increased, its meetings have been better attended, and, above all, a more abundant supply of communications has been received. The library also has been gradually improving; a house has been obtained in a more central and suitable situation; and, in short, the Society has assumed a position which augurs most favourably for its permanent and increasing utility." I have spoken these words of congratulation and cheerful anticipation, but they are not words of mine. They are an extract from the Advertisement, as it is quaintly called, of Volume the Second of the *Medico-Chirurgical Transactions*, published by the Medical and Chirurgical Society of London in 1811.

In casting about for a subject of my address, and knowing that I could not instruct you, I lighted on this volume, and found in it a good deal of matter which I thought might at least entertain you for a few minutes.

Instead of indulging in the routine account of "the advances of medicine during recent years," let me bring into vivid contrast the subjects of the communications just referred to, as received in 1809-11, with the subjects of those which have been read and discussed in our Section during the years of my office. For, very curiously, the two terms exactly correspond: "Volume the Second," as it is called, contains the papers received in the course of two years, from 1809 to 1811 inclusive. To me at least there is a certain fascination in the republication of the events of a hundred years ago. The *Times* daily keeps us informed of them as far as the country was concerned. It was a memorable, a critical period in our history. The Peninsular War had opened, and this month we were rejoicing over the retreat of Massena from the lines of Torres Vedras, the evacuation of Portugal by the French, and our victory in the bloody battle of Albuera. It is interesting to note how little disturbed by the life-and-death struggle proceeding on the Continent of Europe, and events of smaller but still great importance to the country in India, America and, indeed even then all the world over, was the scientific and professional work of the members (as they were then called) of the Society. Only here and there in this volume do we find echoes of war and its consequences. William Fergusson, Esq., Inspector-General of Hospitals to the Army in Portugal, acceded to a request to allow the Society to publish a private letter which he had written on "The Mercurial Plan of Treatment in Dysentery," as well as in "Yellow Fever and the Remitting Fevers of Europe and the East and West Indies." He had himself recovered from a violent attack of dysentery when serving under Sir John Moore by taking grain doses of calomel, and had treated it and fever in Walcheren and the West Indies. In dysentery ½ gr. of calomel with 1 gr. of ipecacuanha was given every hour, and continued till the gums were affected, generally in forty-eight hours; and he supports his recommendation of the mercurial treatment by adding a note from a professional comrade in the Army, who inuncted a drachm of strong mercurial ointment twice or four times a day until ptyalism reached fully a pint. Opium he considered dangerous. In the remittent fevers of the West and East Indies mercury was given in the second stage, apparently for its purgative effects. In the early stage, before "determination" to the vital organs—more particularly the liver and spleen—the most powerful means of treatment he considered to be venesection, which ought to be generally and decidedly practised in the young sanguineous soldier on the first invasion. Is it possible that venesection *did* rid the blood of the malarial organism?

Meanwhile, in spite of the moving events of the world of international politics, men at home were working steadily, not only at practical medicine and surgery, but at anatomical and chemical pathology. Here is a list of the Council of the Society elected in March, 1811, most of the names memorable in the annals of our profession: Sir Henry Halford, Bt., M.D., F.R.S., President; John Abernethy, Esq., F.R.S.; John Addington, Esq.; C. R. Aikin, Esq.; Wm. Babington, M.D., F.R.S.; Matthew Baillie, M.D., F.R.S.; Thos. Bateman, M.D., F.L.S.; George Birkbeck, M.D.; Robert Bree, M.D., F.R.S., Vice-President; Thos. Chevalier, Esq., F.L.S., Vice-President; Astley Cooper, Esq., F.R.S., Joint-Treasurer; Everard Home, Esq., F.R.S., Vice-President; Wm. Lambe, M.D.; Alex. Marcet, M.D., F.R.S., Secretary for Correspondence; Thos. Nelson, M.D.; John Pearson, Esq., F.R.S.; Peter M. Roget, M.D., Secretary; Wm. Saunders, M.D., F.R.S.; John Sims, M.D., F.L.S., Vice-President; Benjamin Travers, Esq.; Stephen Winthrop, M.D.; John Yelloly, M.D., Joint-Treasurer. When we are reminded that these were the men that were filling the foremost places in the hospitals and medical schools, and promoting the interests of the Medical and Chirurgical Society a hundred years ago, we understand how beginnings were made in the advances of the nineteenth century.

Let me take some of the papers published in this volume, and set them in contrast with those which have been read in our Section during the last two sessions. I do not set up this contrast either to disparage the past or to magnify unduly the present state of medicine and surgery. I desire, rather, to trace in the work of a hundred years back the beginnings of knowledge now fully established.

It is in respect of clinical methods that the contrast is most striking. During the two years of my Presidency we have had a discussion on the "Physical Signs of Pericardial Effusion"; an elaborate paper by Dr. William Ewart on "Dorsal Percussion of the Thorax and of the Stomach"; and a paper by Dr. Essex Wynter on the "Absence of Abdominal Respiratory Movements as an Indication of Pericarditis."

Now on January 1, 1811, Dr. Robert Bree, F.R.S., Vice-President of the Society, presented a communication on "Painful Affections of the Side from Tumid Spleen." After some introductory remarks on "Diseases of the Spleen," the writer records a case of pain in the left side in a young lady, aged 17, from which she had suffered for two months "after prolonged bathing in the sea." The pain was severe and aggravated by right cubitus, and was "connected with giddiness of the head, cough and difficult breathing, particularly when the body was put in motion." (I am quoting the words of the description.) The girl was "bled largely, without relief"; "blisters were applied with little advantage." She had calomel and opium every six hours, and a saline draught with jalap and aloes every morning. Palpitation set in. Then she was given an emetic, and myrrh and iron without benefit. In something like despair, Dr. Bree next tried gentle exercise on horseback, but pain became insupportable. Examination now revealed "a local swelling from within the margin of the false ribs," and recourse was had to systematic purging combined with nitre, the patient being again in bed. At the end of three months the swelling had disappeared and dyspnœa was less; and after five months' illness the patient was fit to leave her room. The writer concludes that in cases of the kind, of which he had "seen many," when there is much dyspnœa, without fever, attending on heavy uneasiness in the left side, "the spleen may be considered as the suffering part."

It is difficult for one who reads the history of this case, extending over a period of five months in time, and occupying twelve pages of the volume in length, to understand how no attempt was made to employ, even in rudimentary form, the simple methods of physical examination —percussion and auscultation—the elaborate developments of which, and their clinical revelations, have been submitted to us in this Section during the last few months. The condition of the base of the left chest does not appear to have been investigated or considered by Dr. Bree. Yet Auenbrugger's work on percussion had been published in 1761— i.e., exactly fifty years. They had to wait seven more years for Laennec's introduction of the stethoscope.

Yet here and there there are evidences in these papers that beginnings were being made in the employment of other than symptoms for the purpose of diagnosis. Mr. Armiger, in describing a case of aneurysm of the thoracic aorta which caused dysphagia, not only "pressed," as he says, "on the epigastric region" to discover "internal enlargement or hardness," but applied one hand to the left side of the chest and the other to the sternum, and distinguished "agitated action of the heart from a pulsation attended with the sensation which is occasioned by blood passing into an aneurysmal tumour." Further, he determined that the radial pulses were unsymmetrical. But, although the patient complained of a constant humming sound in the right ear, no attempt appears to have been made at auscultation.

But, as I have said, do not let us reflect unfavourably on these evidences of failure to employ methods which to us appear elementary. And, on the other hand, do not let us regard the present developments of the methods of physical examination in other than a proper spirit—the spirit of respect, but not of complete contentment. Let us not forget that at the discussion on pericardial effusion it transpired that when all our elaborate methods of physical examination have failed, effusion can be readily diagnosed from cardiac dilatation by means of the X-rays; and I, for one, am ready to confess that I frequently meet with cases of pulmonary tuberculosis in the earliest stage—associated usually with passing hæmoptysis—in which the X-rays succeed after the stethoscope has yielded only negative signs.

It is in connexion with another case of aneurysm that one meets with the most interesting evidence of genuinely scientific observation in the volume. Astley Cooper, Esq., F.R.S., Surgeon to Guy's

Hospital, contributes a paper on "Dissection of a Limb on which the Operation for Popliteal Aneurism had been performed." I was not prepared to find in it an anticipation of part of the knowledge of the hæmodynamics of the circulation of which so much has been made recently, and which has been illustrated in our Section by Dr. Oliver's paper on his newer methods of estimating the blood-pressure, and Dr. Edgecombe's communication on Low Blood-pressures. Astley Cooper puts forward at the very commencement of his description the fact that "the arteries which form the new circulation are not only enlarged, but have also become *tortuous*." This change he refers to elongation of the vessels: an artery injected with much force becomes serpentine. But, he adds, in "the living artery at the time that the vessel elongates and increases in diameter, its coats also become considerably thicker than natural, and thus it is kindly provided (observe the quaint and beautiful word 'kindly'!) that, as the vessel enlarges, and the original matter of which it was formed is expanded over a large surface, instead of the vessel becoming thinner, the increased determination of blood upon the artery occasions the deposit of additional matter in its coats, and its strength as well as its length and diameter are increased." He adds that "this tortuous course of vessels will be also observed in the arteries of old persons, in whom the coats of the vessels are ossified . . . . the heart is called upon to make extraordinary efforts, by which the blood is sent with such momentum upon the aorta, as to increase its length and diameter, and render its course serpentine." Here, as I have said, we have, plainly foreshadowed, the doctrine of high blood-pressure, arterial sclerosis and increased cardiac action which is agitating the mind of the profession at the present time. It required the genius of an Astley Cooper to discover, from the simple observation of the effect on the walls of an artery of injection of it under different pressures, the elements of some of the fundamental laws of hæmodynamics. How far we have advanced, thanks to him and the workers in physiology, pathology and clinical medicine, during the century, may be gathered from the consideration of the results obtained, and recently described to us, by Dr. Thomas Lewis, under the title of "The Electrocardiographic Method and its Relation to Clinical Medicine."

While anatomical pathology was being pursued in this spirit of scientific inquiry, chemical pathology was interesting another equally active set of observers. Dr. William Henry of Manchester, Dr. Peter Roget also of Manchester, Dr. John Bostock of Liverpool, and Dr. Marcet of Guy's Hospital, all presented papers on chemical subjects.

How little advanced was clinical chemistry one hundred years ago may be gathered from an early sentence in Dr. Henry's paper on "Experiments on the Urine discharged in Diabetes Mellitus." Writing of the *specific gravity* as a test of the presence and amount of sugar, he says: "It may be objected perhaps to the employment of this test that it requires more familiarity with the method of taking specific gravities than falls to the lot of the greater part of medical practitioners. By means, however, of a hydrometer, which is well known to practical chemists, and which may readily be procured at a small expense, the specific gravity may be taken in a few minutes, and with the greatest accuracy, by persons wholly unaccustomed to experiments of this kind."

The main question that Dr. Henry attempted to solve in this paper was whether urea was absent in diabetic urine, as generally maintained by recognized authorities. The test for urea was the addition of nitric acid to the extract of urine dissolved in a small quantity of water. When urea is present, a copious precipitation is immediately produced of bright pearly scales. The outcome of his observations is that the nitric acid test for urea gives a negative result when sugar is in large amount; and that, when allowance is made for the urea that has been lost by its decomposition into carbonate of ammonia, in the distillation of the diabetic urine, the urea in diabetes is not over one-fortieth to one-sixtieth that in normal urine. As for the nature of diabetes mellitus, Dr. Henry concludes that "It appears to be necessary to a just pathology of the disease that some morbid condition of the kidneys should be admitted, though of a kind which has not yet been explained by anatomical investigation": that "the function of the kidneys is perverted so as to render them instruments for forming sugar." But he considers it "probable that the assimilatory organs are also disordered," since diet has great influence on the amount of sugar produced; and his final conclusion is that "neither derangement of the organs of assimilation nor morbid action of the kidneys is of itself sufficient to account for the disease, and that both are probably concerned in its production." May we not say that, in these words of Dr. Henry, read on March 12, 1811, we find evidence not only of the notions that have reached such full development in the papers presented to us last Session by Dr. Cammidge and Dr. Maguire, but also of the spirit of close, patient and determined inquiry which has possessed the members of our Society throughout the intervening century, and still possesses them? Prout, Bence Jones and Wharton Jones all sent in papers on the "Chemical Pathology of Diabetes" in the first half of the century, and in the forty-third volume of the *Medico-Chirurgical Transactions* there is a paper by Dr. Sydney

Ringer on the same subject as Dr. Henry's, "The Relative Amount of Sugar and Urea in the Urine in Diabetes Mellitus," in which, I need not say, an entirely different conclusion was reached.

Two of the papers in the volume were the products of more advanced and complete chemical investigations in physiology and pathology: one on "The Serum of the Blood," by Dr. Bostock of Liverpool; the other on "Various Dropsical Fluids," by Dr. Alexander Marcet, F.R.S., one of the physicians to Guy's Hospital, which it is interesting to note was undertaken in conjunction with Astley Cooper. The principal result of Dr. Bostock's research was to prove conclusively that, in "opposition to the highest chemical authorities" of the time, blood does not contain jelly or gelatine. Dr. Marcet, accepting this result, goes much further, and publishes a table of the constituents of the serum, which corresponds closely with that given in our modern text-books. He also examined fluids from hydrocele, hydatid cysts, ascites, and the pleural and pericardial cavities. Particularly interesting are two other observations of his. First, in his description of the composition of fluid from a cyst on the thyroid gland he mentions that "innumerable small micaceous particles were seen floating in it," and when the sediment was dried it burned with a bright flame. Obviously he had found cholesterin. Secondly, he analysed a specimen of fluid from a case of spina bifida under Astley Cooper; and the result which he published might be accepted now as perfectly correct. And here I may mention in passing that the credit which has been given to C. Schmidt, of having originally propounded the idea that the cerebrospinal fluid should be classed rather with secretions than with transudates, obviously belongs to Marcet, for he speaks of it as "the fluid which the spinal canal secretes." The members of our Section will be specially interested in this reminiscence in connexion with the important paper which they heard on "The Treatment of Tetanus by the Intraspinal Injections of Solution of Magnesium Sulphate," by Professor Llewellyn Phillips, of Cairo.

In those days trismus, as it was called, was treated in a different fashion. J. Harkness Esq., surgeon, of Ratcliff, publishes a case of traumatic trismus which, in consultation with Mr. Thomas Blizard, surgeon to the London Hospital, he treated successfully with an average daily dose of 1 fl. oz. of tincture of opium, 40 gr. of calomel, 51 gr. of gamboge, 2 bottles of wine and 6 pints of porter. As the author puts it, "Under this mode of treatment symptoms of amendment soon began to show themselves, insomuch that in three or four days the patient was able to open his mouth tolerably well." Altogether, according to a table included in the paper, he took 485 gr. of calomel, 825 gr. of gamboge, 204 fl. dr. of tincture of opium, 58 bottles of wine and 191 pints of porter! So impressed was the profession by the successful issue of this case—Mr. Blizard apparently in particular—that on June 18, 1811, a similar case was brought before the Society by Dr. John Parkinson, surgeon. It was that of a case of a lady who was attacked with trismus three weeks after compound fracture of the leg. She was ordered 1 fl. dr. of tincture of opium every hour, and a powder containing 6 gr. of calomel and a scruple of jalap and scammony every two hours. The dose of opium was slowly reduced after a time. In five weeks health was completely re-established.

But I must bring to a conclusion this resuscitation of the spirit of our fathers and their work a hundred years ago. If I am asked what has struck me most forcibly in perusing the volume before us, I reply: Two things. The first of these is the almost total undevelopment of what we call physical examination in medical cases and the interpretation of physical signs. I have indicated this backwardness of clinical methods by a somewhat lengthy résumé of a case of supposed splenitis, which more probably was pleurisy. The second outstanding feature of the medicine of the time is the extraordinary—I may say extravagant or excessive—lengths to which treatment was carried: bleeding for malaria, dysentery, pain in the side, and even for arsenical poisoning; enormous doses of calomel, opium, and powerful purgatives; emetics; blisters, and an unlimited supply of alcoholic stimulants. Whilst we wonder at the hardihood displayed by the doctor as well as by the patient in those days, and dwell with regret on the imperfect knowledge on which treatment of such severity was based, let us acknowledge that there was an element of reason in it, and ask ourselves whether we might not with advantage discuss some of these methods afresh at the present time.

In the passage which I read to you from the "Advertisement" to this interesting volume, you will remember the importance attached by the Council to the progress of the Society in other respects than the characters of the papers: to the number of its members; to the improvement of its library; and to its new house in a more convenient and central situation. Here is an anticipation by 100 years of a reference that I desire to make to a matter which deserves the earnest attention of the Medical Section. During my term of office as President I have been your representative on the Council of the Society. I need not tell you what you all know—that these two years have been an anxious, indeed critical time in the history of the Royal Society of Medicine. The move from Hanover Square came close upon the amalgamation into one great corporate body of a number of independent bodies. To satisfy the demands for increased space and convenience, a step had to be taken which involved great financial concern. This concern is not at an end. Subscriptions to the Building Fund come in but slowly.

For this indisposition of the Fellows and Members to respond to the appeal of the Council there are a good many reasons to which I need not refer, with the exception of one, which it is proper that I should mention on this occasion. I believe that the interest of the Members is so concentrated on the work of the Sections, respectively, that they are in a manner forgetful of the Society as a whole, and somewhat indifferent to its general prosperity. I must be careful not to be misunderstood. I am the last to blame the Members of any Section for devotion to it. The life and health of an organism and of an organization are dependent on the life and health of each of its individual parts. But, conversely, no individual part will flourish unless the general constitution be strong. May I venture to ask you to keep this consideration before your minds, and to promote it practically? You ask me in what way this might be done. How is the interest of Fellows in the Society as well as in the Sections to be increased and maintained?

First, I believe at this present moment, and increasingly in the immediate future, by means of the new house. The position of it, and the splendid proportions and characters that now can be appreciated by simple inspection of it, are calculated to re-assure and favourably influence hesitating Fellows, and the opening of it is sure to increase and consolidate the Fellowship. Here is one reason, and a strong one, to move us to promote individually the Building Fund. It is a matter of getting numbers of subscriptions—not large sums, but many small sums, for which there would be abundant return in the course of a year.

The second means, in my opinion, of promoting the interest of the Fellows and Members in the prosperity of the Society as a whole, is to bring the Sections more frequently together. Mutual intersectional invitations should be more often sent out, and more generally accepted. General discussions, in which members of every Section might take a part, should be continued. We have had no discussion of that character this session, perhaps because the Council, and the officers of it in particular, have had their hands more than full of other matters. And I would suggest that these discussions might, with great advantage to the interests of the Society itself, as well as of medicine, be of a less elaborate and special character: on simple or so-called "common" subjects, of which we all know something in different respects, such as gout, rheumatism, or even the humble disorders of the alimentary canal, when the physician, the surgeon, the dental surgeon, the radiographer, the pharmacologist, the gynæcologist, the neurologist, the specialist in the diseases of childhood, and indeed I may say the Members of all the Sections, might meet on common ground. Meetings for open discussion of the kind would also reduce what I venture to call the danger of specialism—over-specialism. The fusion of the different societies in the Royal Society of Medicine—calculated to reduce this danger—was one of the objects, no doubt, of the promoters of the great move. But the Sections stand too much aloof of each other, which, I maintain, is to their own disadvantage. It has been well said that men working in science should "look out at the window"; their interest in their subject, and the interests of the subject itself, need refreshment from the outside. Is it not our individual experience, when preparing a paper, that proportion and perspective may be easily lost, and that a brief conversation on the subject with a friend is an invaluable corrective? Moreover, when those who are simply Members of the special Section appreciate how much they may learn by intercourse with each other, they will not fail to understand the advantages of becoming Fellows. If all our Members were but Fellows the financial concern of the Council surely would be at an end.

A third means of enlisting and preserving the interests of the Members in the Society as a whole is social intercourse—at the annual dinner, by conversaziones which we may expect to be arranged in the new house, and at the dinners of the Council Club.

Lastly, there is the means of which I have taken present advantage—the opportunity afforded by the Annual Meeting of the Sections, when the President in an address can ventilate the subject of the progress and prospects of the Society. Thus, and by other provisions which will occur to those more experienced and more far-seeing than I, the highest interests of the Sections and of the Society will be promoted together.

As for the Medical Section, let me say in conclusion with how much satisfaction and confidence I leave the management of its affairs in the hands of Dr. Frederick Taylor, as your President here, and as your representative on the Council. Dr. Taylor possesses not only great experience of business successfully conducted, but an intimate acquaintance with the history of the Society and with two at least of its constituent elements before the fusion, the Royal Medical and Chirurgical Society, which he served as Hon. Secretary and Trustee, and the Clinical Society, of which he was President. With him at its head the Medical Section is sure to enjoy the prosperity which I sincerely wish it in vacating this chair.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

VOLUME THE FOURTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1910-11

NEUROLOGICAL SECTION

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1911

Neurological Section.

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

Neurological Section.

November 3, 1910.

Dr. J. A. ORMEROD, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

Two Theories of Hysteria.

IT is first of all my duty, and a most pleasant duty it is, to thank you for the very great honour you have done to me by electing me to be your President for the coming year, and to assure you of the great pride and pleasure I feel in being allowed to preside over your work, and of my earnest wish to help it forward, so far as in me lies. But there may be always a fly in the amber, and a thorn in the rose: and the thorn to me has been this address. For it is a disappointment to me to feel myself unable to put before you some new and original piece of work, as so many of my predecessors have done. I recall that on the very subject I have chosen, hysteria, addresses of this kind have been given you by Dr. Thomas Buzzard and Dr. Sharkey. But I am obliged to forego the ambition to be original; and, in lieu thereof, I ask your attention while I endeavour to sketch out two striking theories on the subject of hysteria—first, that of Professor Pierre Janet, of Paris; secondly, that of Dr. Breuer and Professor Freud, of Vienna. There are some of you, I know, who have already studied these theories in detail—from them I must simply ask indulgence; there may be others whose attention has not yet been turned this way, and perhaps they may find what I have to say sufficiently novel to be interesting.

I shall not, of course, attempt to give you any history of all the theories that have been held about hysteria—interesting subject though that might be—but I shall merely remark that you may find underlying most of them two different drifts of thought, which have only within comparatively recent times become distinctly differentiated. The one seeks to explain the disease on a physical or bodily basis, the other on a psychological. Even the old writers who held the frankly material view that hysteria was due to the wanderings of the womb round the body, like those of a lion round its cage, freely emphasized the mental peculiarities of the patient, though the term "vapours" furnished a convenient physical explanation of these also. Briquet, who led the way in the modern study of hysteria, speaks very cautiously. To him, as to many still, emotion was the key-note of the disease, but he says "hysteria is a disease of that part of the brain which subserves the emotions." Since his time the trend has been more and more towards the psychological explanation of the disease, in spite of the gallant attempts, if I may so call them, of Dr. Bastian and of the late Dr. Savill to put it on a physical basis. Paget spoke of "neuromimesis" and of "want of will," Russell Reynolds of "paralysis from idea," Möbius declared that the disease was "ideogenous." But none of these suggestions carried us very far: first, because they failed to cover the whole ground; secondly, because a patient could have replied with perfect truth, "I make the effort of will as much as ever I did; I know nothing about the diseases you suppose me to copy; and before I was paralysed, no notion of paralysis had ever occurred to me." So the psychologists had to take a wider ground, and this they did most effectually when they included in their theory subconscious mental operations, and the notion (arising out of them) of dissociation of the personality.

Since these conceptions underlie both the theories I shall deal with, something must be said, however briefly, about them first. It seems that there are two classes of mental phenomena; one which at first sight hardly seems to deserve the name mental, because it concerns facts of which we take no personal cognizance. Yet it is certain that impressions of all kinds pour in upon us from the organs of our body and from the outside world; that memories and ideas of the past are constantly rising up within us, of which we have no personal consciousness; that they affect us, that they group and associate themselves just as do our conscious thoughts, and that under some circumstances they may emerge into the light of consciousness, though we never before knew them to exist. These are called subconscious ideas, the workings of the subconscious mind—a phrase which is becoming dangerously popular, since we hear it nowadays from the pulpit and from the daily press. The other class consists of ordinary mental phenomena, the perceptions, feelings, and thoughts of our daily life which are known to our conscious personal selves, and which perhaps seem to need no further explanation. Yet what is this self, this personality, which consciously feels and thinks? According to Janet it is the vast system of ideas which has been growing up, all our lives, by continuous perception, storage, and grouping, of mental facts into a systematic whole. Each time that a new mental fact, however simple or complicated, or the memory of an old one, is brought into relation with this system, and relegated to its place therein—assimilated (to put it shortly)—an act of consciousness takes place. Thus the personality grows, becoming daily a more complicated yet more individual mosaic, as the individual experiences are fitted in.

When did this process begin? What was the birth of this self? I do not know the answer of modern psychology. Aristotle, if I remember rightly, said that the soul did not enter the body till after the first years of life. And so says a modern poet:—

"The baby new to earth and sky,
What time his tender palm is prest
Against the circle of the breast,
Has never thought that 'This is I.'"

But another one says:—

"The soul that cometh with us, our life's star,
Hath had elsewhere its setting
And cometh from afar."

But I am digressing. We must return to the subconscious. Just as our recognized perceptions, our conscious mental states, become assimilated to the system which we call our personal self, so (it is said) may our subconscious mental states, of which we are not cognizant, group themselves into systems of their own. Indeed, this may go on to such an extent that in certain instances there may grow up a second self, independent of our known self. This process is called the dissociation of our personality.

Now, though our subconscious mental life, and the secondary states that may arise therein, play a most important part in hysteria, nevertheless that which, according to Janet, is really distinctive of the disease is what he calls the restriction of the field of personal consciousness. This may be explained as follows: The normal mind can perceive—that is, take in and arrange—many things at once, whether these be sensations, memories, or more complicated mental states, such as emotions or abstract ideas. This comprehensiveness of perception may be called the field of consciousness. The hysterical mind can take in much less. The multiple sensations are there: they reach the subconscious mind (as can be shown in various ways), but they are not assimilated to the system which is called the personal consciousness. As an example is taken hysterical restriction of the field of vision. A normal person, besides seeing the object fixed by him, is conscious of objects at the periphery, an hysterical patient very often is not. At the risk of being a little tedious, I must now try to follow out this theory through some of the symptoms of hysteria, making first this assumption, that in proportion as the field of personal consciousness diminishes, so do the subconscious mental conditions tend to flourish and abound. In this way is accounted for the prevalence and importance of these secondary states of hysteria.

It is natural to begin with some of the mental manifestations of the disease. Owing to the particular disability which has just been mentioned, the patient may become unable to combine certain memories of the past with the perceptions of the present. He then exhibits hysterical loss of memory—amnesia. Strange instances of gaps in the memory are related by Charcot, Janet, and other French authors. We hear less of them in this country; still, they occur. Thus, a girl was under my charge at St. Bartholomew's Hospital, about a year ago, with hysterical paralysis. On questioning her as to the circumstances under which it arose, I found she had entirely forgotten some months of her life at that time. Neither the girl nor her mother had said anything about this loss of memory, so it was found out by chance. Another patient, after falling off a bicycle without apparently sustaining any serious injury, did not know who he was or where he was, or even that he was married, until his wife arrived to remind him. Short of such a complete blotting out of memories, it is obvious that a person who can only bring together a limited number of ideas at once is less checked and governed by past experiences than a normal person, and thus lays herself open to the charge of fickleness and insincerity, whereas she really suffers from a species of forgetfulness. Again, since she can take no comprehensive view of facts and motives, being only able to realize them piecemeal, as it were, reasoned volition becomes replaced by impulsiveness, and she is said to suffer from weakness of will. Further (and this seems to be of great importance), when a particular idea is placed prominently before such a patient's mind, there is lacking the restraining and modifying influence due to the simultaneous presence of other ideas and counter-experiences. Thus the one idea acquires undue importance, and tends to reign supreme over the thoughts and conduct. Hence the importance of the fixed idea in hysteria, and the power of suggestion when an idea is forced on such a patient by another person.

This untrammelled force of a single idea might very well be used as an explanation of the emotional character of such patients. It might be argued that the emotional reaction is violent because the stimulus is untempered. Professor Janet, however, takes a very different view. An emotion, he says (following herein Professor William James), is a very complicated mental state, requiring the combination of a great many mental phenomena. But this is just what the hysterical patient cannot do. Hence she is not emotional, in the sense of readily responding with conscious emotion to the circumstances of the present. The emotions she exhibits are really old ones, relating to circumstances of the past, which have become automatic, old tunes played in her subconscious mind, as if on a barrel-organ. I doubt whether this view will commend itself to everybody.

But to tell the truth, the mental and moral symptoms of hysteria are a little vague; let us therefore turn to the more definite physical symptoms. An hysterical fit is held to be an attack of somnambulism. There are two elements in it: first, amnesia, for all the facts of the patient's real life pass out of her mind, and she takes no notice of her surroundings, except so far as they can be made to enter into her dream. But her mind is not empty, it is possessed with some idea or scene of the past, usually those circumstances which led up to the first fit. In the most typical fit these circumstances are acted out in detail by pantomime or speech. All this is forgotten as soon as the fit passes off, but recurs with the same manifestations when the next fit comes. So that we may distinguish two factors: (*a*) the alternating amnesia, and (*b*) the dominating idea with its appropriate histrionic setting. In such cases we can hardly speak of restriction of the personal consciousness, for during the fit the personal consciousness has disappeared, and something else reigns in its stead. The full-blown fit of this type appears to be commoner in France than in England. It may be that the English patient lacks the dramatic instinct. Anyhow, her performance, if such it be, is far less intelligible to the onlookers and gives much less clue to her thoughts. The fits we see here are generally of the hysteroid type, in which meaningless struggles, rigidity, and opisthotonus play a large part; or the popular "fit of hysterics"—globus, followed by an emotional display, and apparently without loss of consciousness. It is rather difficult to explain these as a scene from the past, unless we suppose that the past of very various patients has been very much the same. I should like, however, to mention here an instance of the dramatic style of fit, the appearance of which made an important difference to the diagnosis of the case. A young sailor was sent into Queen Square Hospital with the following history: In the course of a gun-drill an explosion had happened, and a piece of flying metal had struck him on the head and rendered him unconscious. Ever since he had been subject to fits, characterized (we were told) by unilateral convulsions. It was peculiar, however, that the convulsions were on the same side as the injury to the head. Nothing happened in the hospital till the day when he was going out; then he had a fit (witnessed by the resident officer) in which the whole scene of the gun-drill was re-enacted. He listened for the word of the commanding officer, made the proper reply, went through the manipulations of the gun, and finally, presumably when the explosion happened, fell down unconscious. Up to that time the question had been whether he might not have some local injury to the brain, for which he ought to be trephined.

Hysterical anæsthesia, the most important of Charcot's "stigmata" of the disease, is thus explained on Janet's hypothesis of a restricted field of personal consciousness. Suppose that a patient can consciously perceive only a small number of sensations at once, it follows that he must cease to attend to some sensations, and such failure of attention may become habitual, till at last the conscious perception of them ceases entirely—i.e., he becomes anæsthetic. Whether this be the exact process or no, a psychical rather than a physical explanation of hysterical anæsthesia seems necessary. For, firstly, the distribution of such anæsthesia is not determined by anatomical conditions such as the distribution of nerves and nerve-roots or blood-vessels, but rather by the patient's own conception of his hand, arm, or leg, or whatever the anæsthetic part may be. Secondly, the anæsthesia is physically unreal. Even in cases where it is most intense (nay, perhaps most easily in such cases), it can often be shown that sensations still reach the patient; that he feels though he thinks that he does not feel. Thirdly, the discovery of the anæsthesia often comes to him as a surprise, he did not know of it before he was medically examined. On the strength of this last fact, indeed, Babinski maintains that the anæsthesia is the result of suggestion, put there, in point of fact, by the physician. This may be true for some cases, but certainly not for all, nor even, I think, for most cases.

Now anæsthesia, which we commonly regard in the light of its geographical distribution over the body, really leads up to a more difficult and general question. This lack of personal perception may be "systematized." This is best seen perhaps in amnesia or forgetfulness, which is really only anæsthesia to perceptions of the past, an inability to revive them in consciousness. Such forgetfulness may centre round a particular event or a particular person; everything else is remembered, but all connected with that event or person is forgotten. There may be similar gaps in the perception of present things. Perhaps this is most often seen in artificial hypnosis: the hypnotized subject hears and obeys the voice of the charmer, but not that of others, and he can be rendered blind to a particular object while he sees everything else. Still, the same sort of thing occurs in hysterical somnambulism. But it is added (and this gives to the theory a very general significance) hysterical disabilities may centre not merely around this or that person, or thing, or district of the body, but also around particular bodily functions. We must consider here for a moment what a particular bodily function means. To the physiologist it is an organized grouping of physical events, muscular contractions, secretion of cells, reception, transmission, and arrangement of nervous impulses, and so forth. Some of these groups, let us say those of cardiac action, or respiratory movements, are so thoroughly organized that they seem to be completely automatic, and to have no mental correlation. Doubtless they once had, perhaps they still have, but at any rate it is unimportant. But it is quite otherwise with less organized functions, such as speech, writing, vision, hearing, &c. In correspondence with these, which are being continually developed to meet the continuous demand of new circumstances in which they have to be exercised, there is a vast amount of psychological construction still going on. Mental facts of all kinds connected with their exercise, sensations, volitions, emotions, memories, &c., are being built up into a system corresponding to the function, into the idea of it, if you like. This is the function from the physiologist's point of view, and upon this, according to Janet, the hysterical ban may fall.

This digression was necessary before we consider the explanation of hysterical motor paralysis. There may be several varieties of this. A patient may lose all sensation in a limb, including the sense of position; in such a case she can only move the limb when she looks at it (Lasègue's phenomenon). If, in addition, she has lost the memories of movement, she will be unable to move it all. There was a child in Queen Square Hospital lately who, after some acute illness, had hysterical paraplegia. When asked what had caused the paralysis, she replied, with perfect truth, "I was in bed too long." She had forgotten the use of her legs.

Again, paralysis may be due, as has been said, to idea. The patient has adopted a disabling suggestion that she cannot, or ought not, to move, and forthwith she becomes unable to do so. The idea may be subconscious, the patient not knowing of its existence, or of the relation of the paralysis to it. Lastly, there remains the condition where paralysis seems to affect a function rather than a particular part of the body. The grasp of the patient's consciousness is so limited that she cannot make the combination of mental activities requisite for the exercise of a particular function. The condition called in France astasia-abasia is an example of this. The muscles of the legs are strong, the reflexes normal: there is no anæsthesia, the movements of the legs when lying down or sitting are perfect. Only, the patient can neither walk nor stand. She is paralysed for those actions, and for those only. Nay, the specialization may go further. I remember a lady who could walk quite well in the house, but who could only walk out of doors when she knew some one was by her side. If some one did, she could walk quite well and without support. In such a case the structures necessary for the function are sound—nay, the function itself can be performed, but only under special circumstances. It will be said that is mere nervousness; but then what do we mean by "nervousness"?

The idea of a function as a kind of nervous entity is pushed farther (perhaps you will think too far) to explain certain motor disturbances which are characterized by agitation rather than paralysis. Thus, as mutism is the paralysis of speech, so (it is said) there is a condition of logorrhœa, a deluge of inconsequent talk, in which speech seems to have freed itself from personal control, and exercises itself automatically. It is hinted that hysterical chorea and involuntary movements may sometimes be of this character. But probably most of these latter are due to a fixed idea. Janet gives this instance: A man ever since an attack of typhoid had the habit of making an odd grimace ending with a noisy expiration through one nostril. This was due to the unconscious idea that he had a caked secretion in that nostril, as indeed he had had at one time. He was cured by a counter-suggestion. I may remark *en passant* that ideas seem readily to fix themselves on the nose. A governess used to come to the out-patient room at Queen Square Hospital complaining of "nervousness" which she said had lost her several situations. One day, finding me alone, she told me what the nervousness was. She said, "Can you tell me whether there is a passage from the eye to the nose, big enough to get my little finger down? I can't get rid of the idea that there ought to be." I drew her a diagram of the lachrymal duct, enlarging the size a little to meet her requirements, and she went away saying, "Thank you so much; now I feel that I shall be much better." But I never heard whether she was.

I shall only touch on one more out of the long gamut of hysterical symptoms—namely, localized pain, and, what is still commoner, localized tenderness. These symptoms, apparently physical, are to the psychologist expressions of a fixed idea, generally subconscious, and often connected with the shock that first called the hysteria into active existence. Examples:—

(1) A man[1] hurt his shoulder in a lift accident. Long after the injury was cured there remained a tender point; when this point was touched he had spasms in the shoulder muscles, a feeling of terror, and a sense of suffocation.

(2) A woman came to Queen Square for trigeminal neuralgia. An operation for the removal of the ganglion had been already performed; a second, and finally a third, one was performed. Thus the surgical treatment was certainly sufficiently complete, yet relief was only temporary; the local pains returned, and with them she developed a peculiar mental state and a left hemianæsthesia, undoubtedly hysterical. It was curious to see in the same patient an hysterical hemianæsthesia and an organic anæsthesia in the district of the trigeminus.

(3) A girl[2] had shiverings and horrible feelings whenever her right side was touched. For this she could give no reason, but finally she had an attack of somnambulism, during which it was ascertained that she had once had a dream in which she fancied that a man lay by her right side and attempted to assault her.

So in these three cases the fixed idea originated from an injury, from previous disease, and from a dream respectively.

How does hysteria, then, differ from the allied complaint commonly called neurasthenia, or, more properly, psychasthenia, in which also fixed ideas play so large a part? The psychasthenic does not suffer from the disability characteristic of hysteria—i.e., the inability to bring within range of his personal consciousness more than a few facts at once. Hence he retains the appreciation of his fixed idea, along with that of his personal life. It has not dropped completely into his subconsciousness. Thus it is more under his cognizance and control, but it torments him more. The hysterical patient, on the other hand, is liable to alternations of mental state—one, the paroxysm, in which his personal consciousness is in abeyance, and the fixed idea, unchecked by realities, holds complete sway; the other (the interparoxysmal state), in which he is unconscious of the fixed idea, which nevertheless may be there, and producing some paralysis or other permanent symptom. An elaborate comparison and contrast between the symptoms of hysteria and psychasthenia are given by Janet in his book on the neuroses.

How does the hysterical state differ from that of hypnosis? The answer is that there is no essential difference. Hypnotism is somnambulism artificially produced; and the peculiar relation of the subject to the hypnotizer is produced solely by the gradual introduction into the patient's mind of the idea of the operator's personality; and this, like other fixed ideas, soon comes to dominate his whole mental state. That hypnosis is at bottom hysterical is shown by several facts, such as (1) the similarity of symptoms; (2) that the remembrances of previous hysterical states can be revived in hypnosis; (3) that none but hysterical subjects are hypnotizable. But this last statement is certainly not admitted by all authorities.

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[1] From Janet, "Les névroses," Paris, 1909, p. 171.

[2] From Janet, *ibid.*

I must now turn to the theories of the other two authors I proposed to discuss—Breuer and Freud. It seems that their earlier papers were written conjointly, and are more concerned with the explanation of certain hysterical phenomena than with an exposition of the essence of the disease. On this latter point Freud has given his views in later papers more fully. Their theories are closely connected with, one might almost say founded upon, a special method of treatment. Starting with the facts of traumatic hysteria, as described by Charcot, they regard hysteria in general as a kind of psychical trauma. The patient receives some mental shock which, instead of passing off by degrees, remains buried in his consciousness, or, rather, subconsciousness, as a foreign body might remain in a wound, and constituting a permanent source of irritation, which is manifested by hysterical symptoms. The treatment, to carry their own metaphor a little further, may be compared to opening up the wound and removing the foreign body. Leaving metaphor, the broad outline of the theory is as follows: Every event that appeals to our emotions, and particularly to painful emotions, such as fear, anger, sorrow, &c., raises in the mind a disturbance, stress, and strain, which they call the "affect"; this latter generally finds an appropriate outlet, as in an act of retaliation, or, perhaps, merely in tears or language; this process they call "abreagirung." In English we speak of giving vent to pent-up feelings, or, in common parlance, of "working them off" or "blowing off steam." But whatever name be used, the fact is well known that by outward expression of the feeling the mental tension is relieved.

"Home they brought her warrior dead.
She nor swooned nor uttered cry;
All her maidens, watching, said,
'She must weep or she will die.'"

Only according to Breuer and Freud she will not die, but become hysterical. But how or why should she? If her distress expended itself in tears, the next time that scene recurred to her, the mental tension would be less, probably she would weep again, and that again, as the saying goes, would do her good; till finally the memory would become sad, no doubt, but bearable—"the idea would be shorn of its affect." That is one principle, that complete and appropriate expression of feeling robs the idea of its emotional effect. But if she cannot weep, what then? Either the course of time, the wear and tear of her future life, the calmer feelings that she can bring to bear upon that memory, or perhaps some skilfully-contrived counter-association, like that contrived by the nurse who "slipped her child upon her knee," will gradually bring relief; or else a process may take place which Breuer and Freud call "conversion of the affect," and which they regard as lying at the root of most hysterical symptoms. This is as follows: The unbearable memory cannot be annulled; but the patient banishes it from consciousness. That is, however, a *pis-aller*. "Naturam expellas furcâ, tamen usque recurret." For the idea remains in the lower strata of the mind, the subconsciousness, whence it crops up from time to time, like a submerged rock at low tides, unrecognized, it is true, by the patient, but yet producing the same mental distress as at first. This distress must be relieved, and it is relieved by providing some physical and bodily occurrence to take its place. How this can be done we do not know, but assuming that it can be done, that such a substitution of physical for mental can be made, it is obvious that the act will grow easier with repetition, till finally it becomes automatic. So now the patient, having banished the original idea from his mind and substituted a symptom for the mental "affect," may know nothing of the meaning of this symptom, which to him and to his friends appears to be a new disease, instead of being the effect of an old memory.

The process then involves two steps. First step: The patient by an effort of will forcibly represses an idea which is troublesome to him. How this is done we do not know, but it is very justly pointed out that just as we can voluntarily concentrate our attention on an idea, so probably we can voluntarily withdraw it. Some childish talk may illustrate this: A little girl was afraid to go up some dark stairs because she thought of a wolf there. A little brother's advice was "Don't think of the wolf: think of something nice: think of those cakes." And she successfully did so. Second step: "Conversion of the affect" substitution of a bodily phenomenon for the mental distress. This process we do not understand at all. Perhaps we may call it an extension of the principle with which we began—namely, that appropriate expression of emotion tends to relief. Only in this case the expression often seems singularly inappropriate. Take examples: Why should disappointment in love find expression in the smell of a cigar? Why should hyperalgesia of the thigh follow the nursing of a patient with heart disease? Why should facial neuralgia follow an insult from a husband? The answers to such questions open a fine field for guess-work. The association may be due to mere coincidence in time. Thus the lady who smells the cigar had a shock from the conduct of her supposed lover while he and others were smoking. She was also suffering from a chronic affection of the nose, so perhaps the symptom attached itself to a point of least resistance. The lady with hyperalgesia of the thigh used to rest her father's œdematous leg on her thigh while she bandaged him. The lady with facial neuralgia deals in symbols. When her husband insulted her she thought (consciously or otherwise) "that was a slap in the face," and the neuralgia came. Or there may be, it is suggested, deep and old associations, common to the race, which we can hardly hope to trace out. Perhaps opisthotonus and other fundamental hysterical symptoms may be the expression of these. In this connexion I recall a case of hysterical mutism where, in spite of terrible efforts, the patient could not utter a sound. After several weeks, in response to these efforts, there came a voice so harsh and strained that the whole ward wished the patient was dumb again. Now which of us has not experienced in dreams this painful and fruitless effort to speak, which generally culminates in an inarticulate cry, and the remark from someone close by, "What *is* the matter with you?" Or if this be thought too trivial, let us recall Virgil's description of emotion:—

> "Obstupui steteruntque comae et vox faucibus haesit."

And "dumb-foundered," "struck dumb," are good English expressions.

Now if I have dwelt on this process of "conversion," it is because Breuer and Freud assure us that the power of exercising it—the power, that is, of replacing mental distress by some apparently inappropriate bodily phenomenon—is a special characteristic of hysteria. It may happen otherwise; the distressing idea may be repressed, but the distress itself remain, not having been "converted." Then there results (they say), not hysteria, but a more obviously mental neurosis, such as "phobias" and "obsessions." The voluntary repression of the distressful idea, the banishment of it into the realm of subconsciousness, is the same in both cases. Hence both hysteria and these same phobias and obsessions are called by Freud "defence neuroses." But he recognizes (or, at any rate, his colleague Breuer does) another process by which hysterical symptoms may arise. In this second process the original tormenting idea is never present to the consciousness at all. It has arisen while the patient was in a peculiar frame of mind, which they call a "hypnoid state." The hypnoid state is a sort of absent-mindedness—not, indeed, the absent-mindedness which is produced by active concentration of thought, as in the solution of a problem or the following out of some difficult train of reasoning, but rather a kind of day-dream wherein the thoughts are unaffected by the present or past events of real life, and are unchecked by self-criticism. To be sure, we cannot call everyone who indulges in reveries or builds pleasant castles in the air mentally abnormal, but there is just the risk that something pathological may develop out of this—viz., the leading of a second imaginary life apart from his own, the beginnings of a dissociation in his personality. In this pathological state ideas may develop which remain out of all relation to the current of his normal ideas, and finally establish themselves as hysterical fixed ideas, the sinister results of which we have already seen.

Now the soil on which such hypnoid states grow seems to be mental isolation amid depressing circumstances or remembrances, such as the atmosphere of the sick-chamber of some dear relation, the brooding over misfortunes or injustice—in short, "chewing the cud of melancholy." One emotion, indeed—namely, terror—is credited with producing such a state suddenly and at once. Even animals may be fascinated with fear. Most of us could give instances. I remember one which impressed me much in my boyhood. In those days there was a single railway line between Norwich and Yarmouth. Upon this line, near Norwich, two trains going in opposite directions collided, with very terrible results. A certain person of whom I had often heard was in the rear of the train going towards Norwich. He was not injured, but walked at once (so I heard) past the scene of the accident into Norwich, where he said nothing of what happened, and afterwards, when questioned, said he did not know there had been an accident. Of course kindly critics said that he had neglected the duty of giving help, that he was a coward, and also a liar. But I suppose he was so profoundly affected that he really did not know. What happened to him afterwards I never heard, but there were all the materials ready for a traumatic hysteria.

These, then, are the two principal ways in which, according to these authors, hysteria may originate—either as a defence-neurosis where the idea has been forcibly relegated to the subconsciousness, or as a hypnoid hysteria when it has always been there. A third possibility seems to be that the idea may not be banished, but that, through want of a proper vent to the emotional disturbance it sets up, hysteria develops. This would be, I suppose, what they call a simple "retention-hysteria." But neither of them lays much stress on this, and indeed Freud seems to think that the first-named process—namely, the defence hysteria—is the only one of capital interest.

I ought perhaps to say something here about Freud's method of treating hysteria, called "psycho-analysis," which professes, as I understand, to be not merely a method of treatment, but also the means of eliciting facts on which the theory is based. The principle of it, as I have already said, is to bring the troublesome idea into the light of personal consciousness, and to allow the mental distress which it then produces to have a complete vent, say in tears or in speech; and when this is done the hysterical symptoms (it is averred) will disappear. The idea, being the forgotten memory of some past event, or scene, or feeling, can only be discovered by gradually directing the patient's thoughts into such channels as may lead back to it, and it goes without saying that this may be a most difficult, delicate, and, I should imagine, unsatisfactory process. The original practice was to hypnotize the patient. But this, owing to the difficulties presented by many patients, was abandoned; and instead of it the patient was placed in a recumbent position, with the eyes closed, and told to concentrate her attention on the memories of her past life. If appropriate memories refused to come, pressure was made by the physician's hand on the patient's head, coupled with the assurances that such memories must and will come. The process of directing them and eliciting them was carried on until the crucial memory was reached. Nay, more, they might have to be forced upon the patient (this is Freud's own expression) because the revival of them is disagreeable just in proportion to the original effort made to banish them. As may easily be imagined, the pressure on the head has in turn been given up; and I understand that the method now employed is as follows (I quote from Brill's preface to a translation of some of Freud's papers):—

> "The patient lies on his back on a lounge, the physician sitting behind the patient's head at the head of the lounge. In this way the patient remains free from all external influences and impressions. The object is to avoid all muscular exertion and distraction, thus allowing thorough concentration of attention on the patient's own psychic activities. The patient is then asked to give a detailed account of his troubles, after having been told before to repeat everything that occurs to his mind, even such thoughts as may cause him

embarrassment or mortification. On listening to such a history one invariably notices many memory gaps, both in reference to time and causal relations. These the patient is urged to fill in by concentration of attention on the subject in question, and by repeating all the unintentional thoughts originating in this connexion. This is the so-called method of free association. The patient is required to relate all his thoughts in the order of their sequence, even if they seem irrelevant to him. He must do away with all critique and remain perfectly passive. It is in this way that we fathom the original meaning of his symptoms. But as the thoughts which originate in this way are of a disagreeable and painful nature, they are pushed back with the greatest resistance. This is further enhanced by the fact that the hysterical symptom is the symbolic expression of the realization of a repressed wish, and serves as a gratification for the patient. He strives very hard, unconsciously of course, to retain the symptom, as it is the only thing left to him from his former unattainable conscious wishings and strivings. The object of the psycho-analytic treatment is to overcome all these resistances and to reconduct to the patient's consciousness the thoughts underlying the symptoms. Here lies the greatest difficulty; for, just as in the normal life and the dream, a psycho-neurotic symptom is merely a symbolic or cryptic expression of the original repressed thoughts. . . . To discover the hidden mechanism one must make use of the author's developed method of interpretation; that is, one must look for symbolic action, lapses in speech, memory, &c., and, above all, one must resort to the analysis of dreams, as they give the most direct access to the unconscious."

It remains to notice these later developments of Freud's theories, which are of the highest importance, *if true.* Concerning these I must be very brief. What is the nature of the tormenting ideas which the patient has repressed, and whose emotional "affect" has been "converted" into hysterical symptoms? The answer is that they are always of a sexual nature. The ground of this assertion seems mainly to be that in all hysterical cases which Freud has submitted to his process of psycho-analysis he has found this to be the case, having traced the hysterical symptoms back to some disagreeable occurrence, thoughts, or feelings in the sexual sphere. But this is not all. The simple, though sweeping, statement that sexual memories are the cause of hysteria has been further elaborated. It is supposed that the primary fact is some disagreeable sexual occurrence in infantile life (say, between the ages of 2 and 10) before the actual awakening of sexual feeling which is caused by the advent of puberty; that the memory of this occurrence, which at the time is not understood, lies buried till it is revived at the time of puberty. Then some second event happens which brings the patient into conscious relation with sexual ideas, and the old memory is aroused, bringing with it this time feelings of repulsion and mental distress, which latter are got rid of by conversion into hysterical symptoms. What are the primary sexual occurrences in infantile life which give rise to all this later trouble? In one of his writings, on the basis of thirteen cases, Freud declared that they were actual sexual assaults on the infant. This very surprising assertion he has since withdrawn. What he has substituted for it is, to my mind, by no means so clear or definite. He still maintains that hysteria is the remote consequence of sexual phenomena during early life. These sexual phenomena do not, however, consist in accidental or external shocks, such as assaults or occurrences of a less degree of unpleasantness, but rather in the multifarious unconscious workings within the organism which are caused by the gradual developing of the sexual organs and instincts. These in themselves are not necessarily abnormal, but as time goes on, and especially as the revelations which accompany puberty arrive, the memories of these workings may have to be repressed, some of them, as, for example, homo-sexual impulses, for the sake of the necessary ordering of the sexual life, others perhaps because they are disagreeable to the personal consciousness of the particular individual. But how can unconscious memories be disagreeable to the personal consciousness? Either, I suppose, in the way which has been already indicated—namely, that the mental disturbance set up by the subconscious idea is felt, though the idea itself is not realized; or else—and this appears to be a new addition to the theory—by supposing that the unconscious sexual memories have become transmuted into what Freud calls hysterical fancies, day dreams which do duty for, and are the representatives or descendants of, the original sexual memories. The struggle to repress these gives rise to hysteria, and the conversion of them into bodily phenomena gives rise to hysterical symptoms. I had better quote the author's own words:—

> "The most complicated symptoms become revealed as the converted representatives of fancies having a sexual situation as a content."

And again:—

> "He who can interpret the language of hysteria can understand that the neurosis only deals with the repressed sexuality."

I confess that I see little proof of these statements; they appear to me theory *in excelsis*.

Now, I feel that I may be asked, "What do you think about all this that you have been telling us?" It may be said to me, "You are in the Chair, give us a criticism *ex cathedrâ*." Well, that, gentlemen, I should be very sorry to attempt; for I cannot put myself quite in the position of the authors of these theories. I have not lived among hysterical patients, as Professor Janet appears to have done at the Salpêtrière, nor conducted, as he has done, the subtle analysis of their minds under hypnosis. Nor can I profess to be an adept in the method of "psycho-analysis" as practised by Breuer and Freud. So that any criticism I may make must be of a commonplace rather than of an esoteric kind. To my mind a purely mental theory of hysteria is not altogether satisfactory. The physical phenomena of disease seem to require physical antecedents. If it be said that there is no more difficulty in referring the symptoms of hysteria to mental action than there is in so referring the common voluntary actions of our daily life, I must reply that, with some exceptional symptoms of the disease, there really is such a difficulty. I mean those symptoms which are usually classed as "trophic" or "vasomotor," such as muscular wasting, œdema, hæmorrhage, and perhaps some others. Either we must deny the existence of such symptoms, as Babinski does, or we must suppose that "mind" has much more influence over "body" than most of us would admit; or we must take refuge in our ignorance of the relations between mind and matter, and suppose that hysteria occupies a ground where these two empires meet. Still, in spite of all this, I quite admit the force of what Professor Janet says—namely, that, considering our utter ignorance of the nervous actions which underlie mental phenomena, it may be best to speak of hysteria in terms of mind, and investigate it from the side of laws of mind about which we do know something, remembering and hoping that some day the physical revelation may come as well. Perhaps, then, a mental theory of hysteria promises best for the present. I have already indicated that this kind of theory did not get far until it adopted among its postulates the subconscious workings of the mind. About the existence of these there can be little doubt; but in such a realm both individual facts and general laws are very difficult to ascertain with certainty. They may be the best material we can get for a theory, but they are very unsubstantial, and literally "such stuff as dreams are made of."

What shall we say of Professor Janet's view, that the psychological "stigma" of hysteria, the essence of the disease, is a restriction of the field of personal consciousness? This explains in a very admirable manner some well-known features of the disease—e.g., contraction of the visual field of vision, anæsthesia, and amnesia. To extend it to sundry other features requires, I think, all this distinguished author's ingenuity. One would have thought, too, that there would be some independent evidence of the existence of such a disability; that some of the patients, at any rate, would have recognized it for themselves; and one might urge (as do Breuer and Freud) that many hysterical patients seem to suffer from no mental disability at all.

The theories of Breuer and Freud start, at any rate, with a well-established fact of human nature—namely, the relief that is experienced by giving vent to pent-up mental distress. Yet I suspect they rate the power of the confessional too high. Can we suppose that every severe and ingrained hysterical symptom would be cured in this way? About the so-called process of "conversion," which is really one of the keystones of their theory, one would like some independent proof. Is there really evidence to show that hysterical patients possess a peculiar faculty whereby they can substitute a bodily symptom for mental distress? There is the same lack of evidence for the statement that hysteria originates in forcible repression of a distressing idea; and still more to the statement that the ideas at the bottom of hysteria are always of a sexual nature. We may grant that sexual ideas form a most important part of our subconscious mental state, and that the mental and physical manifestations of them are bound up together in the most remarkable way; but that does not prove that they are the basis, as Freud asserts, of *all* hysteria, indeed of *all* neuroses. The very example with which his theory started—namely, traumatic hysteria—proves the contrary. I know the answer made—viz., that the proof of these statements lies in the so-called facts of psycho-analysis. But can we implicitly rely on the dreamy imaginings of a patient undergoing that process? Is it not very probable that we may put into his mind just what we want to find there? We know that Freud gave up that portion of his theory which relates to sexual assaults because he found that his patients had misled him. And if the master of this method may be deceived, how much more his disciples? To this I cannot help adding, that psycho-analysis, if it is to mean the resurrection of buried sexual ideas, might to many patients do much more harm than good. The proof of the pudding, we are reminded, lies in the eating; but the dish is a little unsavoury, and I cannot help hoping that the taste for it will not become very general.

These are the bare outlines of criticisms which I think might be elaborated so as to obtain some force. But I feel, gentlemen, that I have tried your patience long enough; I shall return, therefore, to that with which I started; and once more thank you for the honour you have done me, and, this time also, for the kind attention with which you have heard me.

Neurological Section.

December 15, 1910.

Dr. J. A. ORMEROD, President of the Section, in the Chair.

Case of Progressive Cerebellar Ataxia.

By F. E. BATTEN, M.D.

S. G., AGED 1 year and 10 months, the first and only child of Jewish parents; normal labour. The boy was perfectly well till 1 year and 8 months old. He had walked when 12 months old, and had talked fairly well before his illness. When 1 year and 8 months old the mother noticed that his hands became shaky, then he became unsteady on his legs, lost the power of walking, then was unable to balance himself in a chair. When first seen on October 19, 1910, he was a well-nourished child in good physical condition. Whilst lying in bed he is quiet; when he is sat up he puts his hands on the bed at his side for support, oscillation soon begins and eventually overbalances him. He has marked ataxia with the hands. When stood on his legs he is able to support his weight, but is wildly ataxic when attempting to walk. There is no nystagmus and no macular change. The knee-jerks are active; no ankle clonus; plantars flexor; abdominals present. The cerebrospinal fluid is normal; von Pirquet's reaction is negative. On November 18 the child developed measles, and since then has begun to show some improvement, but is still ataxic.

It is suggested that this case might be similar to the family amaurotic idiocy of the Jewish race, the stress of the disease falling on the cerebellum rather than the cerebrum. The fact that improvement has taken place since measles tends, however, to negative this suggestion.

[*Note.*—By January 7 still further improvement had taken place, and it would seem probable that recovery will be complete.]

Case of Congenital Cerebellar Ataxia.

By F. E. BATTEN, M.D.

H. J., AGED 5½, the eldest of three children, the others being healthy; instrumental labour. He learnt to talk at 2 years, to walk at 3 years, but never walked well. He is said to be quite intelligent. The boy is well grown for his age; he walks with an ataxic gait on a wide base; he can walk as well with the eyes shut as open. There is no nystagmus, but the boy has an alternating squint. The articulation is rather slow and jerky. There is some inco-ordination of the hands. There is no hypo- or hyper-tonia of the muscles; the power is good. The knee-jerks active; ankle-jerks active; plantar, right flexor, left generally flexor; abdominals equal and active.

DISCUSSION.

Dr. HARRY CAMPBELL said he would be glad to know whether, in those cases of congenital atrophy of the cerebellum, the condition involved any one part more than another. Or was it a generalized atrophy?

Dr. BATTEN, in reply, said that he knew of no recent work on the cerebellum which overthrew the experiments of Luciani, who proved that compensation could take place. Luciani removed one lateral lobe of the cerebellum, and such animals recovered perfectly after a certain time. In the second series of experiments the same investigator damaged the cortex on the opposite side, and those animals never recovered from their ataxia. He (Dr. Batten) had always regarded that as evidence that the cerebellar functions were compensated for by the cerebrum. He was not able to give any answer to Dr. Harry Campbell's question; he had never been able to follow up such cases to prove that one portion of the cerebellum was atrophied without the involvement of the other. When seen in the later stages there appeared to be a general atrophy of the cerebellum, both of the lateral lobes and the vermis.

Case of Toxic Neuritis of Doubtful Nature.

By F. E. BATTEN, M.D.

J. F., AGED 16, is a milk boy and has to clean out the milk-churns. Early in July, 1910, he began to feel pains in both legs from the back to the soles of the feet. He continued his work and rode his bicycle.

July 30, 1910: Taken acutely ill, vomited, had headache, pains in the legs; remained in bed, and on August 3 he could not stand; legs now got gradually weaker, and after some weeks he was quite unable to move them and could feel nothing below the knees. About the end of October he began to improve and regain power in the legs, and since then has steadily got better. He never had any difficulty with micturition, and he says he has not noticed any weakness of the arms.

The boy is thin, and has considerable weakness of the hands. He walks with a high-stepping gait owing to the weakness of the dorsiflexors of the feet. The legs are wasted below the knees. All the deep reflexes are absent; the abdominals are active. There is no sensory loss at the present time. There is reaction of degeneration in the thenar muscle, but no definite reaction of degeneration in the leg muscles. No evidence of poisoning by alcohol, diphtheria, lead or arsenic can be obtained.

It is suggested that the neuritis is due to the same poison as that responsible for poliomyelitis.

DISCUSSION.

Dr. Batten added that he showed the case in order to suggest that it was a case of neuritis due to the same poison as that responsible for poliomyelitis. He did not think that question would be solved until a specific reaction for poliomyelitis had been found.

The President (Dr. J. A. Ormerod) said the case seemed to be similar to those which were published by Dr. Barnes under the title of toxic degeneration of neurons.

Dr. Barnes said he did not think the case was exactly similar to the cases which he published some time ago. They were more widespread, and the atrophy was more marked, particularly in the small muscles of the hand. He did not think any one of the cases was affected on both sides, or both limbs, at the stage at which he saw them. The question raised by Dr. Batten as to whether it was a neuritis set up by toxins, caused by anterior poliomyelitis, was very interesting. The diagnosis was difficult, but the method of onset seemed like that of poliomyelitis, and it occurred at a time when so many cases were seen—viz., in July and August. Within a radius of thirty miles of Birmingham there was at that time an epidemic of thirty to thirty-five cases, which practically all started in the same way, with an intense radiating pain most severe in the regions which were about to be affected by the paralysis. In this case the atrophy was less sharply defined and the paralysis was more widespread and more symmetrical than was usually seen in poliomyelitis. He thought that in all probability there had been a neuritis of some form; there was no evidence that the spinal cord had been affected. Dr. Batten gave a fairly clear account as to the method of onset, but he would like to know if this was the observation of a medical man or if it was simply the boy's own account. It was very difficult to accept such a statement as that he lost sensation below the knee. A patient with motor paralysis frequently said he lost sensation, whereas, on testing, all forms of sensation were found to be normal. The same might be said of the statement that it took from July to the middle of October to reach its worst stage. Medical evidence as to the progress would help in the diagnosis more than anything else.

Dr. GRAINGER STEWART remarked that he had recently seen three similar cases where there was no history of any exciting cause, though one of the patients had had a slight illness and feeling of malaise for some ten days previously, but had not complained of any pain or paralysis until the tenth day. In those cases there was first weakness, most marked in the lower extremities, followed gradually by an increase in the extent and severity of the weakness, which later involved the upper extremities. The patients then developed slight hyperæsthesia and a relative degree of sensory loss. The electrical reactions were lost, the muscles wasted rapidly, and the patients appeared gradually to get worse for a period varying from two to eight weeks. Improvement then set in, and recovery took place. In one case there was paralysis of the face and some weakness of the palate. In the other cases the cranial nerves were not affected. He thought the condition was probably a neuritis, the greatest incidence of which was on the lower motor neuron. Whether it affected the cells first he was not prepared to say, but, clinically, such cases were more like the condition seen in post-diphtheritic paralysis than the condition seen in a toxic neuritis such as that seen after alcohol. On the other hand, they were entirely different from cases of poliomyelitis in the fact that the paralysis took such a long time to develop. In one case when the paralysis reached its worst, the temperature rose to 102° F., whereas it had previously been normal. After remaining at 102° for three days, it gradually came down, and became normal. In each case he knew from personal observation that the maximum amount of paralysis was not reached until ten or fourteen days after the onset, and it was not until after that time that definite objective sensory symptoms began to manifest themselves.

Dr. W. B. WARRINGTON said he did not doubt that similar cases were fairly frequent. He had recently had under his care a similar case. Something, though little, was known as to its ætiology. The patient was a governess in charge of several children who were attacked by some febrile disease, not one of the ordinary exanthemata nor anything like infantile palsy. She nursed the children for a fortnight, and then was herself taken ill, with fever, vomiting, and gastric symptoms. She went to bed, and in the course of fourteen or fifteen days was unable to stand owing to marked paralysis in the legs. The patient then came under his care, and was nursed for a long time in hospital. At that time the peripheral neuritis involved both lower and upper limbs, the proximal parts as well as the distal, also the trunk muscles. A notable feature of the case was intense hyperalgesia of the skin of the lower limb. •She completely recovered, but the illness lasted five months.

Dr. JAMES TAYLOR said he recently had in hospital a very similar case which he had not associated with poliomyelitis. He looked upon it rather as a case of toxic neuritis, the toxin, whatever it was, being more analogous to the post-diphtheritic toxin, or possibly to the toxin of Landry's paralysis. The case was that of a man who had been exposed during a long walk and ride one Sunday, and the following day felt ill. During the next few days he became worse, with gradually increasing pain in the legs, and weakness, so that at the end of the third week, when he was in hospital, he had practically complete paralysis of both upper and lower limbs, and also of the face, so that he could not whistle; he also had squint, although he did not complain of diplopia. There was also complete loss of both superficial and deep reflexes, and a slight degree of blunting of the touch sensation below the knees and below the elbows. He had to be catheterized during three days, and then gradually began to improve and was now well. The analogy to Landry's paralysis struck him as being fairly close, although that disease was supposed to be fatal, a supposition which he doubted. A patient, in hospital four years ago, had what most people would call Landry's paralysis, and recovered. But he afterwards developed paralysis, apparently peripheral, of one serratus magnus; this passed off completely, and the patient got well. He would be inclined to connect Dr. Batten's case with Landry's paralysis rather than with anterior poliomyelitis. He thought there was a definite type of peripheral neuritis, possibly caused by the same toxin which produced Landry's paralysis, but of less virulence.

A Case of Pseudo-bulbar Paralysis of Doubtful Origin.

By T. GRAINGER STEWART, M.B.

E. L., AGED 24, married. Family history: Eight brothers and sisters alive and well. Married six years; four healthy children, no miscarriages. Past health: Good. Present illness: Three years ago—gradual onset, without fits, of weakness in right arm and leg; no aphasia; in three weeks was unable to walk or raise right hand to mouth; recovered completely under treatment in seven months and has been bright and well since. Six months ago—gradual onset of weakness and stiffness in both legs; in three weeks unable to walk; both arms were slightly weak; no aphasia or sphincter trouble ; at the end of seven weeks' treatment she again became apparently well, but became tired easily. Fourteen days before admission—woke up suddenly with "peculiar feeling" in head ; began to tremble all over ; two hours later suddenly had a fit, turned head and eyes to left, extended left arm rigidly, making stiff, slow flexor movement of the clenched hand upon the forearm, extended left leg rigidly ; no clonic movement ; patient unconscious and incontinent ; duration, ten minutes ; during the day six more similar attacks ; she was very drowsy for the rest of the day. Since then she has appeared sensible, has been very weak in all limbs, and unable to speak except in an indistinct whisper. Throughout, no headache or vomiting ; vision unaffected.

Present condition : Attention poor ; cerebration usually slow ; optic disks normal ; no hemianopsia ; special senses good. Cranial nerves : Pupils react slightly less well to light than to accommodation ; seventh—bilateral spasticity of face, right weaker than left, emotional movement good ; ninth and tenth—speech only a whisper and articulation very poor ; twelfth—scarcely any movement in tongue, which shows no wasting. Motor : No local wasting ; tone everywhere increased, but much more on the left side ; movements in right arm of good range and power, movements in left arm of poor range and power ; much difficulty in executing the finer movements with left hand. Sensory : No anæsthesia, analgesia ; no loss of muscle-pain sense ; no loss of sense of passive position ; some loss of sense of active muscular contraction in left hand and arm. Reflexes : Jaw—very brisk ; upper extremities—right well marked, left exaggerated ; epigastric—absent ; abdominal—absent ; knee-jerk—right brisk, left very brisk ; rectus clonus on left side only ; ankle-jerk—right weak, left very weak ; ankle clonus—present right and left ; plantars—left always typical extensor, right usually very doubtful, occasional extensor. Sphincters not affected. Gait : Can walk with assistance ; drags both feet, the left much more than the right ; when standing tends always to fall straight backwards ; makes scarcely any attempt to recover balance by bringing the feet under the centre of gravity. Cerebrospinal fluid colourless ; came away in drops under normal pressure ; no lymphocytosis. Wassermann reaction negative in blood and cerebrospinal fluid.

DISCUSSION.

Dr. JAMES TAYLOR expressed the opinion that it was an extremely interesting case of bilateral, possibly multiple, cerebral lesion causing bulbar symptoms —so-called pseudo-bulbar paralysis. With regard to the situation of the lesion, that seemed to differ from most of the other cases in that the fits were a marked feature. In most of the cases in which a lesion had been described, it was situated in the lenticular nucleus. But a bilateral lenticular lesion would scarcely cause all the symptoms which this case presented. He thought there was something more in this case—possibly a multiple cerebral lesion: certainly both hemispheres were involved, and he thought there was evidence of descending pyramidal degeneration on both sides. Of all the cases of pseudo-bulbar paralysis which he remembered reading about, this one most closely resembled one of the early cases reported by Sir Thomas Barlow,[1] in which there had been successive embolisms on each side of the brain.

Dr. GRAINGER STEWART, in reply, agreed that the case resembled that which Sir Thomas Barlow had described. The case had certain features which he hoped to learn more about when he had seen the patient for a longer time.

Case of Syringo-bulbia.

By H. R. PRENTICE.

M. L. S., AGED 26, cook, unmarried, under the care of Dr. Gordon Holmes at the National Hospital. Family history: *Nil.* Past history: *Nil.* History of present condition: August, 1909—weakness in right knee, later in left knee; in a month both legs so stiff that patient could not walk at all; some loss of sensation up to the knees, particularly for thermal and painful stimuli; slight difficulty in micturition. February, 1910—spasticity began to diminish and has become progressively less since. May—began to walk. Patient had noticed no affection of arms or head.

Present condition: Well nourished; viscera healthy; slight cervico-dorsal kyphosis; cerebral functions unimpaired; special senses unimpaired. Cranial nerves: Third, fourth, and sixth—left pupil slightly larger than right, *nil* else; fifth—normal right and left; seventh—slight facial asymmetry without motor loss; ninth, tenth, and eleventh—speech and swallowing unaffected, palate deviates to left; twelfth—

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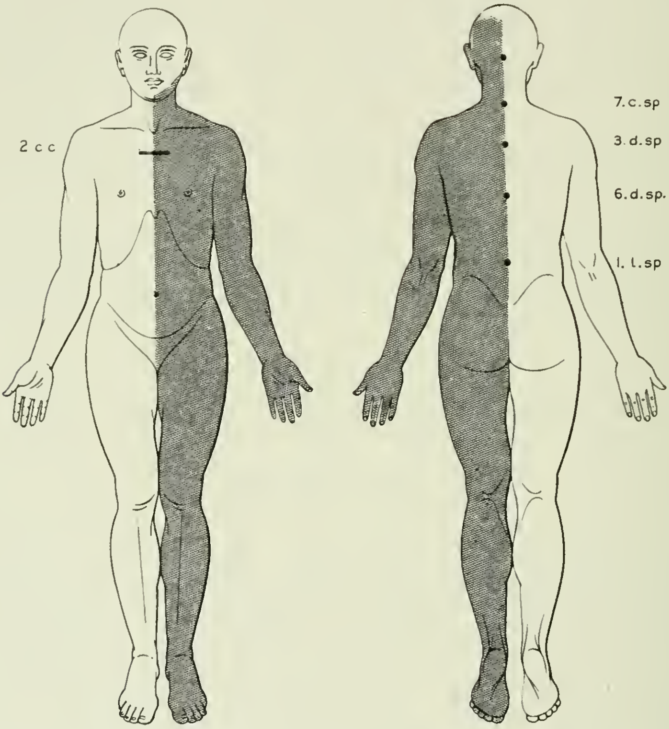
[1] *Brit. Med. Journ.*, 1877, ii, p. 103.

marked hemiatrophy with fibrillary tremor on right side, slight longitudinal folding on left. Motor system: Wasting of right sternomastoid and left supraspinatus. Upper extremities: Right normal, left spastic with consequent limitation of power and range; no definite inco-ordination. Trunk: Slight kyphosis in cervico-dorsal region. Lower extremities: Left spastic, right tone good; some loss of power on left side; no inco-ordination. Sensory system: No anæsthesia to cotton-wool touch; a very high degree of analgesia and thermanæsthesia on left side from first cervical level downwards; sense of active muscular contraction good in all limbs; sense of passive position slightly defective in both upper limbs only; muscle pain sense slightly defective in left arm and leg. Reflexes: Upper extremities—exaggerated on left, well-marked on right; epigastric—absent right and left; abdominal—absent



Sensation chart showing a high degree of analgesia and almost total thermanæsthesia for both heat and cold.

right and left; knee-jerks—left brisk, right present, not brisk: ankle-jerks—left brisk, right present, not brisk; no ankle clonus; plantars—double extensor. Sphincters normal. Gait: Drags left leg: no Rombergism.

DISCUSSION.

The PRESIDENT said that two years ago he saw a case which also came under the notice of Dr. Gordon Holmes. The patient was a woman who had paraplegia associated with very marked sensory paralysis of the dissociated type. She could not feel heat and cold, but retained sensation for tactile stimuli. It seemed to be either a case of syringomyelia or some tumour of the internal part of the cord. She was for a long time in Queen Square Hospital, but got progressively worse. After she had left he was asked, as a matter of charity, to take her into St. Bartholomew's Hospital. He admitted her there for a time, and to his surprise, though she had the same treatment as at Queen Square—iodide of potassium, massage, and electricity to the legs—she began to improve, and when he last saw her she was able to walk. That showed that cases with grave cord symptoms might improve without drastic treatment.

Dr. WILFRED HARRIS asked how high up the left side the analgesia and the thermanæsthesia extended. He also asked what evidence there was that it was syringomyelia. Why could it not be a focal lesion, possibly hæmorrhagic, and not due to hæmorrhage into the syringomyelic cavity? This case did not remind him of the cases of syringomyelia which he had seen involving that region. He had two now in which there was hemiatrophy of the tongue, and in those cases the thermanæsthesia involved the same side of the body much more extensively than on the opposite side, although on the opposite side there was some analgesia. In both his cases there was anæsthesia of the second, third, and fourth cervical segments on both sides, and on the same side wasting of the tongue. In one case there was nystagmus on the same side. Another case he saw fifteen years ago, and it was then called hæmorrhage into a cavity. The onset was sudden, with intense pain in the back and paralysis, whilst he was bathing at Folkestone. He was paraplegic, analgesic, and anæsthetic up to the second rib. He developed complete paralysis of the triceps, serratus magnus, latissimus dorsi, and the lower half of the pectoral, which was to be ascribed to a lesion in the sixth cervical segment. Those muscles wasted and had not altered. The anæsthesia cleared up, but the thermanæsthesia did not. Though that was regarded as syringomyelia with hæmorrhage in the cavity, it had not shown any further sign, and he had seen the patient at intervals since the onset. He doubted whether it was syringomyelia at all.

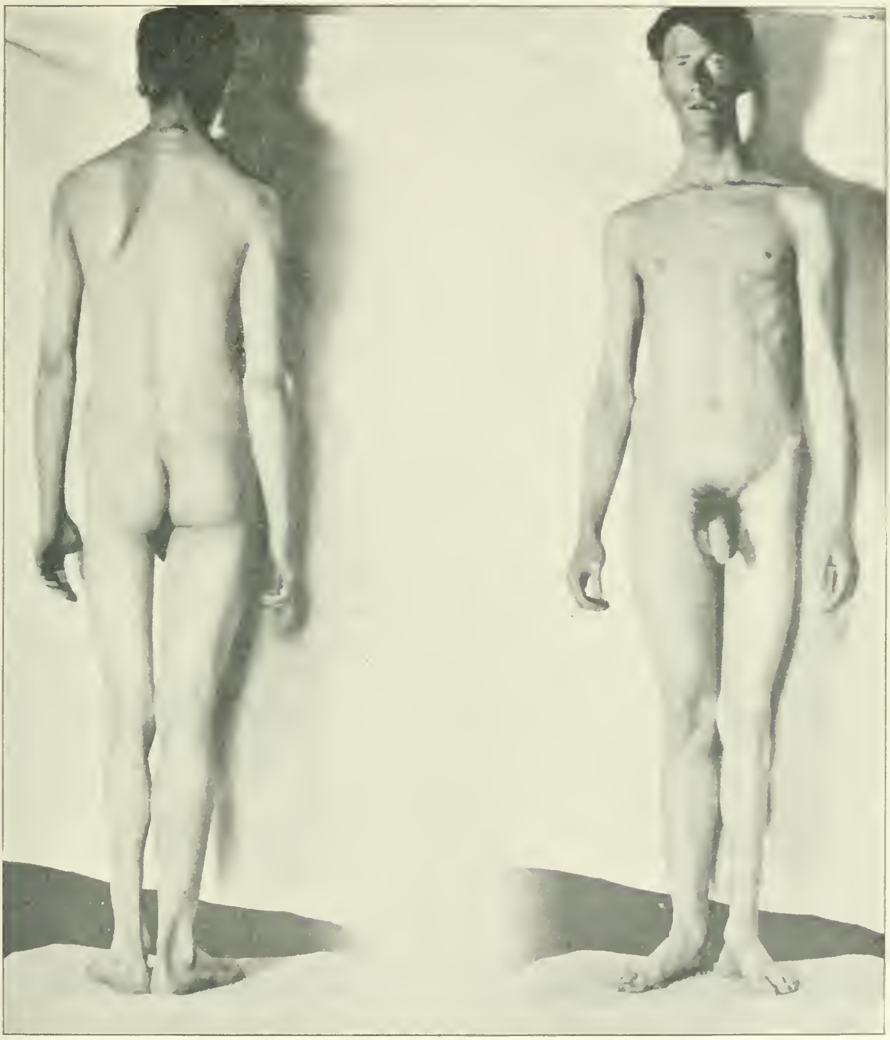
Dr. Grainger Stewart said that last year he saw a patient—a typist—who presented the clinical symptoms of a central cord lesion in the cervical region. The illness had come on progressively, but had become rapidly worse in fourteen days. When she was admitted to hospital she could not use either arm at all; she had dissociated sensory loss in both arms and spastic paralysis on the left side, with extensor response. With quiet and absolute rest in bed she had improved, and after four months she was able to go back to her work and do some typing; she was so slow at it, however, that she gave it up and took to using a pen. She remained well until last week, when she complained of weakness and numbness in the legs, and he found, on examination, that she had evidence of a cavity in the lumbar cord, with dissociated sensory loss there. There was an increase in the cervical symptoms, the lesion having spread upwards two segments. Formerly the seventh cervical was the highest level; now she had got sensory changes as high as the fifth cervical segment. He did not think it was a case of hæmorrhage into a cavity, but rather an œdema, or some subacute inflammatory change, which had passed off under rest. He thought most cases of syringomyelia improved greatly if kept quiet.

Dr. Prentice, in reply, said he had had some doubt as to whether it was syringomyelia, and he tried to picture some lesion which would explain the symptoms if it were not syringomyelia. The weakness in the arms and legs developed during a month, and the onset was gradual, first weakness in the right knee, later in the left knee; then the legs became more and more stiff, and later the arms. At the end of a month she was almost completely paralysed up to the neck. He could not think of a lesion which would explain that, except some rapidly developing tumour, or possibly a gliosis, in that region. A syringomyelic growth might easily explain it, and the fact that it had retrogressed so markedly since February, and was still improving, seemed to fit in with the progress of a syringomyelic case. He suggested that there was primarily a paralysis due to growth, and cavity formation afterwards, leading to freeing of the tracts pressed upon. X-rays had been applied, but he thought the rate of progress during their application was the same as when the case was simply watched, and local treatment applied to the limbs.

Notes of a Case of Hemiatrophy from Sclerodermia.

By Philip Coombs Knapp, M.D. (Boston, U.S.A.).

Male, aged 18, metal worker. Family history negative, previous health and habits good. No use of lead. Two years ago some sharp, paroxysmal pain in the left leg, followed by spasmodic, clonic



Hemiatrophy from sclerodermia.

contractions of the muscles of the left thigh and wasting of the whole leg. At about the same time he noticed pigmented nodules in the left epigastric region, on the anterior aspect of the left thigh, just below the knee and along the lower inner border of the gastrocnemius. For six months he had had some "drawing" sensation and spasm near the sternal attachment of the abdominal muscles, and for two weeks some spasm and weakness in the left upper arm, especially in the biceps. The left leg had grown weaker, and sometimes gave out if it had been held in a strained position. There was no change in sweating. His general health was good and he was able to work regularly. No other symptoms could be elicited on careful inquiry.

There was wasting of the left half of the body, slight in the face, evident in the arm, and marked in the leg. The upper arm was 1·5 cm. smaller, the forearm 2 cm. smaller, the thigh 9 cm. and the lower leg 6·5 cm. smaller. The leg was 3 cm. shorter. A skiagram of the legs showed no noticeable difference in the size of the bones. The shortening of the leg had led to a tilting of the pelvis and compensatory curvature of the spine. There was well-marked wasting over the lower ribs from the median to the anterior axillary lines, so that the pulsations of the heart were very noticeable. The left side of the glans penis seemed smaller. The skin on the left side was thinner, smoother, and more translucent, especially over the thigh and the anterior chest wall. The superficial veins were more marked, there was very little hair on the leg and less over the entire half of the body, although it was formerly as thick as on the right side. This loss of hair and the wastings are well shown in the photograph. The skin was not adherent, did not compress the underlying parts especially, and was fairly elastic. In the regions indicated in the diagram were a few firm, slightly elevated nodules, brownish yellow in colour, coalescing into a broad, flat mass, and freely movable with the skin, of which they seemed to form a part. A few dilated vessels were seen about them. The patch in the epigastrium was about 10 cm. in diameter, that on the thigh 2 cm by 10 cm. in extent. These were pronounced by Dr. C. J. White to be sclerodermia. The left arm and leg were weaker, but all movements were normally performed. There were occasional quick contractions of the left quadriceps, drawing up the patella. The muscles on the left side reacted a trifle more quickly and to a weaker current. Sensation was a trifle more distinct on the left, for contact and vibration, but equal for heat, cold, and pain. The skin and tendon reflexes were a little livelier on the left, except the abdominal reflex, which was greater on the right. The left pupil reacted a little more sluggishly to light. Examination of the eyes, ears, throat, nose, chest, abdomen, blood and urine showed nothing remarkable. The blood-pressure was 127 mm. in the right arm, 121 mm. in the left. Under thyroid treatment and electricity (galvanism, Röntgen rays, high frequency) he felt better, but a few more sclerodermatous nodules appeared and the wasting was more marked, although he did not feel weaker. An examination of the literature has shown only six or seven other cases of complete hemiatrophy from sclerodermia.

Traumatic Neuritis of Inner Cord (Klumpke-paralysis); Prefixed Type of Plexus.

By WILFRED HARRIS, M.D.

A. M., AGED 55, labourer, on July 28, 1910, fell from the top of a hay-cart on to his right shoulder and broke his right collar-bone. The arm was bandaged to his side for three weeks, and when it was taken down he noticed weakness of right hand and numbness of inner side of arm and forearm. Weakness increased for some time, he thinks, but improving lately.

Present symptoms: Complete wasting of right thenar muscles and pads of thumb and first finger, but none of other finger-pads. Marked wasting of interossei and hypothenars; wasting of right lower pectoral. Almost complete palsy of flexors of fingers, especially of median supply. He cannot flex his index. No weakness of flexors of wrist, palmaris longus, or pronator radii teres. Sensation: Almost complete loss of touch and pain along inside of arm and forearm and hand, including little finger; less marked on inside of third finger and in axilla and upper inside of chest. No loss of sensation on thumb or two outer fingers. Bilateral Argyll-Robertson pupils. Marked aortic regurgitation; knee-jerks brisk. X-rays exclude aneurysm.

Electrical: Fair to faradism in flexor carpi radialis, palmaris longus and flexor carpi ulnaris, though distinctly sluggish reaction in the latter both to faradism and to galvanism. Pronator radii teres normal. Very little flexion of fingers either to faradism or to galvanism. No reaction to either current in any intrinsic hand muscles. In the lower pectoral weak reaction both to faradism and galvanism.

Note *escape* of flexor carpi ulnaris, indicating the plexus to be of prefixed type, with probably a moderately large branch reaching the ulnar from the outer cord (outer head of ulnar nerve).

The PRESIDENT said he remembered a patient who came for what was called paralysis of the fingers, but when examined he was found to have sclerodermia of the forearms, which so entangled the tendons of the fingers that he could not move them.

Neurological Section.

May 4, 1911.

Dr. J. A. Ormerod, President of the Section, in the Chair.

Case of Congenital Brain Defect, with a curious stamping gait and tendency to fall backward, suggestive of a Functional Astasia.

By F. E. Batten, M.D.

This case is shown because the child exhibits a peculiar gait which if seen in an adult would suggest a functional condition, or that which is seen in some of the degenerative conditions of the cerebellum, closely allied to olivo-ponto cerebellar atrophy of Dejerine and Thomas.[1]

W. T., aged 5½, is the sixth of seven children, the youngest of whom is dead; all the other children are healthy. He was born at full term in normal labour, but was backward in learning to talk and walk; he has never walked by himself. He is a well-developed boy, not quite clean in his habits, with probably some slight mental deficiency, but he will do what he is told. He is unable to stand alone or walk; if stood up he tends to fall backward, makes a rapid stamping movement with the feet, and if urged forward makes ataxic movements with the legs, but is quite unable to make any attempt to balance. He "buttock" walks very well, but will not or cannot crawl. He can sit up well. He has very little inco-ordination of the legs when lying in bed. His arm movements are good. There is no spasticity of the legs, though they give one the impression of rigidity at times; there is some hypotonia. The eyes are normal. The articulation is very defective. The knee-jerks are active; the left plantar reflex is doubtful; the right plantar is flexor; the abdominal reflexes are active.

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[1] *Nouv. Iconog. de la Salpêtrière*, Par., 1900, xiii, p. 330.

DISCUSSION.

Dr. WILFRED HARRIS asked whether Dr. Batten really associated the peculiar gait with a defect of the cerebellum, although he (Dr. Harris) thought himself that there was certainly an arrested mental development. He also wished to know whether Dr. Batten suggested the lesion was above or below the tentorium.

Dr. GRAINGER STEWART said he thought the case a most interesting one, but one the nature of which it was impossible to determine until the child became older, when he would understand more and make greater efforts than he at present seemed capable of. He had noticed the child's tendency to fall backwards; this he considered to be due to ataxia, probably due to an affection of the middle lobe of the cerebellum. With regard to the peculiar attempts at walking, he thought that they were due in part to the cerebellar lesion, and in part to a defect in the mental development of the child.

Dr. BATTEN said that the child would not crawl, and directly one attempted to make him do so he got into a sitting position and started his "buttock" walking. He did not think the child was mentally defective though he was somewhat backward. He would obey and perform all movements he was told to execute. Dr. Batten was inclined to agree with Dr. Stewart that the defect was in the cerebellum and probably in the mid-lobe. The tendency to fall backwards; and inability to keep balance, and the absence of incoordination in the arm, were in favour of this view.

Case of Syringomyelia, with Symptoms precipitated by Trauma.

By E. GREAVES FEARNSIDES, M.B.

(For Dr. HENRY HEAD.)

A. G., AGED 48, a house painter and decorator, came to the London Hospital on February 22, 1911, complaining of a tight feeling associated with pins and needles in the right side of his head and neck, and general nervousness. He dated his illness from a fall off a scaffold sustained on September 29, 1910, and stated that previous to this date he had enjoyed the best of health and spirits. On this date, whilst he was at work on the top of a scaffold, he slipped and tumbled. He fell on to his right shoulder and caught the back of his head, cutting open the scalp over the occiput. He did not lose consciousness, and with the help of a comrade managed to walk the few hundred yards to his home. He was seen immediately by his doctor, the wound was cleaned up, and two stitches were inserted. The wound suppurated, and he was laid up in bed fourteen days. Whilst he was in bed, he thinks on the evening of the accident, the right side of his head and neck "went funny," and felt as if it were "in a vice." He had been under the care of his own doctor ever since the accident, complaining of pains in his head, neck and right upper limb. The pains had been almost continuous; they came on worse in attacks; the attacks were brought on by exertion, whenever he attempted any task, and had been severe enough to make him lie down and to cause him to "come over all in a shake."

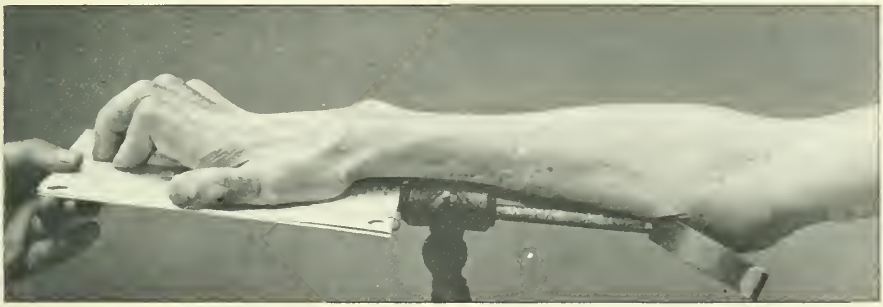


Fig. 1.

Previous health: He has been a house painter for twenty-eight years, but he had never had painter's colic or other symptoms suggestive of plumbism. Six years ago he had an attack of "influenza," with pyrexia, coryza, and pains in the back. Five years ago for a while he was out of employment and looked after a skittle alley; during this time he thinks he must have ricked his back, for his spine became twisted and he "one-sided," but previous to the accident in September last his back had never caused him trouble. Two and a half years ago whilst carrying a ladder he jambed his right thumb. The wound became poisoned and would not heal. Eight weeks later some "dead bone" was removed from the distal phalanx, and then the wound healed within a week. Following upon this, and, as he states, due to it, gradually the fingers of his right hand began to get drawn up, and the muscles of his right hand and forearm to waste, but as the wasting caused no grave disabilities previous to his arrival at the hospital he had taken no notice of the condition of his hands. Since the accident of September, 1910, the right hand had wasted rapidly, and the left little finger had begun to draw up. The patient had never associated the pains with the condition of his hands and spine. He has been married twenty-two years, and his wife has had six children, of whom four survive. His wife has suffered for some years with myxœdema. He denies all venereal disease. He has not been a heavy drinker, but at one time he smoked heavily.

Present condition: Motion—right upper extremity (fig. 1): Small muscles of right hand much wasted; thenar and hypothenar eminences flattened; thumb extended, lying in the plane of the palm; all interossei atrophic; proximal phalanges of fingers hyperextended at the metacarpo-phalangeal joints with flexion of the terminal phalanges; only slight contraction seen or felt when abduction or opposition of the thumb is attempted; interossei movements impossible; abduction of the fingers almost impossible. Muscles of right forearm all wasted, flexors to a greater extent than extensors; all movements of right wrist possible; wrist-drop slight; triceps and biceps wasted; rhomboids and deltoids slightly wasted; abduction and circumduction of the shoulder limited; passive movements at shoulder free. Left upper extremity (fig. 2): hand shows wasting similar to that of right; interossei chiefly affected; thenar and hypothenar eminences somewhat flattened; little finger hyperextended at metacarpo-phalangeal joint; abduction of the fingers limited, other movements fair; condition of forearm fair; triceps definitely wasted. Electrical: With the exception of the thenar groups and the interossei of both hands and the long flexors in the right

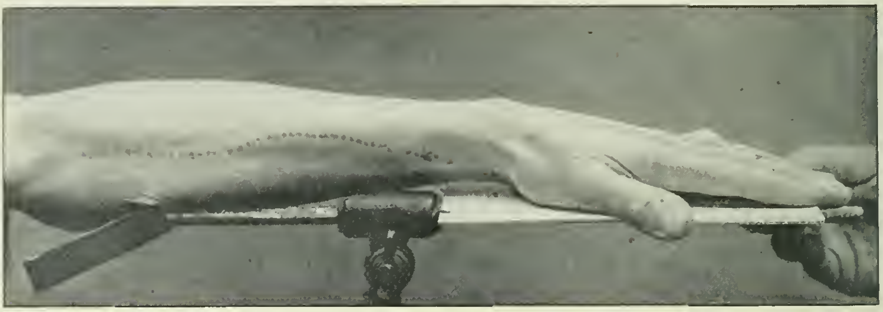


Fig. 2.

forearm, which gave diminished or absent reactions to the interrupted current, and sluggish, diminished contractions to the constant current; all the muscles of both upper extremities reacted well to the interrupted and to the constant current. Right shoulder raised; head habitually tilted over to right. Kypho-scoliosis involving the whole of cervico-dorsal spine, with greatest convexity at the second dorsal vertebra; passive movements of neck to right and left free. X-ray:

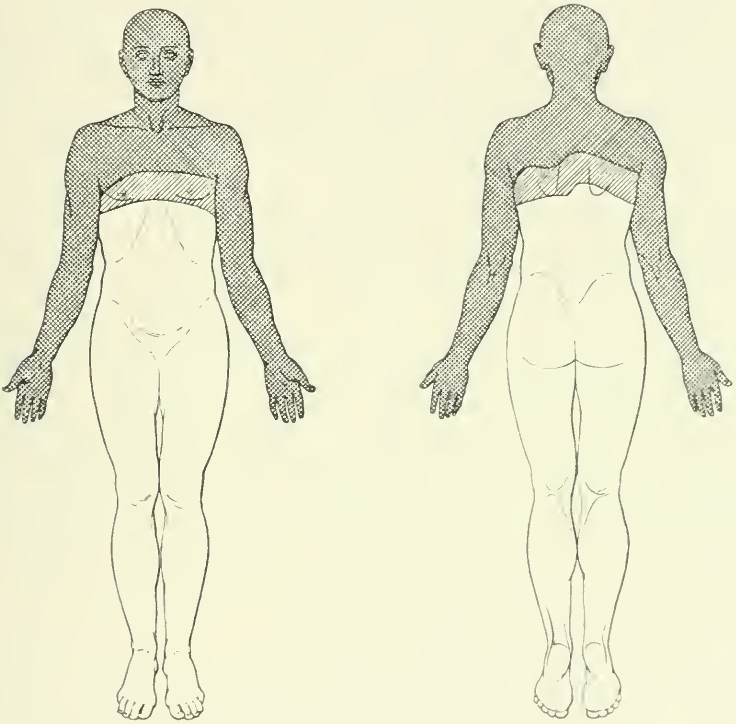


FIG. 3.

Light shading indicates thermo-anæsthesia; dark shading indicates mixed analgesia and thermo-anæsthesia. The borders are nowhere very distinct, and the analgesia and anæsthesia nowhere quite absolute.

no actual bone disease. Gait shuffling. Romberg negative. Grasps good; co-ordination of hands perfect. Leg muscles neither wasted nor spastic; fibrillary twitchings constant in muscles of upper extremities below the shoulders, most noticeable in upper arms and right forearm and also in flat abdominal muscles. Reflexes: Knee-jerks brisk; no ankle-clonus; plantar reflexes flexor on both sides; abdominal and cremasteric reflexes not obtained. Sensation: Numbness of right hand and "pins and needles" constant in right neck region; in attacks "tight feelings as if his head and neck were in a vice"; attacks associated with mental dullness and ordinary reactions of pain. Two attacks seen in hospital. Analgesia to pin-prick and to painful interrupted current everywhere above distribution of Thor. 4 on both sides (fig. 3); borders soft; analgesia nowhere absolute. Thermo-anæsthesia almost coincident with analgesic areas, nowhere absolute. Tactile sensibility everywhere perfect. Sensation of passive position and passive movement unaltered. Nails of right thumb and left middle finger ribbed, nails of other fingers slightly irregular; joints normal. Vision: Fields for white and colours normal; disks and fundi good; arteries, early "silver-wiring." Smell, taste, and hearing normal. Cranial nerves; Left palpebral fissure wider than right; no nystagmus; no ocular paresis; left pupil in the shade wider than right; to light and accommodation both pupils reacted normally; facial movements good; fifth cranial nerve supply shows partial analgesia with some loss of sensibility to heat and cold. Tongue straight, a little tremulous, and slightly asymmetrical (? left side slightly greater than right). Palate anæsthetic, movements good. Larynx normal, no dysphagia. Sphincters unaffected. Heart not enlarged; vessels generally thick and tortuous. Wassermann reaction in serum positive, in cerebrospinal fluid negative; no lymphocytosis in cerebrospinal fluid.

DISCUSSION.

Dr. F. J. SMITH said he felt a little out of place amongst such learned neurologists in appealing to any other aspect of the case than that of diagnosis. The point he wished to raise was as to the precise relationship of the traumatism to what, he supposed they must admit, was definite organic disease of the spinal cord. He was not so much concerned with the precise nature of that organic disease, as with the exact causation of it—namely, its relation to those traumatisms which the man had definitely undergone upon two separate occasions. Judging from the man's history, the condition might have started six years ago with the influenza, if they assumed that he had at that time a microbial invasion; two and a half years ago the man had, at any rate, a very definite microbial invasion—viz., the poisoned thumb. Then last September found him with his third microbial invasion in the shape of a suppurating wound of his scalp. He wished to ask how far they were justified in attributing the organic disease of the spinal cord on the one hand to definite poisons, either influenzal, streptococcic, or whatever these might be in a poisoned wound, giving rise to a distant meningitis and myelitis; or, on the other hand, to simple shock—whatever "shock" might mean—but assuming it to mean mere nerve impulses. Obviously, the meaning of his question was, were they to allow that traumatic neurasthenia, or traumatic shock, might end in definite disease? With regard to this particular case, had they not the very definite microbial invasions to account for the present organic changes? It seemed to him to be an exceedingly important point in view of the great frequency of simple traumatic neurasthenia in which disease a man simply knocked, not cut—perhaps scarcely bruised—might subsequently develop some apparently serious symptoms, but which were found, as a matter of experience, to clear up entirely. This man's symptoms, however, had not cleared up at all, and the point he wished to raise was the relationship of the microbial invasion to the fact that they had gone on to organic change.

The PRESIDENT (Dr. J. A. Ormerod) said the first question—the relation of trauma to progressive organic disease—was a very big one; and the second, he understood, in relation to this particular case was whether these symptoms were precipitated not so much by the trauma as by infection resulting from the trauma.

Dr. BUZZARD suggested that this case was rather an unfortunate one upon which to start a discussion on the relationship of trauma or infection to the origin of syringomyelia. If the history of this patient was carefully analysed it would appear that his back was known to have become twisted and one-sided five years ago, that is to say, previous to the blood-poisoning and previous to his other injuries. This constituted very important evidence in favour of the view that the early symptoms of the disease preceded both trauma and infection. The fact that the man had no loss of pain in connexion with the injury to his hand could not be regarded as any great argument against the presence of syringomyelia. There might, in fact, have been some analgesia at that time, but there were degrees of analgesia, some of them very incomplete in the early stages of the disease. With regard to the view that syringomyelia might be the result of an infective process, he referred Dr. F. J. Smith to the writings of some French authors who supported the idea that this disease was sometimes due to an ascending infection along the spinal nerves. He himself did not think that the evidence in favour of this view was altogether satisfactory.

Dr. WILFRED HARRIS asked Dr. Fearnsides if he knew whether Dr. Henry Head ascribed the whole illness to the accident; whether the accident caused the lesion, or whether the trauma simply brought out latent symptoms as might occur in tabes and other organic diseases. It seemed to him that that might account for it. Did Dr. Fearnsides also think that in this case there was any hæmorrhagic lesion of the cord which might have brought out symptoms which were previously latent?

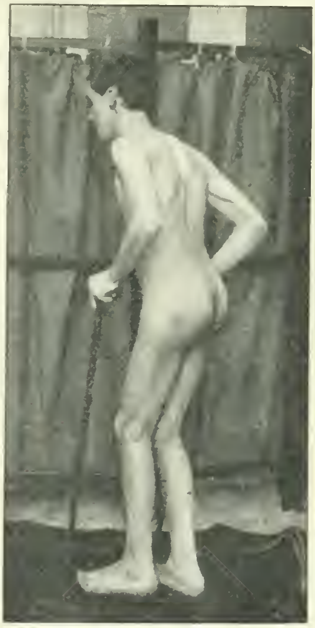
Dr. FEARNSIDES, in reply, said that he understood that Dr. Head's opinion was that the trauma was quite accidental. The probabilities were strongly in favour of that view. It was not hæmatomyelia, because the man had no severe headache, nor any vomiting at the onset, and further, the patient had been under observation for the past two months and the condition was certainly progressive. These facts, Dr. Fearnsides considered, were decidedly against the hæmorrhagic hypothesis. He thought that, seeing that the patient only came under observation so many months after the accident, the evidence required to answer Dr. Smith was in this case quite unobtainable, and that in consequence the point raised by him was academic rather than practical. The patient was under the scope of the Workmen's Compensation Act, and at the present time was receiving 17s. 3d. per week. His wages at the time of the accident in September, 1910, had been £1 per week. In answer to a question as to whether there was any known evidence that a hæmorrhage into a previously healthy spinal cord ever caused a lesion with symptoms progressive for a long time after the accident, he replied that in the course of an extensive search through the literature he had been unable to find any such evidence.

Case of Spondylose Rhizomélique.

By F. E. BATTEN, M.D.

H. D., AGED 25. His illness began three years ago with pains in the feet and he became flat-footed. He is the eighth of a family of nine. One brother has died of phthisis. He denies infection by gonorrhœa or syphilis. Two and a half years ago the thighs began to waste and they have steadily got thinner. He then had pain in the back. He had increasing difficulty in walking owing to the pain and stiffness of the legs, and this so much increased that six months ago he could only walk with the help of two sticks. He has difficulty in lying down in bed owing to the rigidity of the back. He is a thin, delicate-looking man. He can stand with the trunk bent forward, the back rigid, and a slight antero-posterior curve in the mid-dorsal region. He has practically no movement in the spine below the neck, but he can flex and rotate the head fairly well. The thorax is flat, moves very little with respiration, and there is rigidity of the thoracic and abdominal muscles. The hips are fixed at about an angle of 120° to the spine, adducted and any attempts at passive movement give rise to pain. The movements of the jaw, elbow, wrist, finger-, knee-, ankle- and toe-joints are fairly free; it is the spine and proximal joints which are affected. All the deep reflexes are very active. There is no affection of sensation.

In March of this year he had an acute inflammatory condition of the left eye, which was said by Mr. Leslie Paton to be a superficial keratitis. The Wassermann reaction is negative. A skiagram shows no marked change. The condition above described corresponds with those cases described by Marie under the title "Spondylose rhizomélique": a good digest of the whole subject is given by Léri.[1]



Spondylose rhizomélique.

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[1] *Rev. of Neurol. and Psych.*, 1908, vi, p. 12.

Case of Spondylose Rhizomélique.

By E. Farquhar Buzzard, M.D.

S. S., a fitter, aged 34, came under observation in March, 1911, complaining of pains around the lower part of the thorax and upper part of the abdomen during the previous eighteen months. The pains are generally worse at night and not so noticeable when getting about, but he is unable to work because any slight slip or jar causes a sensation as if his back was breaking. Has had enteric fever, ten years ago, and sciatica and synovitis of the knee-joint about five or six years ago. No history of any venereal disease.

On examination he shows complete immobility of the spinal column below the upper dorsal level with diminution of the normal lumbar curve. No bony deformities can be detected either on palpation or by means of the X-rays. There is no affection of any of the joints in any other part of the body or limbs.

Spondylitis, with Progressive Muscular Contracture.

By Wilfred Harris, M.D.

W. A., aged 33, a soldier for ten years. Left the Army in 1908 owing to pains in the shoulders and the whole of his back, lasting for two months. Was thought to have pulmonary tuberculosis. Went to Frimley Sanatorium for ten months. From the time of the commencement of the pains his neck and shoulders became gradually stiffened; this has gradually increased up to the present time. Has never suffered from any venereal disease.

Present state: Although the pains have disappeared he feels weak and helpless. The spine is curved antero-posteriorly and quite rigid, with the exception of very slight lateral movements of the head. The thorax is quite fixed, no movement of ribs or chest whatever in respiration. The scapulæ are fixed to the chest wall by muscular contracture, and the arms cannot be raised above the horizontal owing to strong contracture of the pectoral muscles (figs. 1 and 2). No affection of shoulder- or hip-joints. Lower extremities and pelvis normal. Under chloroform anæsthesia the scapulæ were moved fairly freely.

Owing to the kyphosis and fixation of the head forwards the aspect somewhat resembles Parkinson's disease, but the features are mobile. Skiagrams show no abnormality of cervical or dorsal spine, or of thorax.

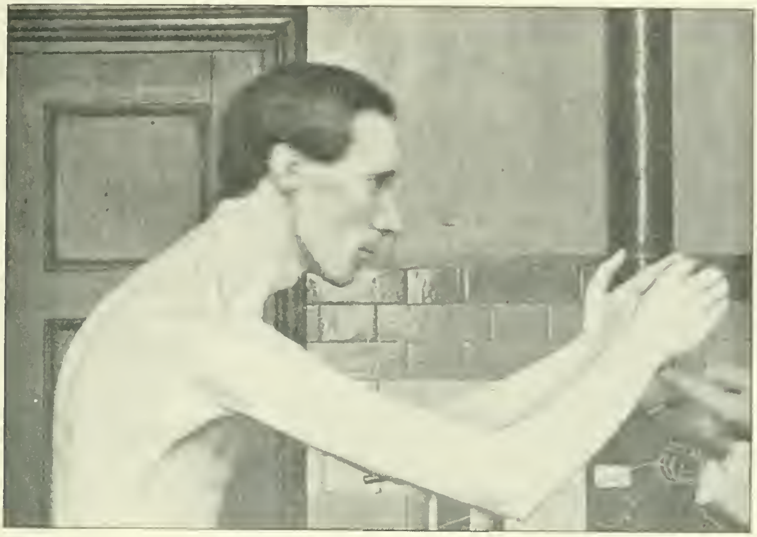


FIG. 1.

Showing fixed spine and arms raised as high as possible.

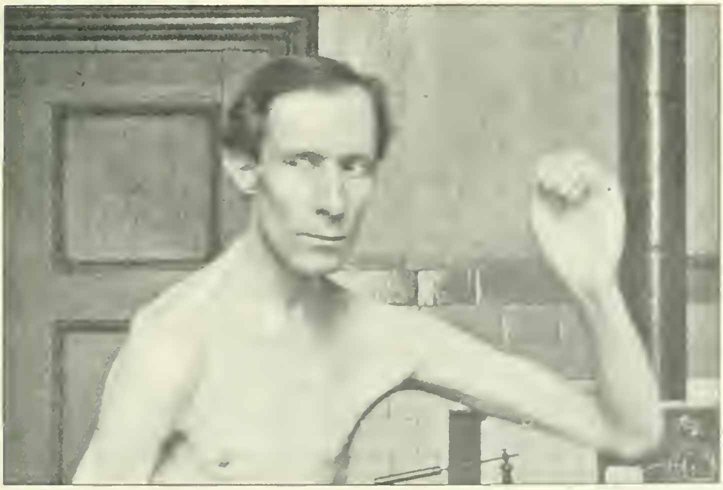


FIG. 2.

Showing limit of turning power of head to the right.

DISCUSSION.

Dr. F. E. BATTEN said he wished to add a reference as to the view which Marie took with regard to cases of spondylose rhizomélique. Marie stated that the subjects were generally young, either adolescent or in the first part of adult life, up to the age of 35 to 40 years. The cases occurred nearly always in men. In almost all cases Marie had found evidence of some infection, of which the existing ankylosing symptoms would appear to be the consequence. This infection was frequently tuberculosis, but still more commonly gonorrhœa; sometimes it was one of a variety of other infections. It was the spine and the proximal joints which were first affected, and in the later stages the knee and other joints might also become affected. The foregoing was the clinical picture of the disease as described by Marie, and it was a question whether the three cases shown that evening would fit in with that description. The clinical features of these three cases differed considerably. For instance, in Dr. Harris's case the scapulæ were fixed, while in his (Dr. Batten's) case they were perfectly free. In Dr. Harris's case the hips were completely free, in his own case they were fixed; whilst in Dr. Buzzard's case the spine alone seemed to be affected. In all three cases the spine was the characteristic feature—they were all fixed and rigid.

Dr. BUZZARD said he would like to hear the experience or opinion of any member with regard to the question of treating these cases. He had been giving his patient salicylates and iodides for some weeks, and had been successful in alleviating his pain to a very great extent. According to the patient he was not as stiff as he had been before treatment was begun, but it was difficult to detect any great difference in the mobility of the spinal column. What further measures would be likely to prove beneficial, and is the application of heat of any value in such cases?

Dr. WILFRED HARRIS said that he should have included in his report that his patient still had pains constantly if he did not take aspirin. He was taking 10 gr. every day, and when it was dropped the pain began to return. Dr. Harris then quoted from Dr. Léri's paper as to treatment, and said he thought that extension might be tried in his case. This patient's head was fixed forward, and he would have liked to have been able to try the treatment referred to, but the patient had stayed in the ward only one day; he had, therefore, no opportunity to do so.

Dr. WARRINGTON referred to Léri, who said that if the treatment were to be of any benefit it must be utilized in the very earliest stages, when the bones were quite soft. He had had an opportunity of seeing a few of those cases, and, in fact, he then had a very typical case under his observation. A little while ago he had come across two in the same family. That had interested him because the condition had been said not to be a family one, differing in that respect from the condition of the spine called hereditary traumatic kyphosis. He first became acquainted with the brother who was a sailor, aged 24. Dr. Warrington was assured that he had been perfectly well until he had fallen out of a bunk one night at sea. He did not then sustain any severe injuries, but rapidly became stiff in the back within a few days. Only two or three months elapsed before he arrived back in Liverpool, and then he had marked ankylosis of the shoulder-joints and cervical spine. He had subjected that man to all kinds of treatment, without any effect; finally, he had given him an anæsthetic and tried to move his neck. That made him worse. The man then informed him that his sister was similarly affected. Constitutionally she was a perfectly healthy young woman, with characteristic joint changes, very marked ankylosis of the hips, marked ankylosis of the shoulder-joints, and rigid spine. That family case had interested him a good deal, and the curious history had apparently started from injury in the first case.

Dr. BUZZARD asked whether the sister had had an injury too, to which Dr. Warrington replied in the negative.

The PRESIDENT said he was afraid they knew very little about the pathology of those cases, or about the treatment.

Case of slowly progressive Right Hemiplegia suggesting Tumour (Operation—no Tumour found).

By F. E. BATTEN, M.D.

A. E., AGED 52, married. Twelve years ago attack of diplopia, from which she rapidly recovered. Five years ago the right leg gradually began to give way. Three years ago the right hand became weak. For many years she has suffered from headache, which has become worse lately; she has not vomited; she has had no aphasia and no fits. For two years she has had precipitant micturition. The weakness has steadily increased. She is a well-nourished woman, with grey hair; she walks with help, drags and circumducts the right leg, keeping it very stiff at the knee. There is slight inco-ordination in movements of the right hand, and it is slightly weaker than the left; the right leg is very rigid; the right knee-jerk is greater than the left; ankle-clonus is present on the right; the right plantar is extensor, the left flexor; abdominal reflexes absent. There is no optic neuritis, no aphasia, no weakness of the face, no sensory loss. Wassermann reaction negative.

Mr. Percy Sargent made a bone flap and exposed the surface of the anterior portion of the left hemisphere. No tumour was found; the membranes appeared normal; the cortex was a little shrunken, and the vessels small. There was no increase of intracranial pressure. The patient made a good recovery from the operation, but the paralysis was neither increased nor lessened as the result of the operation.

Dr. STANLEY BARNES said he was able to sympathize with Dr. Batten. Nowadays cases of progressive hemiplegia with headache should, he thought, be explored in the hope of finding a cerebral tumour before the development of optic neuritis, as if these cases were left until the latter symptoms had developed not only was there danger of permanent blindness coming on, but also the operation was more difficult, and if successful led to greater disablement on account of its more extensive nature. Like Dr. Batten, he had had a patient trephined for probable tumour, in which the diagnosis had been wrong. The patient was a young man, aged 24, with progressive left hemiplegia, moderately severe headache, and occasional fits, which were never seen but which were judged, from the description given, to be of a Jacksonian type. The condition had lasted for over twelve months and was becoming progressively worse, but there was no optic neuritis. At the operation it was obvious that there was no material increase in the intracranial pressure, and the wound was closed without replacing the bone. The rate of progress of the condition had not been interfered with by the operation, but other symptoms had developed during the two years which had elapsed since the operation; the patient now presented a fairly typical picture of disseminated sclerosis. Dr. Barnes still thought that with those two symptoms he was not to blame in having recommended that the operation be performed.

Case of Pseudobulbar Paralysis of doubtful origin, probably Disseminated Sclerosis.

By T. GRAINGER STEWART, M.B.

E. L., AGED 24, married. Family history: Eight brothers and sisters alive and well. Married six years; four healthy children, no miscarriages. Past health: Good.

Present illness: Three years ago gradual onset, without fits, of weakness in right arm and leg; no aphasia; in three weeks was unable to walk or raise right hand to mouth; recovered completely under treatment in seven months and has been bright and well since. Six months ago—gradual onset of weakness and stiffness in both legs; in three weeks unable to walk; both arms were slightly weak; no aphasia or sphincter trouble; at the end of seven weeks' treatment she again became apparently well, but became tired easily. Fourteen days before admission woke up suddenly with "peculiar feeling" in head: began to tremble all over; two hours later suddenly had a fit, turned head and eyes to left, extended left arm rigidly, making stiff, slow, flexor movement of the clenched hand upon the forearm; extended left leg rigidly; no clonic movement; patient unconscious and incontinent; duration ten minutes; during the day six more similar attacks; she was very drowsy for the rest of the day. Since then she has appeared sensible, has been very weak in all limbs, and unable to speak except in an indistinct whisper. Throughout no headache or vomiting. Vision unaffected.

Condition, January, 1911: Attention poor; cerebration usually slow; optic disks normal; no hemianopsia; special senses good. Cranial nerves: iii, iv, vi—pupils react slightly less well to light than to accommodation; ocular movements good, no nystagmus; vii—bilateral spasticity of the face, right weaker than the left, emotional movement good; viii, x, xii—speaks only in a whisper, articulation very poor, scarcely any movement of tongue, which shows no wasting. Motor: No local wasting; tone everywhere increased, but much more on the left side; movements in right arm of good range and power; movements in left arm of poor range and power; much difficulty in executing the finer movements with left hand. Sensory: No anæsthesia or analgesia; no loss of muscle-pain sense; no loss of sense of passive position; some loss of sense of active muscular contraction in left hand and arm. Reflexes: Jaw—very brisk; upper extremities—right well marked, left exaggerated; epigastric—absent; abdominal—absent; knee-jerks—right brisk, left very brisk; rectus clonus on left side only; ankle-jerk—right weak, left very weak; ankle clonus—present right and left; plantars—left always typical extensor, right usually very doubtful, occasional extensor. Sphincters not affected. Gait: Can walk with assistance; drags both feet, the left much more than the right; when standing, tends always to fall straight backwards; makes scarcely any attempt to recover balance by bringing the feet under the centre of gravity. Cerebrospinal fluid colourless; came away in drops under normal pressure; no lymphocytosis. Wassermann reaction negative in blood and cerebrospinal fluid.

Present state: On March 20 the patient had completely recovered from all motor weakness. Speech and articulation were normal, and gait quite natural. The only physical signs present were absence of the abdominal and epigastric reflexes, and the presence of extensor plantar responses. The patient was treated with increasing doses of arsenic.

Dr. GRAINGER STEWART added that he had shown the case in January last when the patient's condition was much worse. At that time she could not speak or protrude her tongue and she was quite unable to stand, always tending to fall backwards. There was one interesting point about the case which he had not mentioned, and that was the fact that all the attacks had come on about within six to eight weeks after the birth of a child. The case was a little like Dr. Batten's case, in that the first attack was a slowly progressive right hemiplegia. In reply to Dr. Warrington, Dr. Stewart said that the patient had recovered almost completely from the original condition. Answering Dr. Barnes, Dr. Stewart said that the extensor responses had not disappeared in this case, but that he had seen them return to normal in other cases of disseminated sclerosis.

Case of Gastric Crisis in ? Tabes. Division of Posterior Roots.

By C. M. HINDS HOWELL, M.D.

A. R., AGED 39, was admitted to the Great Northern Central Hospital in August, 1909. Family history: *Nil.* Past history: Syphilis fourteen years ago, treated two years; health otherwise good.

Present illness: Eighteen months ago had attack of abdominal pain, followed by vomiting. Pain always at umbilicus, commencing at 7.30 a.m.; vomiting about two hours later, which relieved pain somewhat, continuing till about 9 p.m. First attack lasted three to four days; attacks recurred about every two months; latterly has had attacks every week. Occasional shooting pains in legs, with hyperæsthesia of skin of trunk. Sphincters: Occasional difficulty in passing water. Has complained at times of diplopia.

Condition, August, 1909: Intelligent man, fairly well nourished; no thoracic or abdominal disease detected. Central nervous system: special senses *nil.* (ii) Vision: Myopic, disks normal, 3. 4. 6. Left pupil a little larger than right, both small. Right, Argyll-Robertson; left reacts very little to bright sunlight, brisk on convergence. Cranial nerves otherwise normal. Motor: No paresis in arms or legs; occasional slight inco-ordination in finer movements of upper extremities; no ataxy in legs; no Rombergism; gait natural. Reflexes: All deep reflexes very brisk, abdominal and epigastric present right and left; plantars flexor. Lumbar puncture: 200 cells per cubic millimetre; 94 per cent. lymphocytes; globulin present. Wassermann reaction positive. In hospital had very severe attacks of pain and vomiting, relieved only by morphia, which was given only two or three times. Treated by intramuscular injections of hyd. perchlor. Improved considerably; right pupil again reacted to light, and Wassermann reaction became indefinite.

Condition relapsed. Re-admitted to Great Northern Hospital, June, 1910. Mr. Warren Low operated and divided the posterior roots of D 8, 9, 10, within spinal canal; wound healed well. Improved condition for six weeks, then relapse again: pain referred, as before, to umbilicus in total cutaneous anæsthetic area. Pain relieved sometimes by injections of water.

April, 1911: Re-admitted. Has been suffering intensely again; attacks at very short intervals. Injection of "606" given intravenously; no change in condition four weeks after injection.

DISCUSSION.

Dr. HOWELL added that he thought it must be regarded as a case of tabes with gastric crises, although many of the characteristic features of tabes were absent. All the deep reflexes were present. Except for the analgesia, which was widely distributed about the body, and the gastric crises, the patient showed no symptoms of tabes. When he first came under observation he had definite Argyll-Robertson pupil in the right eye. He was given intramuscular injections of perchloride of mercury—fifteen injections of $\frac{1}{10}$ gr. each. Following that treatment, the light reflex returned in the right eye, though not very actively. He had not seen another case in which the Argyll-Robertson pupil had become developed and the reaction to light had returned under treatment. At the operation the eighth, ninth, and tenth posterior roots were divided within the dura, those being selected because the crises always commenced with severe and violent pains which occurred at the umbilicus. The man used to make himself vomit, when he brought up bile and mucus. In all the reported cases Dr. Howell had traced in the literature in which the posterior roots were divided the pain had been much higher than the pain in this case. In one case, the seventh to ninth roots were divided, and in another the seventh to the tenth. Of the cases recorded only one was a failure, and he died from infection of the wound. The others recovered from their vomiting attacks, but still had a certain amount of pain, though not in the same region as before. He thought it remarkable that in this case the pain was still referred to an area which had been rendered completely anæsthetic by division of the posterior roots. About a month after operation the man had an area of total sensory loss on either side in the form of a band round the abdomen. Outside this area—i.e., above and below—was an area in which tactile sensation remained, but in which there was loss to painful and thermal stimuli. At the present time the limits of anæsthesia and analgesia agreed exactly and there was no overlap. In the cases reported in Germany the area of tactile loss had exceeded that in which pain and thermal loss had been noted. He asked for suggestions as to treatment, as the attacks were very bad, and morphia was the only thing which stopped the pain.

Dr. F. E. BATTEN said he would like to know what the posterior roots looked like at the time of the operation, and what was their appearance on pathological examination. He supposed that when the surgeon divided the roots he took out a portion for examination.

Dr. HOWELL, in reply, regretted that no portion of the roots was preserved for examination. Mr. Low said the roots were fairly normal. In reply to Dr. Harris, he did not consider that the stomach was supplied by the tenth dorsal root, but that root had been divided because the pain was always referred to the umbilicus and the gastric symptoms—viz., vomiting—only seemed to occur secondarily to the abdominal pain. The anterior roots were not divided.

Juvenile Tabes.

By WILFRED HARRIS, M.D.

(For Dr. GUTHRIE.)

R. M., AGED 22. About four months ago patient suddenly got diplopia, and has had it ever since. Four weeks before admission left-sided ptosis suddenly appeared, and this also has persisted. She has had pain in the small of the back for the last three months, and also shooting pains down the thighs. She has had no sickness, and only an occasional slight headache.

Previous history: Always good health. Had amenorrhœa for a year, being anæmic at the time.

Family history: Two brothers died of phthisis.

On examination (February 2, 1911): Patient is a well-developed, robust girl. Heart, lungs and urine normal. Temperature, pulse and respiration normal. Nervous: There is marked left-sided ptosis, patient being able to raise the lid only very slightly. The movements of the right eye are normal, and of the left normal, except upwards, inwards, and downwards, and then there is some slight nystagmus; the left internal rectus is completely paralysed; the movements up and down are incomplete; the pupils are large, left slightly, > right; there is no reaction to light on either side, but they both react well to accommodation, and are spastic in their reaction. Other special senses are normal. Motor: Face and mouth normal; legs and arms normal; there is no sign of motor weakness, excepting palsy of the left third nerve. Sensory: There is marked disturbance of sensation. Pain is chiefly affected, a large part of the body being analgesic. Temperature is also largely affected, while tactile sensation is diminished and retarded in most of the areas where there is analgesia. Patient complains of shooting pains down thighs, in arms and round waist. Sphincters are normal. Co-ordination: Arms normal; there is some slight Rombergism. Reflexes: Knee-jerks normal, plantars are flexor, supinators and superficial abdominals normal. Tremor: There is some fine tremor of the hands. Optic disks normal. Trophic disturbance absent.

Dr. HARRIS said there was no evidence of other syphilitic lesions, as was, indeed, usual in any tabes. In answer to Dr. Batten, his experience was that such cases rarely showed evidence of syphilis except, perhaps, smallness of stature.

Case of Unilateral Paralysis of Ninth, Tenth, and Eleventh Cranial Nerves.

By P. W. SAUNDERS, M.B.

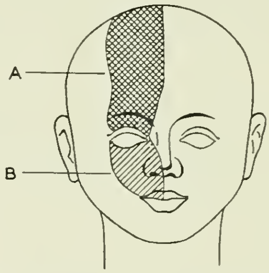
(For Dr. JAMES TAYLOR.)

J. H., AGED 42, married, complains that for nine months she has had weakness of the right arm, and difficulty in swallowing and in speaking. These have become much more marked during the last three months. She has lost weight lately. On examination there is found considerable paralysis of the right palate, abductor paralysis of the right vocal cord, extreme wasting of the right sterno-mastoid and of the upper part of the trapezius, with slight wasting of the lower part, slight wasting with fibrillary tremor of the right side of the tongue which is protruded to the right. Speech is nasal and hoarse. Apart from these the results of examination are negative.

Case of Tic Douloureux illustrating the Anæsthesia produced by Alcohol Injection of the Superior Maxillary and Supra-orbital Nerves.

By WILFRED HARRIS, M.D.

M. B., a married woman, aged 43, had suffered from attacks of severe neuralgia during the past six years, at first in the forehead and top of head on the right side, and subsequently also in the cheek, nose and upper gum. At first the neuralgia attacks lasted several weeks only, with intervals of many months' complete freedom from pain. The present attack has lasted since last December, the paroxysms of pain occurring every few minutes.



A, area of loss of sensation on scalp, resulting from injection of supra-orbital nerve. *B*, area of loss of sensation on cheek, nose and upper lip, resulting from injection of superior maxillary nerve.

A week ago Dr. Harris injected the right supra-orbital nerve with 10 ♏ of 90 per cent. alcohol, preceded by a few drops of 2 per cent. eucaine solution in order to render the injection comparatively painless. This abolished the pain above the eyebrow, but the pains in the cheek still continued, with diminished intensity. Dr. Harris then two days later, injected the superior maxillary nerve at its exit from the foramen rotundum, with eucaine and 90 per cent. alcohol, and since then the pains have entirely ceased.

The resulting areas of anæsthesia to touch, pin-prick, and also to moderate pressure, are indicated in the diagram.

Prognosis: The anæsthesia fades away gradually during twelve months to two years, the freedom from pain lasting as long or sometimes longer.

Neurological Section.

January 19, 1911.

Dr. J. A. Ormerod, President of the Section, in the Chair.

Progressive Spinal Muscular Atrophy of Infants and Young Children.

By Frederick E. Batten, M.D.

Introduction.

The occurrence of progressive paralysis and muscular atrophy due to a lesion of the lower motor neuron is a condition of some rarity both in infants and young children. During the past ten years I have seen some eight cases which may be placed under such a heading. Some of these have been under my own care, others under the care of my colleagues, but I have made the pathological examination in all cases in which an autopsy has been obtainable. Some of these cases have already been published. It is from a consideration of these cases that I propose to deal with the somewhat difficult problem of progressive spinal muscular atrophy of infants and young children. It will be well in the first place to mention certain classes of cases, which though they may show progressive muscular atrophy dependent or independent of spinal disease, it is not intended to deal with in this paper. Into this category are placed: (1) The primary muscular atrophies (myopathies), and with these I include the condition known as "myatonia congenita"; (2) the effects of acute poliomyelitis; (3) spinal or spinal root injury; (4) congenital defects of the cord; (5) the peroneal type of muscular atrophy (Charcot-Marie-Tooth); (6) von Recklinghausen's disease; (7) the effects of spinal caries and spinal growth.

The eight cases on which this paper is based may be arranged in three classes; each class will be dealt with in a separate chapter. A short statement of the clinical and pathological features of the cases belonging to each class will be found at the beginning of the chapter; but for a full clinical report and details of pathological examination reference should be made to the appendix for those cases not already published, and to the article referred to for those which have been published.

Of the eight cases *six* belong to the first class, *one* to the second class, and *one* to the third class. The characters of these three classes may be stated as follows:—

(1) A type of case in which progressive muscular weakness occurs during the first weeks or months of life, gradually progresses, and terminates in death after a variable period of weeks, months, or years. Sometimes more than one member of the family may be affected. Atrophy of the cells of the anterior horn is the constant pathological change found in such cases, but it may be associated with other changes in the central nervous system (Werdnig-Hoffmann type).

(2) A type of case in which progressive muscular weakness and atrophy begins somewhat later in life, after the child has already walked, and slowly progresses till death occurs from respiratory failure or pneumonia. The pathological changes found in such cases consist in a degeneration of the lower motor and sensory neurons resembling those occurring in a "toxic neuritis."

(3) A type of case in which progressive muscular weakness and atrophy begin in later life after the child has already walked, slowly progress, and are attended by very marked degeneration of the spinal cord, not limited to any one group of cells or tracts (a "myelitis").

CHAPTER I.—WERDNIG-HOFFMANN TYPE.

The cases which belong to this type are six in number, and the following is a short abstract of the clinical features and pathological changes in those cases in which a post-mortem was obtained:—

Case I.—C. H., aged 4 weeks. First child of healthy parents; no miscarriages. Said by the mother to be healthy at birth. Took breast well for fourteen days; then fed on goat's milk. Wasting and weakness of the limbs noted when 14 days old; got tired when sucking. The child never cried well. On admission to hospital, small child, 5⅛ lb., feeble cry. A complete flaccid paralysis of arms and legs, with slight movements of fingers and toes; neck

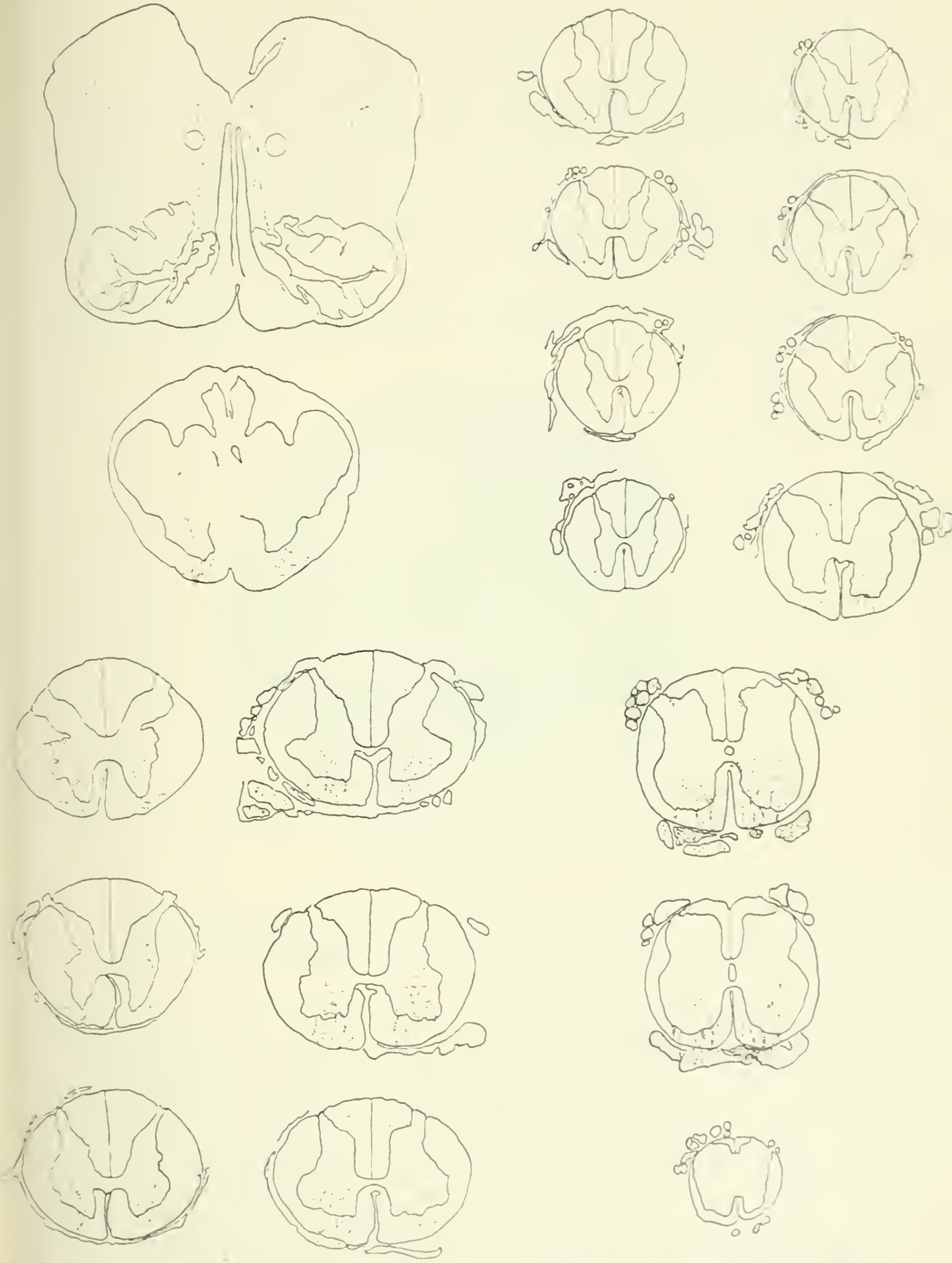


FIG. 1.—Series of section through the spinal cord, from Case I, C. H., stained by the Marchi method, showing the degeneration of the anterior root. The stress of the disease is seen to fall on the lumbar and the cervical region of the spinal cord, but some degeneration is present throughout the whole length of the cord and medulla.

muscles flaccid. Sucks well, no regurgitation; deep reflexes absent, sensation apparently normal, no reaction obtained in muscles by strong faradic current. Death after three days.

Post-mortem: Nothing abnormal to naked eye in visceral or nervous system. No evidence of birth injury. Microscopical examination: Marked degeneration of anterior nerve-roots throughout the cord; some chromatolysis of anterior horn cells: no evidence of muscular degeneration. (*See* figs. 1, 2, 3, 4, 5, 6.) (*See* Appendix.)

Case II.—W. G., aged 5 weeks. The eighth of eight children, three of whom have died of a similar complaint within the first few months of life. The labour was difficult. On admission to hospital the child was well nourished, looked healthy, and took food well. There was complete flaccid paralysis of all four limbs, except for slight movements of the fingers. There was complete paralysis of the trunk muscles and intercostals, but the diaphragm acted well. All the deep reflexes were absent; there was analgesia below the second rib. The child died when 8 weeks old of broncho-pneumonia.

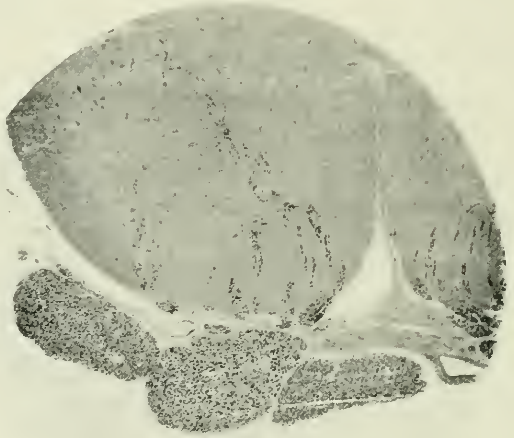


Fig. 2.

Photograph of the anterior region of the sacral cord from Case I, C. H., showing the degeneration of the anterior nerve-roots in their intramedullary course. Stained by the Marchi method.

Post-mortem: Nothing abnormal on macroscopical examination; but on microscopical examination widespread atrophy of the cells of the anterior horn, and degeneration of the fibres of the posterior column. (*See* figs. 7 and 8.) (See *Brain*, 1902, xxv, p. 85.)

Case III.—E. T., aged 4 months. Fourth of four children, one of whom was said to have died of a similar complaint. Easy labour. General helplessness noticed one week after birth; gradually increased. On admission, flaccid paralysis of arms and legs, absence of deep reflexes, paralysis of intercostals. Death. No post-mortem. (*See* Appendix.)

Case IV.—P. B., aged 6 months. Fourth of four children. Full term; normal labour. Healthy at birth. Gradual onset of weakness first noticed in the arms when 4 months old. Well nourished, intelligent child. Flaccid paralysis of arms, legs, trunk, neck. Paralysis of intercostals. Absent reflexes. Sensation apparently normal. Death. No post-mortem. (*See* Appendix.)

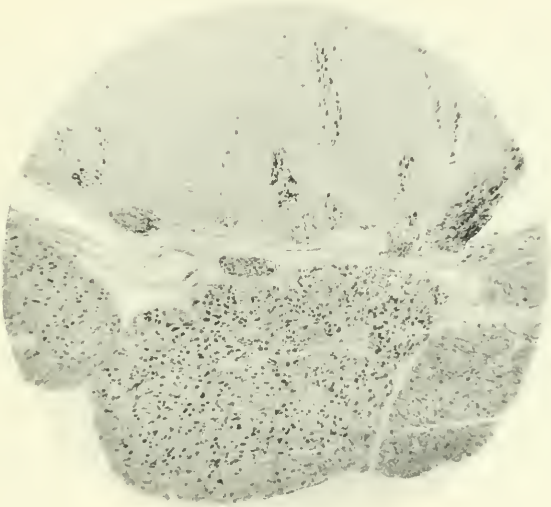


FIG. 3.

Photograph of anterior roots showing degeneration of the anterior roots in their extramedullary course. Stained by the Marchi method.

Case V.—H. C., aged 8 months. Third of three children, others healthy. Breast-fed. Normal labour. Always weak; never able to sit up or hold its head up. Weakness has increased. There was flaccid paralysis of both arms and both legs. Weakness of back muscles with lateral curvature of the spine. Knee-jerks only obtainable with difficulty. Death after four days in hospital.

Post-mortem: Nothing abnormal on macroscopical examination. On microscopical examination some degeneration of the anterior roots and some chromatolysis and atrophy of the cells of the anterior horn. (*See* Appendix.)

Case VI.—R. A., aged 3½, one of twins. Never learnt to sit up. Learnt to talk early. When 15 months old could not move his legs, but could move the arms. When first seen in June, 1910, he was quite intelligent, had flaccid paralysis of both legs, was quite unable to sit up or maintain a sitting position; he could, however, hold up his head. Sensation was perfect. The knee-jerks were absent. The intercostals paralysed. He died from broncho-pneumonia in September, 1910.

Post mortem: Broncho-pneumonia. Microscopical examination showed smallness of anterior nerve-roots of the spinal cord, and marked atrophy of the cells of the anterior horns. No Marchi degeneration. No vascular change. (*See* figs. 9 and 10.) (*See* Appendix.)

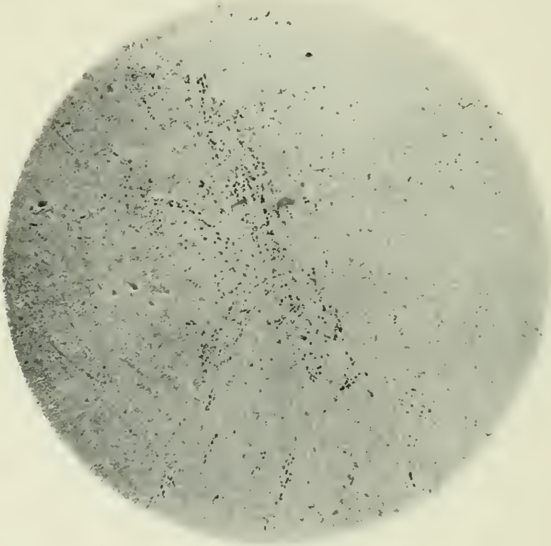


FIG. 4.

Photograph of grey matter, showing degeneration of fine fibres arising from cells of anterior horns. Stained by the Marchi method.

Clinical Features of Class I.

An infant born at full term and apparently normal at birth begins in the first few weeks of life to show weakness of the trunk and limbs. The infant appears of normal intelligence. The weakness steadily progresses, so that there is a complete flaccid paralysis of both arms and legs, and only a little movement is preserved in the fingers and toes. The intercostal muscles become paralysed and the child dies from some intercurrent disease, generally of broncho-pneumonia. Several members of a family may in succession be affected, as in the Case II, where four children out of a family of eight were thus affected. In Case III, another member of the family is said to have died from the same disease.

The clinical picture which these infants present is very striking: The infant lies in bed, happy and contented, taking his food well, but completely unable to move either arms or legs, the limbs being perfectly flaccid. The disease tends to affect the proximal muscles before the distal, so that all movements of the fingers and toes are still possible after power has been lost in the shoulders and hips. If an attempt is made to lift up the child the extreme flaccidity of the trunk muscles is easily recognized. The actual wasting of muscles in some cases is not obvious, for it is covered up by the subcutaneous fat. The child will, as a rule, rapidly respond by crying when pricked by a pin, and as far as can be judged, sensation is perfect. All the tendon reflexes are either absent or greatly diminished. In those cases in which the electrical reactions have been examined the absence or great diminution of excitability to the faradic current is a most striking feature. The duration of life in most of these cases is limited by months, but in one case of the present series it was considerably longer—viz., three and a half years.

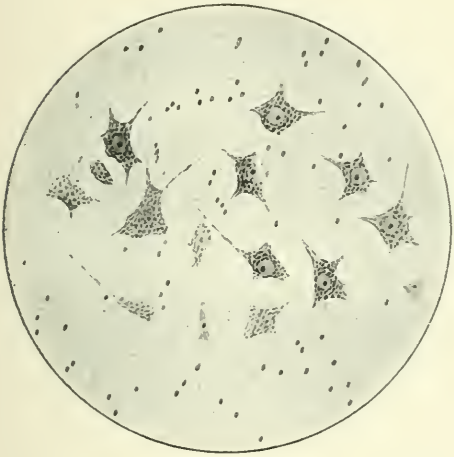


FIG. 5.

Cells of the anterior horns of cervical region, stained by the Nissl method, from Case I, C. H. Most of the cells appear normal; some, however, show slight chromatolysis.

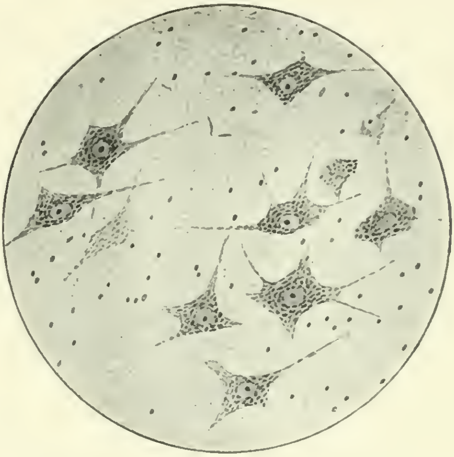


FIG. 6.

Cells of the anterior horns of the lumbar region, stained by the Nissl method, from Case I, C. H. The cells as a whole appear strikingly normal in appearance, considering the marked change seen by the Marchi method; some, however, show slight chromatolysis.

Pathological Features of Class I.

The clinical features of the disease are fairly constant. The pathological features differ somewhat. These differences may depend upon the length of time the disease exists before a fatal issue ensues; for that issue, although dependent in part on the progress of the disease, and especially on the paralysis of the intercostal muscles, is often accelerated by the occurrence of broncho-pneumonia.

In four of the six cases belonging to this class a pathological examination was made—viz., Cases I, II, V, VI.

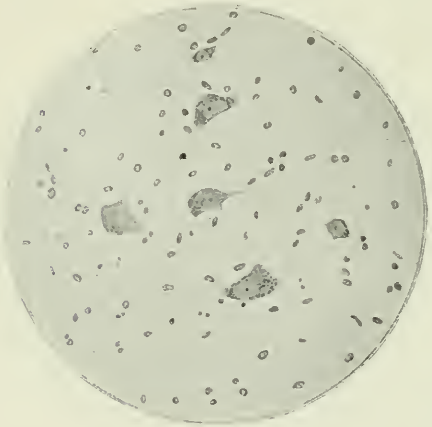


FIG. 7.

Cells from the anterior horns of the cervical region, stained by the Nissl method, from Case II, W. G., showing marked atrophy. Compare them with the normal-sized cells of fig. 5. (Drawn under same magnification, 170 diameters.)

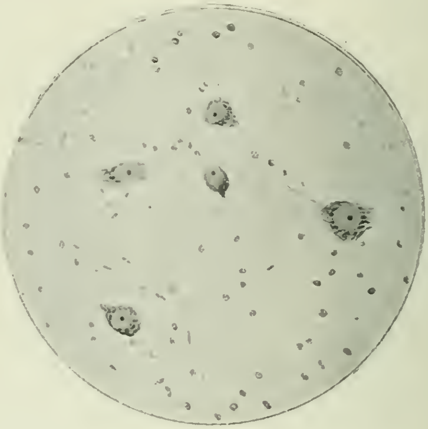


FIG. 8.

Cells from the anterior horns of lumbar region from Case II, showing same condition.

In Case I there is the most marked degeneration of the medullary sheath of the fibres of the anterior roots of the spinal cord from their origin in the cells of the anterior horns, in their intramedullary course through the cord, and in their extramedullary course (figs. 1, 2, 3, 4). This degeneration is most strikingly shown by the Marchi method. The peripheral nerves show but little change. The cells of the anterior horns show but little change, when considered in relation to the marked change in the fibres. Some few cells undoubtedly show chromatolysis (figs. 5 and 6), but only a few compared to the numbers of fibres which are affected. (Compare figs. 1, 2, 3, 4, 5, 6.)

The muscles in Case I do not show any marked change, the muscle-fibres being in the condition seen in a late fœtal or infantile life.

The changes met with in Case II are somewhat different. In this case there is a marked atrophy of the cells of the anterior horn throughout the cord (figs. 7 and 8), without the striking degeneration of the anterior roots as shown by the Marchi method. There is some degeneration of the posterior column of the spinal cord. The muscle shows a curious mixture of atrophy and hypertrophy of muscle-fibre, but no fibrosis.

The changes met with in Case V are similar to those in the two former cases, but differ in the following respects, viz., that the Marchi degeneration is less marked than in Case I, and more marked than in Case II, while the chromatolysis is more marked than in Case I and less marked than in Case II.

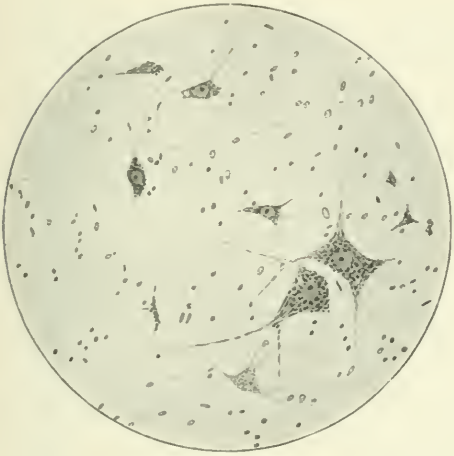


FIG. 9.

Cells from the anterior horns of the cervical region, stained by Nissl method, from Case VI, R. A., showing marked atrophy of cells, two normal cells alone remaining.

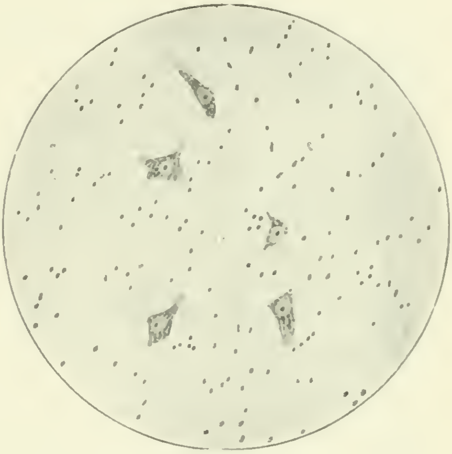


FIG. 10.

Cells from lumbar region of Case VI, showing similar changes. (Magnification 170 diameters.)

The changes met with in Case VI are similar to those seen in Cases II and V—viz., the marked atrophy of the cells of the anterior horns, and of the anterior roots, only a few normal cells persisting. (Figs. 9 and 10.)

If the pathological findings in these four cases are carefully considered together, may not the differences be explained partly by the intensity of the poison, and partly by the duration of the disease? In Case I the appearance is very similar to that met with in diphtheritic paralysis, the most marked changes being visible in the medullated sheath of the nerve, whilst the cells exhibit but little chromatolysis. At a later stage the products of degeneration shown by the Marchi method become absorbed, whilst the chromatolysis and atrophy of the cells as shown by the Nissl method become more marked. Such a condition is well exemplified in Case V, where there is a condition both of chromatolysis and atrophy of the cells, with some degeneration of the myelin sheath of the fibres, an appearance compatible with a more chronic course, dependent on a lesser dose of the toxin. The longer clinical history of this case corresponds with such a result.

In Case VI, a case of three years' duration, the cells and roots have undergone atrophy, and the pathological picture represents the later stages of the disease.

The question as to the nature of the toxin to which these changes are due may be raised, but on that point there is yet no evidence. Certain poisons are known to act especially on the lower motor neurons, and as an instance of such, diphtheria, lead and arsenic may be quoted.

The changes found in Case I resemble closely those found in diphtheritic paralysis, and it might be suggested that the mother had diphtheria before the birth of the child, and the resulting degeneration was due to that poison. Lead and arsenic might similarly be thought of, but the only object of such hypotheses is that in investigating future cases, examination might be made for these poisons. On the other hand, the occurrence of the disease in families, although not absolutely excluding the toxin theory, yet renders such a view improbable, and would seem to point to the condition being due to what Sir William Gowers has termed an "abiotrophy," that is, a "degeneration dependent on a defective vital endurance."

Recently Catola has examined the nervous system of infants born of parents suffering from various diseases. The changes which he figures and describes in the cells of the anterior horns and in the anterior roots in the child of an eclamptic mother and in that of a syphilitic mother are strikingly similar to those described in Case I of my series.

CHAPTER II.—TOXIC NEURITIS.

Abstract of Case.

Case VII.—H. M., aged 3. Weakness of limbs first noticed when 3 years old; the weakness gradually progressed, but there were considerable remissions, during which the child improved. Deep reflexes became abolished. Muscles of trunk, intercostals and diaphragm became involved. Death from bronchopneumonia fourteen months after onset.

Post-mortem: No macroscopic change in nervous system. On microscopical examination chromatolysis of the cells of the anterior horns and degeneration of the peripheral nerves. Fatty degeneration of muscles. (Figs. 10 and 11.)

Clinical Features of Class II.

The case which is placed in this class is one which was under the care of my colleague Dr. Voelcker, and was shown by him at the Medical Society, November, 1908, and February, 1909. I saw the child on various occasions. One of the most striking features of the case was, that although the disease was slowly progressive, yet there were periods during which improvement took place. The disease ran its course in a period of fourteen months.

The clinical features of this class as based on this case are as follows:—

An apparently healthy and intelligent child, who had made normal progress, without any obvious cause was noticed one morning to be unable to walk properly and to fall after taking a few steps. This weakness gradually progressed, so that the child became unable to walk and unable to feed herself. There was incontinence of urine and fæces for a time. There was a transient weakness of the muscles of the face. All the deep reflexes became abolished. The muscles showed the reaction of degeneration. The respiratory muscles became affected. Sensation was unaffected, and there was no pain.

Some improvement took place for a few months, then the disease steadily advanced and the child died some fourteen months after the first symptom.

Pathological Features of Class II.

The brain and spinal cord to the naked eye appears quite normal, and on microscopical examination no change could be shown by the Weigert-Pal method. Some slight degeneration is seen by the Marchi method, both in the white matter of the cord and in the anterior and posterior roots. Some of the cells of the anterior horn, both in the cervical, dorsal and lumbar region show chromatolysis, others have already undergone atrophy. (Figs. 11 and 12.)

The peripheral nerves show a considerable amount of degeneration by the Marchi method. The changes appear to be more marked in the peripheral nerves than in the central nervous system.

The muscles and fibres of the diaphragm show a marked fatty degeneration by the Marchi method.

The changes found are probably the result of some toxin affecting the lower neurons ("toxic neuritis").

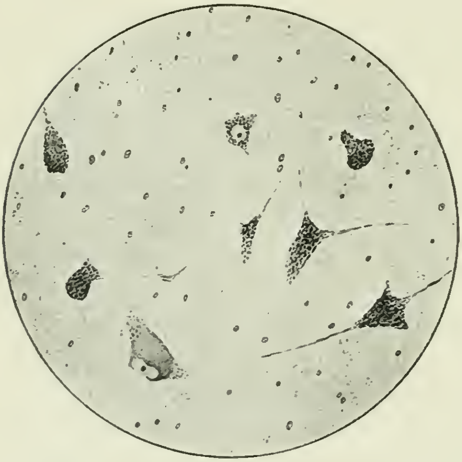


FIG. 11.

Cell from anterior horn of the cervical region from Case VII. H. M., showing marked chromatolytic changes. Stained by Nissl method. (Magnified 170 diameters.)

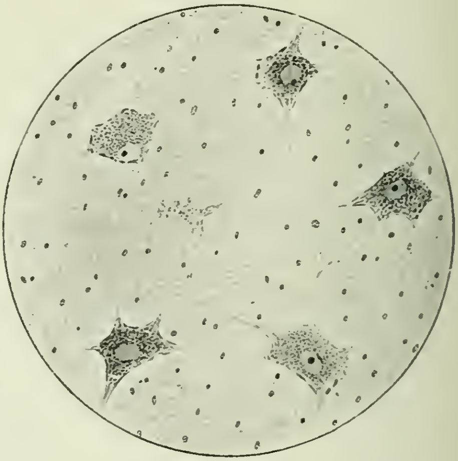


FIG. 12.

Cells from anterior horn of lumbar region, from Case VII, H. M., showing marked chromatolytic changes similar to those in the cervical region. Stained by Nissl method. (Magnified 170 diameters).

This case has been classed as a "toxic neuritis," partly on clinical grounds, partly on pathological grounds. The clinical history of the case is distinct from that of the cases which form Class I. The onset of the disease, the irregular course, the reaction of degeneration, the affections of the diaphragm, are all symptoms to which, if taken singly, no special value in the differential diagnosis could be attached, but if taken together suggest a poison acting diffusely in the lower neurons, rather than a poison which is acting primarily on the cells of the anterior horn.

On pathological grounds the extensive degeneration of the peripheral nerves as compared to the slighter changes in the cells of the anterior horns, together with the character of those changes, seem to me to be points in favour of regarding this case as one belonging to the class of "toxic neuritis."

It is admittedly one of the most difficult problems in neurology to differentiate between cases of "toxic neuritis" and those due to degeneration of the cells of the anterior horn.

On clinical grounds there is no doubt that the distinction is useful; on pathological grounds it seems to me doubtful if the distinction can be drawn between affections of various portions of the lower motor neuron, but for the present I would classify those cases which show rather diffuse changes in the peripheral nerves, and but little change in the anterior horn cells or anterior roots, as "toxic neuritis," and those cases which show marked changes in the anterior horn cells and anterior roots, and but little or scattered degeneration in the peripheral neurons, as "spinal atrophies."

The question as to the advisability of including such a case in a series of cases described as progressive spinal muscular atrophy might be raised. Few, I think, will be disposed to press this objection against the obvious advantages of considering together cases which have a marked clinical similarity.

CHAPTER III.—CHRONIC MYELITIS.

Abstract of Case.

Case VIII.—M. L., aged 11. Twelve months' weakness of hands, four months' weakness of legs. Gradual increase of weakness and muscular atrophy. Reaction of degeneration in some muscles. Sudden death from respiratory failure eighteen months after onset of illness.

Post-mortem: Extensive myelitis of both grey and white matter of the spinal cord, giving rise to atrophy of the cells of the anterior horns. (*Brain*, 1903, xxvi, p. 473.)

Clinical Features of Class III.

The case on which the third class is described is one which was published in *Brain*, 1903, by Dr. Morley Fletcher and myself. It is in its clinical and pathological manifestation entirely distinct from the other two groups, and yet it possesses the characteristic feature—viz., progressive muscular atrophy of spinal origin.

The clinical features are given in the following short abstract:—

A girl, aged 11. Twelve months' weakness of hands; seven months unable to button clothes; four months' weakness of legs. Two months ago adenoids removed and much worse since that operation. Fourteen days' pain in back and increased difficulty in swallowing. Intelligent, deaf, some atrophy of tongue, palate moves well on phonation, weakness of neck, respiration thoracic, upper intercostals only in use. Claw-hand, wrist-drop, weakness of upper arm and shoulder muscles. Can walk but feebly; knee-jerks brisk, plantars flexor. Sensation natural to all forms. Electrical reaction shows R.D. in some muscles, in others only diminution of excitability.

Sudden death from respiratory failure six months later.

Pathological Features of Class III.

There is extensive destruction and atrophy of the cells of the anterior horn from the upper cervical to the lower thoracic segments, these changes being most marked at the level of the sixth thoracic segment; the cells of Clarke's column being also affected. There is extensive degeneration of the whole of the ventral region of the cord in the thoracic segments, the lesion being most marked at the level of the sixth thoracic segment, and diminishing in extent both towards the medulla and caudally. There is extensive degeneration of the direct cerebellar tracts, both dorsal and ventral, beginning in the lower thoracic segments and extending up to their respective termination in the cerebellum. The degeneration affects both grey and white matters alike.

There is a marked increase of the connective tissue in the ventral portion of the cord. The walls of the vessels show no change. The cords show both recent and old degeneration. (*Brain*, 1903, xxvi, p. 473.)

This case must be regarded as a chronic form of diffuse myelitis, secondary to some vascular changes and toxic blood state. The origin and cause of the myelitis and of the peculiar distribution is unexplained.

Chapter IV.—Diagnosis.

The differential diagnosis in the first class of cases above described is one of the greatest difficulties. A child who at the *time of birth* has a complete flaccid paralysis of all four extremities is probably the subject of a spinal hæmorrhage, due to traumatism at birth. The child who during the first few weeks or months of life *develops* a flaccid paralysis of all four extremities is probably the subject of a spinal atrophy; but the difficulty of differentiating these two conditions is well shown in the two cases recorded by Dr. Beevor. Two children, when a few weeks old, exhibited the same clinical picture, viz., that of a complete flaccid paralysis, and yet on pathological examination it was found that in the one case the condition was due to spinal hæmorrhage, in the other to an atrophy of the anterior horn cells. In children rather more advanced in age other questions come up for consideration, and one of the most difficult is the distinction of a primary spinal condition from a primary muscular condition or myopathy.

The forms of myopathy which occur in young children become year by year more generally recognized. The "simple atrophic" type of Erb and the condition known under the name "myatonia congenita" may easily be mistaken for the Werdnig-Hoffmann paralysis. Myatonia congenita is a disease of early childhood, characterized by an extreme degree of loss of tone and feebleness in all the muscles of the body. The condition is stated to improve as the child advances in age.

A child with this disease can, as a rule, perform all movements in a feeble manner, and there is not the marked degree of flaccid *paralysis* seen in the case of spinal atrophy. Although hypotonia is present in cases of spinal atrophy, yet it is not so marked as in cases of myatonia congenita. Again, the intercostal muscles are commonly paralysed in the spinal atrophy of infants, whereas in myatonia they are less affected.

Shortly, it may be said that in spinal atrophy the paralysis is more marked and the hypotonia less marked than in cases of myatonia congenita.

The difficulty of diagnosis is well exemplified by the three cases in one family described by Finkelnburg. These would, on clinical grounds alone, be assigned to the Werdnig-Hoffmann group, and yet on pathological examination he has shown that they belong to the myopathic group—there being no change in the spinal cord, and changes in the muscles typical of myopathy.

Spinal caries may, under certain circumstances, rather closely simulate this disease, especially when the tuberculous process has spread in through the membranes and invaded the spinal cord. Cases of widespread poliomyelitis occurring during infant life may clinically resemble Werdnig-Hoffmann paralysis, but the paralysis and atrophy of muscles is rarely symmetrical, and groups of muscles tend to be picked out, others being left unaffected.

In the second and third class the diagnosis is no more easy than in the first group, and even with the pathological knowledge obtained from the examination of these cases, it would be most difficult to assert in cases presenting similar symptoms what was the cause of the widespread progressive muscular atrophy.

CHAPTER V.—LITERATURE.

Since the year 1898 there are only some five cases of the disease recorded in which there has been a pathological examination. These are the cases recorded, one by Hoffmann, one by Beevor, two by von Ritter, and one by Armand-Delille and Boudet. Previous to that date there are the well-known cases of Werdnig and Hoffmann on which the clinical features and pathology of the disease are founded, to which may be added the case recorded by Thomson and Bruce—i.e., ten cases; to these may be added the three cases in this paper, making thirteen cases in all.

Of cases without autopsy there are those recorded by Senator, Sevestre, Bruns, Wimmer, Haushalter, Lange. Some of these, and notably those of Bruns, Senator, Haushalter, and Lange, and the second case of Wimmers, should almost certainly be classified as myopathies and not with the Werdnig-Hoffmann group. The earlier cases of Werdnig and Hoffmann have already been reviewed in *Brain*, 1897. The later case of Hoffmann and those of Beevor, von Ritter, Armand-Delille and Boudet will be considered.

Hoffmann in 1900 recorded the result of the autopsy on the child M. G., the details of whose case had already been published in 1897. The paralysis slowly progressed, only feeble movements of the hands and feet remaining till the end. The face, tongue, muscles of deglutition, remained unaffected. The higher senses were normal, and there was no affection of the bladder or rectum. Sensation remained intact. The child died when 5 years old of pneumonia. On examination of the spinal cord there was found to be atrophy of the intra- and extra-medullary portions of the anterior roots, degeneration and wasting of the multipolar ganglion cells, and degeneration of the peripheral nerves. The pyramidal tracts showed no change. There was wasting of the muscle-fibres with fatty degeneration and interstitial fat.

Beevor's case, published in 1902, is recorded in the Appendix, and is one of those included in the substance of the paper. Shortly, the leading features are as follows: An infant, the eighth of a family of eight, four of whom had been similarly affected, had flaccid paralysis of all four limbs. The child died at 2 months old, and extensive atrophy of the cells of the anterior horns was found.

Von Ritter, in 1904, described two cases. The first, a female child, 15 months old at the time of death, the only child of healthy parents, born in normal labour, and suckled till 10 months old. When 5 months old it was noticed that the child moved its legs but little, and she seemed to get weaker in the upper extremities. The child was fat, not rickety, and had a flaccid paralysis of all its limbs. The face, mental condition, sight, and hearing were quite normal. On examination, the spinal cord showed marked atrophy of the cells of the anterior horns and atrophy of the muscle-fibres, with fatty degeneration. The second case, a female child, the second of two children, the first being quite healthy. The birth was normal. During the first year of life the parents noticed nothing amiss. In the second year of life the child gradually began to develop weakness in the lower extremities, and she had, by the end of the second year, complete loss of power in the legs, and had paresis of the arms. There was marked wasting of the buttocks, standing was impossible, sitting was possible, all the visceral functions were normal, the deep reflexes were abolished. The child died when 2 years old. On pathological examination, the anterior roots of the spinal cord were found to be very wasted, and very few normal ganglion cells remained. The muscles showed atrophy of the muscle-fibres and a marked lipomatosis. The cord does not seem to have been examined by the Marchi method.

Armand-Delille and Boudet, in 1908, described the case of a child, 5 months old, the first of two healthy parents. The infant had made normal movements of the new-born during the first weeks of life, but when 2 months old weakness of the limbs was noticed. The muscles of the neck were also weak, and the head flopped to the side.

On examination there was almost complete paralysis of the four limbs and muscles of the neck. There was paralysis of the intercostal muscles. The diaphragm alone carried on respiration. The thorax was deformed. Cutaneous sensation was preserved. All the deep reflexes were abolished. There was absence of response in the muscles, both to faradic and galvanic currents. The sphincters were intact. On account of the subcutaneous fat it was impossible to feel the muscles, which were paralysed and in the course of atrophy. The child died when

5 months old. On examination of the muscle, the nuclei appeared very abundant, and the muscle-fibres markedly degenerate. Some bundles were completely replaced by connective tissue, some of the muscle-fibres preserved their striation, but most were already in a stage of advanced atrophy. A perivascular and interstitial sclerosis was present. The muscle-fibres of the diaphragm were normal. In the peripheral nerves a number of the fibres had disappeared. The anterior roots were atrophied, the posterior roots were intact. The posterior root ganglia showed no change. The white matter of the cord was normal. There was very marked atrophy of the cells of the anterior horns and of the anterior roots. The cells which were less affected showed chromatolysis. The atrophy of the cells of the anterior horn contrasts with the normal appearance of those of Clarke's column. The meninges were intact. All these cases may be accepted as examples of the disease.

The cases, without autopsy, arrange themselves in two groups—those which from their clinical symptoms may be considered as examples of the disease, and those which from their clinical symptoms it would not seem justifiable in the absence of any pathological evidence to the contrary, to include under the Werdnig-Hoffmann group.

GROUP I.—CASES WHICH, FROM THEIR CLINICAL SYMPTOMS, AND WITHOUT AN AUTOPSY, MAY BE CONSIDERED AS EXAMPLES OF THE WERDNIG-HOFFMANN TYPE.

In the first group may be placed the cases of Sevestre and Wimmer's first case, while in the latter group I should place those of Haushalter, Bruns, Senator, and Lange.

Sevestre showed the case of an infant 2½ months old, with complete flaccid paralysis of the body, only the muscles of the head, neck, and diaphragm remaining normal. The muscles of the extremities were atrophic, and they had lost their faradic irritability and gave an anodal contraction on galvanic stimulation. The thorax was deformed. Two other members of the family of six had had the same affection and had died when a few months old. In the discussion Hutinel said he had seen a similar condition in two children born in a breech presentation, and he attributed the condition to spinal hæmorrhage. In one case, however, in which an autopsy was obtained, no such hæmorrhage was present.

Wimmer's case is that of a boy, aged 16 months. The labour was normal, and no other members of the family were affected. A few months after birth difficulty in sucking was noticed. The infant was always very weak, could not raise the arms, but could move the fingers and hands. The child could move the toes, but not the legs or thighs. The neck muscles were weak. The child was not imbecile, had no enlargement of the head, and the movements of the face, eyes, and tongue were normal. The child was a cryptorchid. The deep reflexes were absent. The muscles showed altered electrical reactions. He compares it to Beevor's case and says that it most resembles that.

GROUP II.—CASES WHICH, FROM THEIR CLINICAL SYMPTOMS, AND WITHOUT AN AUTOPSY, SHOULD NOT, IN THE OPINION OF THE WRITER, BE REGARDED AS BELONGING TO THE WERDNIG-HOFFMANN TYPE.

With regard to the second group of cases without autopsy, recorded by Haushalter, Bruns, Senator, and Lange, I am unwilling to accept any of these as cases of muscular atrophy due primarily to atrophy of the cells of the anterior horn. Haushalter, indeed, regards his cases as primarily myopathic, though described under the title "Amyotrophie primitive progressive dans l'enfance," the first being a fairly typical case of facio-scapulo-humeral type, the second belonging to the simple atrophic type, and the third probably to the myatonia congenita type, though in many features this case most closely resembles the Werdnig-Hoffmann type.

Bruns records three cases from different families. In the first family two sisters had died of the same complaint. In the second family there was no other case. In the third family the two elder and one of the younger members were healthy. Bruns counts these as five cases, and added to the nineteen cases already published by Hoffmann, and the two cases by Werdnig, and one by Bruce, says that there are now twenty-eight recorded cases of the disease.

The first case is that of a girl, I. R., aged 10, the fourth of four children. The family history was as follows: The first child, a boy, stood and walked at 9 months. Paralysis then occurred and he died when 4 years old. The second, a girl, died from inflammation of the lungs when 3 years old. The third, a boy, normal at birth, developed well till 9 months old, began to sit up, but could not stand, the hands were unsteady. When 2½ years old he was examined. The child could just sit up, but easily fell backward. The legs could not be bent at the hips, the weakness of the quadriceps extensor was very marked. There was loss of faradic irritability in the muscles, and a very weak reaction to galvanism. The weakness gradually extended and the child died when 3 years old. There was no post-mortem.

The girl, I. R., developed fairly well in the first two years of life. She nearly walked at 1¾ years, after which no progress was made. She frequently fell, and her gait became progressively worse. Weakness of the arms came on at a later date. When seen in 1894 there was very marked curving of the spine, wasting and feebleness of the arms, a weakness of the legs. The paralysis was of a flaccid character. The deep reflexes were absent. The lower arm and hands were less affected than the shoulder muscles, and the toes less affected than the upper portion of the leg. Fibrillary tremors were present. There was loss of electrical excitability, but no distinct reaction of degeneration. Intelligence, sensation and sphincters were normal. In 1896 the weakness had increased and she had difficulty in holding up the head. In 1898 there was still more weakness, atrophy of the shoulder girdle and the thighs, and more wasting of the calf muscles. In 1900 she died, when 16 years old. There was no autopsy.

The second case, E. F., aged 11. Nothing is known of the family history. She had general weakness of the shoulders and hips, standing was impossible, there was a marked kyphosis, fibrillary tremor in the shoulder muscles, diminished electrical reaction, absence of the deep reflexes. She remained in this condition.

The third case, A. N., boy, aged 3; two older brothers, aged respectively 7 and 5, are healthy, as is also the sister, aged 8 months. This child walked and talked at 1 year and 3 months. When 2 years old the mother noticed gradually increasing weakness. The child lost the power of walking, developed a tremor of the hands and weakness of the back. The legs showed the greatest amount of weakness. The face, eye, muscles of the tongue were unaffected. There was fibrillary tremor of the muscles of both hands. Kyphosis was present in the lumbar region and the deep reflexes were absent.

Bruns, in considering these cases, says they are undoubted cases of Werdnig-Hoffmann's disease. He notes the difference in age between his cases and those of Werdnig and Hoffmann, but he says that they cannot be mistaken for any progessive form of muscular dystrophy. He suggests that the Charcot-Marie-Tooth, or peroneal, is the type which most resembles these cases, but the atrophy in these begins in the peripheral portion of the limbs and extends upwards.

I should not be prepared to agree with this statement, for the cases rather closely resemble some of the forms of dystrophy, which begin in the proximal muscle, and a very similar case with curvature of the spine has been figured by Jendrassik.[1] Without a pathological examination Bruns is hardly justified in placing in the Werdnig-Hoffmann group a type of case which differs so markedly in course and clinical features from that group.

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[1] *See* Case 10, *Deutsch. Zeitschr. f. Nervenheilk.*, 1902, xxii, p. 441.

Senator describes two cases, a brother and sister, under the title "Zur Kenntniss der familiaren progressiven Muskelatrophie im Kindesalter."

The first case is that of a boy, aged 8, healthy at birth. He walked at 11 months. When 2 years old he often fell. Walking and standing became more difficult, so that when 3 years he had nearly always to be carried. He learnt to write, but when between 6 and 7 he lost this power, owing to the shaking of his hands. His memory also began to fail. There was atrophy of the shoulder and upper arm muscles, atrophy of the back muscles, moderate lordosis and the muscles showed fibrillary tremors. The knee-jerks were absent, sensation was natural. There was altered electrical excitability. Six months later the atrophy of the buttocks and shoulders had made further progress. The gait was unaltered. He was neither better nor worse.

The second case, G. S., a girl, aged 5½, walked at 10 months. When 18 months old she had a fit. When 2 years old she became unsteady on her legs, like her brother, and at the same time there was tremor of the hands. She walked with a wide base, was shaky, had marked lordosis and a lateral curvature with the convexity to the right. There was wasting of the back muscles and to a lesser degree of the upper arm and pectoral muscles. The buttock muscles were flabby, but the lower legs appeared well developed. Fibrillary tremor was present in some muscles. There was diminished excitability in the muscles.

Senator, in considering the differential diagnosis of these cases, questions whether they belong to the neuropathic form of Werdnig and Hoffmann, or to Erb's myopathic form of simple atrophy. He says that Heubner, Schulz and Preisz have shown changes in the nervous system in some cases of myopathy. He thinks these two cases are probably spinal in origin.

Lange describes two cases, a brother and sister, aged 11 and 6 respectively, the first and fourth children of a family of six. The second child was said to have died of a similar complaint when 3 years old.

The first case, a boy, did well for the first nine months of life, then he began to lose power in the limbs, lost the power of sitting up, and also the power in the neck muscles. When 3 he developed double cataract.

When 11 years old he was unable to stand, there was marked wasting of the proximal muscles of the limbs. The shoulders were loose. The knee-jerks were absent.

The second child, sister of the above, aged 6, was quite well till 9 months. Then the legs began to waste. She never learnt to walk. When 3 years old she, like her brother, developed cataract. She could only sit when propped up.

Lange is inclined to attribute these cases to the Werdnig-Hoffmann group, but he says it is impossible to separate such cases from the myopathies without a pathological examination.

In conclusion, as belonging to the Werdnig-Hoffmann group one would accept in addition to the case of Hoffmann, published in 1900, Beevor's case, von Ritter's two cases, Armand-Delille and Boudet's case, all with the pathological examination, and also Sevestre's and Wimmer's first case without a pathological examination. I should not, however, be prepared to accept as belonging to this group the cases of Bruns, Senator and Lange without a pathological examination.

The following list of thirteen cases comprises all those in which the diagnosis has been confirmed by autopsy, and may be accepted as belonging to the Werdnig-Hoffmann type. They have been arranged according to the age at which death occurred. The list shows the marked variation in the duration of symptoms.

Recorder	Age of onset	Sex	Age of child	Family history	Year of publication
1. Batten ...	14 days ...	M.	5 weeks ...	0	1911
2. Beevor ...	Birth	M.	2 months ...	Yes	1900
3. Armand-Delille and Boudet	2 months ...	M.	6 months ...	0	1908
4. Batten ...	Always weak ...	M.	8 months ...	0	1911
5. v. Ritter ...	5 months ...	F.	15 months ...	0	1904
6. v. Ritter ...	12 months ...	F.	2 years	0	1904
7. Hoffmann ...	7 months ...	F.	2½ years ...	Yes	1897
8. Werdnig ...	10 months ...	M.	3 years	Yes	1891
9. Batten ...	Always weak ...	M.	3½ years ...	0	1911
10. Hoffmann ...	9 months ...	F.	5 years	Yes	1893
11. Hoffmann ...	4 months ...	F.	5 years	Yes	1900
12. Werdnig ...	10 months ...	M.	6 years	Yes	1894
13. Bruce and Thomson	12 months ...	F.	6 years	0	1893

SUMMARY.

The paper deals with the widespread progressive muscular atrophy of infants and young children due to a spinal lesion. The paper is based on the clinical and pathological examination of eight cases. These, on pathological grounds, are divided into three classes:—

(1) A type of case in which progressive muscular weakness occurs

during the first week or month of life, gradually progresses, and terminates in death after a variable period of weeks, months, or years. Sometimes more than one member of the family may be affected. The pathological change found in these cases is a degeneration of the lower motor neurons, the character of the change depending on the time after the onset of the disease at which death takes place. The type corresponds to the cases described by Werdnig and Hoffmann.

(2) A type of case in which progressive muscular weakness and atrophy begins somewhat later in life, after the child has already walked, and slowly progresses till death occurs from respiratory failure or pneumonia. The pathological changes found in such a case resemble those found in a toxic neuritis.

(3) A type of case in which progressive muscular weakness and atrophy begin in later life, after the child has already walked, slowly progresses, and is attended by a widespread myelitis of the spinal cord.

Six of the recorded cases are assigned to the first group, one to the second, and one to the third.

The literature is then considered. Ten cases belonging to the first group are at present on record with a pathological examination; two recorded by Werdnig, three by Hoffmann, two by von Ritter, one by Bruce and Thomson, one by Beevor, and one by Armand-Delille and Boudet.

Of recorded cases assigned to the Werdnig-Hoffmann group without an autopsy, the writer is unwilling to accept those of Senator, Bruns, Lange, and the second case of Wimmer's, but would accept Wimmer's first case, and that recorded by Sevestre.

The great difficulty in the diagnosis of these cases from cases of primary myopathy is recognized, and reference is made to cases recorded by Finkelnburg, which on clinical grounds might well be assigned to primary spinal atrophy, but on pathological grounds were shown to belong to the myopathic group.

APPENDIX.

Case I.—C. H., aged 4 weeks, was seen at the Children's Hospital, Great Ormond Street, in May, 1903. The child was the first. The parents were healthy. There had been no miscarriages, the labour had been prolonged, but no instruments had been used. At birth the cord was round the child's neck, and the child was cyanosed. The child was breast-fed for the first fourteen days and seemed to thrive. It was then fed on goats' milk. Wasting and loss of power in the limbs was first noticed when the child was 14 days old, and the child gradually became feebler. The child sucked well, but seemed to get easily tired. There was no vomiting. The child had been rather constipated. On examination the child was small, wasted, weighing 5½ lb. It lay with the arms at its side and did not attempt any movement. Respiration was feeble, the pulse from 84 to 96 per minute. The child seemed to see and to take notice. The eye movements were good, it sucked well, and there was no paralysis of the tongue or palate. There was no regurgitation; it cried fairly well; there was complete flaccid paralysis of the muscles of the neck.

The arms lay quite flaccid by the side or across the abdomen, or wherever placed. The child was just able to move the fingers, the arms were thin and wasted. There was no power in the muscles of the back, and very little in the intercostal or abdominal muscles. There was almost complete flaccid palsy of all the muscles of the leg, but some slight movement remained in the toes. The knee-jerks were absent. The child certainly felt a pinprick all over the body.

On electrical examination of the muscles no contraction could be obtained to strong faradic current in any of the muscles of the arm, trunk, or leg. The child died three days after admission to the hospital. On post-mortem examination nothing abnormal could be found on naked-eye examination in the visceral or nervous system. The brain and spinal cord, the right median, the right phrenic, left sciatic nerves, and biceps muscle were preserved for examination.

The examination of the tissues was carried out by the Marchi, Nissl, Weigert-Pal, and Van Gieson methods.

Microscopical examination: The spinal cord by the Marchi method showed at the level of a third sacral segment marked degeneration in the finer fibres of the grey matter of the anterior horns (fig. 4). These degenerate fibres arising from the cells of the anterior horn form bundles which can be seen passing through the white matter of the cord towards the peripheral, where they form, after their exit from the cord, the anterior roots. Numerous degenerate fibres can be seen in these anterior roots (figs. 2 and 3). The fibres of both anterior horns are about equally affected. The cells of the anterior horn appear for the most part normal and show no evidence of pigmentation or degeneration. It is not possible to assert that one group of cells have more degenerate fibres arising from them than another group. No degenerate fibres could be seen in the antero-lateral tracts other than those above mentioned, which were passing outwards to the anterior roots. In the posterior columns, however, a few degenerate fibres could be seen in the posterior root zone, and also in the posterior roots both within the cord and outside.

At the level of the third lumbar a similar condition was present—viz., degeneration of the fine fibres passing from the anterior horns to the anterior roots. A few degenerate fibres were also present in the posterior columns and roots.

At the level of the first lumbar the degeneration becomes less marked, but a few degenerate fibres can now be seen in the region of Clarke's column, and although they cannot be traced to the direct cerebellar tract, yet they tend in that direction, and scattered degenerate fibres can be seen in this tract. A few degenerate fibres are present also in the posterior columns and roots.

Throughout the dorsal region but little degeneration can be seen (fig. 1). There are a few degenerate fibres in the anterior horns and also in the posterior columns, but the cells of Clarke's column appear normal, and there are no degenerate fibres in that region. (Figs. 1, 2, 3, 4.)

At the level of the first and second dorsal segments degenerate fibres again become more numerous, and at the level of the sixth cervical segment marked degeneration is again present in the grey matter of the anterior horns (fig. 1). The degeneration is more marked about the median group of cells, and least about the lateral group. In the posterior columns there are a few degenerate fibres, whilst there are practically no degenerate fibres in the antero-lateral tract. All through the cervical region there is considerable degeneration which, however, becomes less marked again in the upper cervical segments. There is a small bundle of fibres situated at the root of the posterior horn, which contains degenerate fibres: this can be traced from the seventh cervical segment upwards to the lower portion of the medulla, where it apparently leaves the cord by a lateral nerve-root (spinal accessory fibres).

By the Nissl method the cells of the anterior horn appear numerous, and for the most part well stained. Here and there individual cells may show some chromatolysis, but the greater part of the cells are normal, both as to number and character (figs. 5 and 6).

By the Weigert-Pal method the cord appears normal for a child of this age, although the pyramidal tracts are not fully medullated.

By the Van Gieson method there is no evidence of vascular or interstitial change.

Brain: No change could be found in the cortex of the cerebral hemispheres, in the cerebellum, or in the pons. In the medulla a few degenerate fibres can be seen in the intramedullary course of the twelfth nerve.

The peripheral nerves, the right phrenic, the left sciatic, the right median, examined by the Marchi method, do not show any degeneration, and also appear normal by other methods of examination.

The biceps muscle does not show any marked change. The muscle-fibres are in the infantile condition, uniform in size, there is no fibrosis, and the intramuscular nerves are relatively well stained.

The result of the examination of this case is to show that degeneration has taken place in the lower motor neurons. The changes present are similar to those seen in diphtheritic paralysis—viz., a marked degeneration of the myelin sheath, as shown by the Marchi method, and comparatively little change in the cells of the anterior horn.

Case II.—W. G., aged 5 weeks, was admitted to the National Hospital under the care of Dr. Beevor. The child was the eighth of eight children, four of whom had been similarly affected—viz., the first died at 4½ months, the second died at 8 months, the fourth died at 7 months, and the eighth (the patient) died at 2 months.

The child was full term, there was difficulty with the labour, and he was noticed to be paralysed at birth.

On examination there was complete flaccid paralysis of all four limbs, with the exception of very slight involuntary movements of the fingers of the left hand. Sensation was said to be lost on the limbs and on the trunk to the second rib. All the deep reflexes were absent.

The pathological examination showed no macroscopic change, but on microscopical examination atrophy of the cells of the anterior horns (figs. 7 and 8) and degeneration of the posterior columns were found.

For a full clinical and pathological report of this case see *Brain*, 1902, xxv, p. 85.

Case III.—E. T., aged 4 months, was admitted to the Children's Hospital, Great Ormond Street, under the care of Dr. Penrose, in October, 1901.

History : General helplessness was first noticed one week after birth, and had steadily increased since. The limbs wasted and the breathing became difficult. There was some difficulty in swallowing at times. The child is the fourth of four children. The first is alive, aged 4, and is well. The third child died at 5 months and is said to have had a similar condition to the present patient.

Present condition : The child lies on its back, crying feebly, but employing all its facial muscles. There is no facial asymmetry. The thorax is quite motionless in breathing ; while the abdomen moves freely. There is marked wasting of the shoulder muscles and those of the limbs. The child can apparently do nothing except make a slight movement of the toes. The forearm can be feebly flexed and extended and all movements of the upper arm are capable of being feebly performed, though the child does not raise the arms above the shoulders. The wrists are dropped. The child is absolutely unable to sit up, and when raised the head falls about helplessly. Fibrillary tremor is noticed in the muscles of the arm. The movements of the tongue and eyes appear normal. The deep reflexes are absent and the plantar response, when it can be elicited, is in the direction of the extension of the small toes. The child died. No post-mortem was obtainable.

Case IV.—P. B., aged 6 months, fourth of four children. One died of fits at 4 months, due to injury at birth. One died of tuberculosis. The boy was born at full term, and the labour was natural. He was said by the mother to be healthy at birth. When 4 months old it was noticed that the child could not move the arms. The onset seems to have been quite gradual.

On admission to hospital the child was well nourished, lay on its back taking notice of all around it, smiling, and seemed quite intelligent.

Both arms lay by the side absolutely flaccid, except for some movements of the fingers. Both legs were quite flaccid and powerless. Respiration was almost entirely abdominal, and there seemed to be paralysis of the intercostals. The neck muscles were quite powerless. All the deep reflexes were absent: superficial reflexes indefinite. Sensation apparently perfect. On electrical examination practically no contraction could be obtained to faradic stimulation, except in the flexors of the fingers, and then only with a current which will produce a tetanus in the muscles of a normal individual. To galvanism no definite contraction obtained.

The fundus of the eyes was examined and appeared normal. The child developed broncho-pneumonia and died. No autopsy was obtainable.

The clinical features which this child presented seem to be almost typical of the progressive muscular atrophy of infants, but without the verification of a post-mortem the case loses much of its importance.

Case V.—H. C., aged 8 months, was the third of three children, the other two being healthy. The child had been breast-fed: he had had no difficulty in swallowing and had had no illness. The birth of the child was natural. The history was that the child had always been weak and had never been able to sit up, nor could he hold up the head. The weakness seems to have increased, and when first seen, besides the weakness of the neck muscles above described, the arms were perfectly flaccid, the deltoids being especially weak. The legs were flaccid, only the slightest movement being possible. The knee-jerks were present, but only obtained with difficulty, and the plantar usually gave a flexor response.

There was marked weakness of the back, with a lateral curvature, the concavity being to the left. The pupils were equal.

The child developed broncho-pneumonia and died in a few days.

At the autopsy there was extensive broncho-pneumonia. There was a marked lateral curvature of the spine to the left. The spinal cord and membranes appeared normal. There was no wasting of the cord. Brain appeared normal. All the muscles very pale, but not atrophied. Diaphragm looked a good colour.

Pathological examination: The cord was examined by the Marchi, Nissl, and Van Gieson methods. The cells of the spinal cord appear to be fairly numerous, and show only slight chromatolysis. In most of the cells the nucleus is centrally placed. The anterior nerve-roots do not appear unduly small.

Examination by Marchi method shows a certain amount of degeneration in the anterior roots very similar to that found in the Case I, but to a far less degree.

There was in this case no microscopical examination of the peripheral nerves or muscles.

Case VI.—R. A., a boy, aged 3½, was admitted into the Hospital for Sick Children, Great Ormond Street, under the care of Dr. Garrod, June, 1910. He was one of twins; the other (a male) is healthy. This child cut his first tooth at 8 months, and learnt to talk early. The boy never learnt to sit up, and when 15 months old it was noticed that he could not move his legs. He, however, moved his arms quite well. When 2½ years old he was seen by a surgeon, who ordered him a Phelps's box in which he was kept lying down.

The boy is the result of the fifth pregnancy. The eldest child, aged 15, is alive. There has been one miscarriage and two children stillborn.

The boy lies on his back and he can be freely moved about without pain. He is intelligent and can talk well. There is no affection of the ocular nerves. He is quite unable to sit up, nor can he maintain the sitting posture when sat up. He can, however, hold up his head. He can move the arms about fairly well, but there is some contraction of the outer phalangeal joints which limits his movements. There is a peculiar "athetoid" movement of both hands, and when he grasps an object he overdoes the action. The legs lie flaccid in bed, but the right knee-joint cannot be extended more than 35°. The legs are generally wasted and flabby and there is wasting of both lower limbs, especially above the knee, and this is rather more marked on the right side than the left. The right thigh is adducted and the adductor muscle is contracted. There is genu valgum and talipes valgus on the right side. There is no power of movement in the lower limbs, but they can be freely moved passively, and there is marked flaccidity, except for the contraction above mentioned.

There was well-marked lateral curvature in the lower dorsal region, with convexity to the right. The muscles of the back were wasted. There was no tenderness.

The left knee-jerk was present, the right was absent. No plantar response was obtained. The abdominal reflex was present on the left, absent on the right side. Sensation was perfect. Electrical examination showed greatly diminished excitability to both faradic and galvanic current.

I saw the boy in June, 1910, and from the clinical features came to the conclusion that the case was probably one of old and extensive poliomyelitis. I failed to recognize the progressive nature of the case and attributed his continued weakness to the fact that he had been kept for a year in a Phelps's box. On July 8 he had some retention of urine, which was relieved by the catheter. This retention passed off on the 12th. X-rays showed no disease of spine.

On August 24 intercostals became very weak. Broncho-pneumonia developed, and the child died on September 3.

The post-mortem showed some patchy broncho-pneumonia. The brain and spinal cord appeared normal to the naked eye, except that the anterior roots appeared smaller than normal. The posterior roots were well developed. The spinal cord was examined by the Marchi, Weigert-Pal, Nissl, and Van Gieson methods.

By the Marchi method practically no recent degeneration could be found. There were in the grey matter of the anterior horns in the cervical region a few fibres which showed degeneration, but they were very few in number.

By the Weigert-Pal method the whole of the white matter of the antero-lateral tracts was paler than the posterior columns, but no tract could be said to be definitely affected. There was no diminution in the size of the grey matter, and the medullated fibres in the grey matter appeared fairly normal. The most striking feature was the smallness of the anterior roots as compared with the posterior, and the great diminution of fibres. The most marked change was to be seen in sections stained by the Nissl and Van Gieson methods. The absence of cells in the anterior horns, both in the lumbar, thoracic and cervical region, is most striking. In any given section one or two normal cells can generally be seen, but the other cells are small, apparently well formed, but only about half the size of the normal cell. Many cells must have entirely disappeared. If the number of cells of moderate size are counted in a series of sections in the lumbar region, they average out about ten to a section, whereas the normal average out to about forty, and these figures do not truly represent the state of affairs, for the size of the cells in this cord is somewhat smaller than in the normal cord (figs. 9 and 10.)

Case VII.—H. M., aged 3, the youngest of four healthy children, was admitted to the Children's Hospital, Great Ormond Street, under the care of Dr. Voelcker, in October, 1908. She was healthy at birth, and well up till ten weeks ago, when it was noticed one morning that she was unable to walk properly and fell backwards into a sitting position after taking a few steps. On stooping she was unable to regain the upright position. She had incontinence of urine and fæces. The arms were said to be unaffected and the child was able to feed herself. There had been no constitutional symptoms.

Seven weeks later the arms became weak, causing her difficulty in feeding herself. There was irritability, but no evidence of pain.

She is a bright, intelligent, and well-nourished child. She can move her legs, but cannot stand. There is weakness of both deltoids, but the forearm muscles are good. The cranial nerves, optic disks, and trunk muscles are normal. Sensation is unimpaired. All the deep reflexes are absent, but the abdominals are present and the plantars give an indefinite response.

When at first in the hospital there was a definite increase of muscular weakness. The arms became powerless, only movements of the fingers and slight movements of the wrists remained. The back muscles were weak, and there was a curvature, with the convexity to the right. The legs became weaker and more wasted. A definite weakness of the right side of the face developed. This passed off in a few days. There was the reaction of degeneration in the muscles of the lower limbs.

In December, some two months after admission to the hospital, there was a gradual improvement, so that the child could now get the hand up to the mouth and move its legs. The knee-jerks were still absent. This improvement was maintained till February, 1909. The weakness then again began to increase, the intercostal muscles and diaphragm became affected, and in October, 1909, the child died, twelve months after admission to the hospital, and fourteen months after the onset of the disease.

On post-mortem examination nothing abnormal to the naked eye could be found in the brain or spinal cord.

The cord and medulla were examined by the Weigert-Pal, Nissl, Marchi, and Van Gieson methods.

The cord, examined by Weigert-Pal method, appeared perfectly normal.

By the Marchi method: There was scattered degeneration throughout the length of the spinal cord. The amount of the degeneration was slight, and could not be said to affect any particular tract of the cord. If anything, the anterior and posterior roots in their passage through the white matter of the cord were rather more affected than other parts. There was a little fine degeneration in the grey matter in the region of the anterior horns. The intramedullary portions of the twelfth nerve also show some fine degeneration. The extramedullary portion of the anterior and posterior roots practically show no change.

By the Nissl method: The cells of the grey matter are well stained. Many of the cells of the anterior horns in the lumbar, thoracic, and cervical regions of the cord appear perfectly normal. Some cells, however, in each region of the cord show chromatolytic changes. These changes are not confined to any one group of cells, but affect individual cells in any group, a cell in close proximity being perfectly normal (figs. 11 and 12).

The changes which these cells exhibit are a somewhat swollen appearance, the nucleus being eccentric, but the nucleolus well stained.

The chromophylic substance of the cells stains diffusely, and it is only at the margin of the cell that the normal chromophylic granules can be seen.

There are very few cells that can be said to be in a state of profound degeneration, and in most of those affected the change is comparatively slight.

The cells of Clarke's column also show some changes.

By the Van Gieson method: The interstitial tissue and the vessels of the cord appear perfectly normal, and there is no evidence of either past or present inflammation.

The peripheral nerves examined were those of the sciatic and brachial plexus.

By the Marchi method: A very considerable amount of degeneration of the myelin sheath could be seen. This was more marked in the nerves of the brachial plexus than in the sciatic.

By the Van Gieson method: The same nerves appear perfectly normal, and there is no evidence of interstitial inflammation.

The muscle-fibres of the diaphragm show most marked fatty degeneration when stained by the Marchi method.

The changes found in this case point to a toxic degeneration of the lower motor and sensory neurons. Since the changes are more marked in the peripheral nerves than in the cell body, the case would fall into the group known as toxic neuritis. The clinical course and symptoms do not, however, correspond with those commonly associated with that disease.

Case VIII.—M. L., aged 11. Twelve months, weakness of hands. Seven months, unable to button clothes. Four months, weakness of legs. Two months, adenoids removed, and much worse since that operation. Fourteen days, pain in back, and increasing difficulty in swallowing.

Intelligent, deaf, some atrophy of tongue, palate moves well on phonation, weakness of neck, respiration thoracic, upper intercostals only in use. Claw hand, wrist-drop. Weakness of upper arm and shoulder muscles. Can walk, but only feebly. Knee-jerks brisk, plantar flexor, sensation natural to all forms.

Electrical reaction R.D. in some muscles, in others only diminution of irritability. Sudden death from respiratory failure six months later, eighteen months after onset of disease.

Pathological examination showed an extensive destruction and atrophy of the cells of the anterior horn, from the upper cervical to the lower thoracic segment, extensive degeneration of the whole of the ventral region of the cord in the thoracic segments. Degeneration of the direct cerebellar tracts both dorsal and ventral.

There was a marked increase of the connective tissue in the ventral portion of the cord in the thoracic region.

The walls of the vessels showed no change. The cord showed both recent and old degeneration.

The change in the cord is attributed to a chronic myelitis secondary to some vascular change and toxic blood state. For full clinical and pathological report of the case, see *Brain*, 1903, xxvi, p. 473.

REFERENCES.

[1] Armand-Delille and Boudet. "Les lésions anatomiques de l'amyotrophie spinale diffuse des nouveau-nés," *Arch. de Méd. des Enfants*, Par., 1908, xi, p. 32.

[2] Batten. "Hereditary Form of Progressive Muscular Atrophy with Spinal Lesions in Young Children," *Brain*, 1897, xx, p. 536.

[3] Beevor. "Case of Congenital Spinal Muscular Atrophy," *Brain*, 1902, xxv, p. 85.

[4] Bruns. "Zur Casuistik der infantilen progressiven spinalen Muskel-Atrophie von familialem resp. hereditärem Charakter," *Deutsch. Zeitschr. f. Nervenheilk.*, Leipz., 1901, xix p. 401.

[5] Catola. "Quelques recherches sur le système nerveux central d'enfants issus de parents en état morbide," *Rev. de Méd.*, Par., 1910, xxx, p. 720.

[6] Finkelnburg. "Anatomischer Befund bei progressiver Muskel-dystrophie in den ersten Lebensjahren," *Deutsch. Zeitschr. f. Nervenheilk.*, 1908, xxxv, p. 453.

[7] Fletcher and Batten. "A Case of Progressive Muscular Atrophy of Spinal Origin in a Girl aged 11 years," *Brain*, 1903, xxvi, p. 473.

[8] HAUSHALTER. "Trois cas d'Amyotrophie primitive progressive dans l'enfance," *Rev. de Méd.*, Par., 1898, xviii, p. 445.

[9] HOFFMANN. "Ueber chronische spinale Muskel-Atrophie im Kindesalter auf familiärer Basis," *Deutsch. Zeitschr. f. Nervenheilk.*, Leipz., 1893, iii, p. 427.

[10] *Idem.* "Weiterer Beitrag zur Lehre von der hereditären progressiven spinalen Muskelatrophie im Kindesalter," *Deutsch. Zeitschr. f. Nervenheilk.*, 1897, x, p. 292.

[11] *Idem.* "Dritter Beitrag zur Lehre von der hereditären progressiven spinalen Muskelatrophie im Kindesalter," *Deutsch. Zeitschr. f. Nervenheilk.*, 1900, xviii, p. 217.

[12] JENDRASSIK. "Beiträge zur Kenntniss der hereditären Krankheiten," *Deutsch. Zeitschr. f. Nervenheilk.*, 1902, xxii, p. 444.

[13] LANGE. "Beitrag zur Klinik der progressiver Muskelatrophie im Kindesalter," *Deutsch. Zeitschr. f. Nervenheilk.*, 1910, xl, p. 65.

[14] v. RITTER. "Zur Kenntnis der progressiven spinalen Muskelatrophie in frühen Kindesalter," *Jahrbuch der Kinderheilk.*, Berl., 1904, lix, p. 224.

[15] SENATOR. "Zur Kenntnis der familiären progressiven Muskelatrophie im Kindesalter," *Charité Annalen*, Berl., 1902, xxvi, p. 81.

[16] SEVESTRE. "Paralysie flasque des quartre membres et des muscles du tronc (sauf le diaphragme) chez un enfant de deux mois et demi," *Neurol. Centralbl.*, Leipz., 1900, p. 904.

[17] THOMSON and BRUCE. "A Case of Progressive Muscular Atrophy in a Child with a Spinal Lesion," *Edinburgh Hosp. Reports*, 1893, i, p. 361.

[18] VOELCKER. "A Case of Widespread Peripheral Neuritis of Uncertain Origin," *Trans. Med. Soc. Lond*, 1909, xxxii, p. 355.

[19] WERDNIG. "Zwei frühinfantile hereditäre Fälle von progressiver Muskelatrophie unter dem Bilde der Dystrophie, aber auf neurotischer Grundlage," *Arch. f. Psych.*, Berl., 1891, xxii, p. 437.

[20] *Idem.* "Die frühinfantile progressive spinale Amyotrophie," *Arch. f. Psych.*, 1894, xxvi, p. 706.

[21] WIMMER. "Zwei Fälle von congenitalem Muskelleiden bei Kindern," *Arch. f. Psych.*, 1907, xlii, p. 960.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FOURTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE
SESSION 1910-11

OBSTETRICAL AND GYNÆCOLOGICAL SECTION

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1911

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Obstetrical and Gynæcological Section.

October 6, 1910.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

The Lessons of a Session.

It may be well at the close of the year to review the work which has been done during the session, and to ask how we have added to our experience, and what permanent and practical results are likely to follow from the meetings and discussions that have taken place.

Quinquennial Summary of Proceedings.

There is to me always a certain atmosphere of sadness surrounding the volumes of transactions of a medical society. Every now and then some investigator, interested in unearthing a record, pathological or clinical, which he requires to illustrate his view or enforce an argument, ransacks the library shelf in search of the reference he requires. Sometimes years elapse before this record is dragged to light, and the work of research, which has taken great pains and much time to complete, is again brought to notice. It has remained immured in the catacombs of transactions, awaiting some happy necessity of an explorer, who, in his desire to embellish a theory or amplify some fact, seizes on it and utilizes it, giving it the tardy recognition of its originality and worth.

It may be said that this is just the use of transactions—to serve as records for future reference. This is true, yet I cannot help feeling that it would be a great gain to our department if every five years a digest of the transactions appeared, in which the various subjects dealt with would be carefully and systematically codified, with a brief epitome of each author's deductions appended. A Section such as this would surely provide an editorial staff willing to carry this out, and I have no doubt a publisher would be found ready to take the risk of publication and issue the small volume at a very moderate cost. In it the original communications and the discussions on these would be summarized, the specimens classified, and the remarks on these epitomized.

Functions and Aims of the Section.

It is my desire to-night to illustrate my meaning in miniature by summarizing the work of one session and classifying it under the three heads of Communications, Clinical Cases, and Specimens. The communication, be it in the form of a pathological specimen, a clinical history, or an original thesis, which leaves no practical lesson in its track, opens up no new field for exploration, or does not assist in solving some unravelled mystery is not worth the time taken up in listening to it—yet it is obvious that there must be degrees of importance in each of these modes of communication. We might take them in this order: (*a*) A new theory (provided it be not "a maggot in the brain" of the advancer) based on original observation; previously unnoticed or rarely observed pathological changes, and their initial ætiological relations. (*b*) Unrecorded or unusual clinical experiences, bearing on treatment and the action of certain therapeutical remedies. (*c*) Confirmatory evidence touching some doubtful or disputed point in operative procedure. In short, some distinct gain in progress, either in the substantiation of previously advanced views requiring confirmation or the demolition of those which do not stand the test of time and further investigation. What there is not room for is—the study of generally accepted ancient history.

There is, however, another most important aspect of a society such as this. Representing as it does all the divisions of the, *as yet*, United Kingdom, it ought to be the accepted source of reference and tribunal of appeal in this country for deciding those critical questions which arise in the every-day practice of obstetrics and gynæcology. Opinions are fairly agreed on the great majority of these. Some are yet *sub judice*. It is in the handling of these latter that so great a responsibility rests. That which may be safe in procedure or practice in the hands of some is absolutely unsafe and dangerous in those of other men. We can also do much to combat the regrettable tendency to look too lightly on the incipient stages of disease in the female genitalia. Resort to operation has become so usual that it is always present to the mind as a ready and final means of escape from error, doubt, difficulty, or danger.

Therefore there are two directions in which the Section can influence the general practice of this branch of medicine. One, which demands the utmost caution in its following, is the recommendation for general application of new modes of practice, the carrying out of which by inexperienced hands is fraught with possibly greater immediate danger to life than is the condition for which they are undertaken. The other is in that of early and correct diagnosis. It calls for enforcement through repeated emphatic utterances and convincing proofs of the grave responsibility that attaches itself to temporizing and masterly inactivity. For these reasons it is well that, in addition to those aims I have referred to, the Section should keep before it the position it occupies as a recognized guide when the practitioner arrives at one of those cross roads in practice where the turning he takes means life or death to the patient. Communications and discussions are thus occasionally justified that may seem to some unprofitable, yet which have lessons of supreme value to others who have had less opportunity and experience.

Communications—Electro-therapy in Gynæcology.

Following the division I have made, and taking the communications in chronological order, the first paper on the list is that of Dr. Curtis Webb, on "Points for the Use of, and Indications for, Electro-therapy in some Gynæcological Affections." Considering the action of the direct current under three heads—chemical polar, ionic, and inter-polar—he dealt mainly with the two former. Incidentally he bore testimony to the sedative action of the rapidly interrupted induced current in neuralgic affections of the pelvic organs, as also of static electricity in dysmenorrhœa, and dwelt on the influence upon nutrition of the chemical interchanges that occur throughout the circuit, and the circulatory effects that result from vaso-motor stimulation and the contraction produced in unstriated muscular tissue. Summarizing the effects of the faradaic, constant, and static currents, he divided the first two of these into (*a*) sedative and stimulation contraction; (*b*) (1) cauterizing, (2) congestive or liquefying, (3) ionization, and (4) metabolic. Treatment by the static current, the physiological effects of which were summarized, was not entered into.

Dr. Curtis Webb was rather optimistic in his review of the results of electro-therapeutic treatment by ionization of inflammatory affections of the vagina, disorders of menstruation, and some diseases of the uterus and adnexa. Certainly he adduced sufficient evidence to prove that in ionization we have an efficient therapeutic agent in cases specially selected for its employment. His paper, however, and the discussion which followed it, tended to show that such electropathic treatment demands primarily three essential conditions for its safe application: (1) Special training and skill in the application of the kind of electricity used, its nature and quantity; (2) accurate diagnosis and differentiation of the states in which it is contra-indicated; (3) reliable judgment as to its continuance or abandonment, according to the effect it produces.

The associated danger appears to be twofold: first, the temptation to postpone more radical measures in those cases in which time is an all-important element for success; secondly, that it is liable to be used for conditions for which physically and pathologically it is not merely unsuitable but dangerous.

Leukoplakic Vulvitis and Kraurosis.

Dr. Comyns Berkeley and Mr. Victor Bonney made one of the most interesting and original contributions to our year's work, on leukoplakic vulvitis. Here the different stages in the pathological processes were beautifully illustrated by microphotographs, which have been reproduced in the *Proceedings*, and the differences in the microscopical appearances present in kraurosis vulvæ were demonstrated. The two diseases were thus shown to be pathologically distinct. An important diagnostic difference in their symptomatology was referred to—viz., the occurrence of pruritus in leukoplakia and its absence in kraurosis, while in the latter affection dyspareunia is more usually present. This does not altogether coincide with my own experience, though I may have in the past confused the two conditions. One most important point in the communication was the inquiry into the *relationship between leukoplakic vulvitis and carcinoma of the vulva*. The authors stated that "in any case of carcinoma of the vulva operated upon by them leukoplakic vulvitis had always been present," while they knew of "no instance where kraurosis had been associated with malignancy." Here, again, we require a larger experience to decide the accuracy of the conclusion, for many authorities do regard kraurosis (in the sense in which the authors apply the term) as having a correlation to epitheliomatous change. For example, at the same meeting Dr. Arthur Giles narrated the particulars of an advanced case of kraurosis of the vulva in which there was a malignant growth at the one side of the vulva, and he stated that he had seen malignancy follow in one previous instance. I have been so far fortunate in not having in any case of kraurosis that I have treated seen malignancy result. According to the authors, it is most likely to occur in the second stage of the leukoplakia, when there is excess of cellularity and rarefaction of the sub-epithelial tissues. A succinct review of the pathology of carcinoma of the vulva followed, and it would appear that coitus, advanced life, and sterility in younger women have a distinct ætiological relation to the two affections. So, in my experience, have protracted vaginal purulent discharges, gonorrhœal or other.

In view of the possibility of this malignant change occurring, or of its recurrence, the landmarks for removal should be completely outside the diseased area. In severe cases, before resort to extirpation, where other remedies have failed, recent experience would encourage the use of the Röntgen-ray and radium treatment.

Operative Procedures in Contracted Pelvis.

There have been three very important original obstetrical communications made during the Session. I propose to group these together. The first was a remarkable paper by Dr. Hastings Tweedy, in which some rather novel propositions were ably advanced in discussing modern methods of delivery in contracted pelvis. The paper practically dealt with the relative indications for Cæsarean section, symphysiotomy, pubiotomy, and hysterotomy. I may group the principal conclusions he offered for discussion: (1) Pelvimetry by Skutsch's pelvimeter gives the most accurate pelvic measurements; the size of the fœtal head may be disregarded; Müller's manœuvre (pressing the head into the pelvis in deep anæsthesia) often fails in its purpose. (2) Induction of premature labour is never required, nor is prophylactic turning justifiable when employed as a measure to aid delivery. These procedures, with those of high forceps and perforation, are to be looked upon as "obsolete," and in a few years will be "viewed with abhorrence." (3) Cæsarean section is indicated in a pelvis with a conjugate between 3¼ in. and 2¾ in. in a healthy patient with suitable surroundings and early in labour; also, when the child is living in a pelvic conjugate of

$2\frac{3}{4}$ in. to $2\frac{1}{4}$ in. (4) If the membranes have ruptured for some time in a conjugate above $2\frac{1}{4}$ in., and the child be living, symphysiotomy (Bumm's) and pubiotomy are preferred. (5) These operations are also indicated when danger symptoms to mother or child arise during labour in a pelvis not below $2\frac{3}{4}$ in. in a true conjugate. (6) In a true conjugate of less than $2\frac{3}{4}$ in. when labour is advanced with ruptured membranes, and Bandl's ring is well up towards the umbilicus, Cæsarean section is unsuitable, and hysterotomy is the operation of selection. (7) Subcutaneous pubiotomy, the technique of which was fully described (not Döderlein's operation), is so safe in its technique that it may be performed by the general practitioner. (8) Perforation should never be performed in a case of contracted pelvis on a living child. (9) In the lesser degrees of contraction Walcher's position should be adopted.

The hysterotomy described and recommended by Dr. Tweedy is the extra-peritoneal Cæsarean section. The primary abdominal incision is transverse, from one anterior superior spine to the other, about 1 in. above the pubis, neither the peritoneal cavity nor the cellular tissue being exposed. He does not regard the presence of sepsis as a contra-indication.

As might be expected, these views gave rise to an animated discussion, in which several of the obstetric stalwarts in London, including Sir Francis Champneys, and Drs. Herman, Herbert Spencer, Eden and Griffith, took part. Such names show the weight of evidence brought to bear on the contested points. I may summarize the lessons to be drawn from the paper and the discussion: (1) Greater care generally should be taken in the early months of pregnancy to estimate approximately the pelvic capacity for labour at full term. In lying-in hospitals and maternities Skutsch's pelvimeter may well be employed, but for general purposes in ordinary practice the hand, properly used, is the most ready and reliable guide. (2) The time has come when the general indications for Cæsarean section, symphysiotomy, pubiotomy, hysterotomy, and the ordinary obstetric methods of dealing with contracted pelvis can be more exactly defined. (3) There is still a useful place for induction of labour, craniotomy or cephalotripsy, and high forceps application. While ever regardful of the life of the child, that of the mother takes precedence in determining our selection of the manner of delivery.

In connexion with this discussion I may refer to the Master's report of the Rotunda Hospital, recently issued. Dr. Tweedy certainly shows that the obstetric reputation of the hospital is worthily upheld.

Between the intern and extern maternities there were 4,163 deliveries, not including 203 abortions. Of the 1,963 intern cases, there were four deaths; and of the 2,200 extern, eight deaths. But of the four intern cases, one died in the last stage of phthisis pulmonalis; one was admitted with septic peritonitis and rupture of the uterus, with the fœtus half-way through the os; the third was a case of hydramnios, the anencephalous fœtus being dead on admission; the fourth appears to have been a death after spontaneous rupture of the membranes in hydramnios.

Of the deaths in the extern maternity, two had died before the physician arrived; none of the others died in labour. No death has occurred in the last twenty-six cases of true eclampsia. Only two operations for Cæsarean section were performed. There were eight cases of placenta prævia, and no death. Considerable stress, in the report, is laid on the importance of the careful differentiation of true eclampsia from other types of convulsion. No case in which there was a history of previous fits unconnected with pregnancy or labour, nor any which is free from albuminuria, has been included.

If asphyxia threatens, the plan of turning the patient on to her chest, with the head hanging over the side of the bed, is advocated, the manœuvre being followed by artificial respiration and administration of oxygen.

Cæsarean Section in Placenta Prævia.

The next communication is also from the pen of a foremost representative of the Dublin school. Dr. Jellett dealt with the all-important question, *Will Cæsarean section lessen the mortality of placenta prævia for the mother or the child?* Speaking from the statistics of the Rotunda Hospital for twenty years, and the confinements of 32,546 women, he showed that out of this number 138 had placenta prævia, the percentage mortality of the latter being 3·6, closely corresponding to the 3·1 mortality of Fry.[1] I collected statistics from the clinics of Professors Mangiagalli, Pinard, Bumm, and Dmitri Ott, which taken together include a period of thirty-six years of experience in some of the largest obstetric establishments in Europe. Adding the twenty years of the Rotunda Hospital to these, we find that in fifty-six years there were 666 cases of placenta prævia. The total mortality of 611 of these cases occurring in four of these clinics did not amount to 3 per cent. One

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[1] *Trans. Amer. Gyn. Soc.*, Philad., 1909, xxxiv, p. 36.

Cæsarean section (followed by death) and one Porro's operation were performed in these 611 cases. In the fifth clinic, Professor Bumm's, fifteen Cæsarean sections were performed in 179 cases, with a mortality of 6 per cent., the total mortality being 2·5 per cent. I do not here include Professor Krönig's statistics, in which 50 per cent. of the 40 cases of placenta prævia had Cæsarean section performed without any death, as I do not know the total number of deliveries or over what period of time the report spread. In the other twenty of his cases there was one death. Up to the time that Dr. Jellett's paper was read there was a record of one case performed in Scotland (that of Dr. Munro Kerr), and the only cases referred to in the discussion as occurring in England were those of Dr. Griffith and Dr. Gow. In Dr. Munro Kerr's case, which he brought before the Section, the placental presentation was central and was complicated by the presence of fibroids; and in that of Dr. Gow the presentation was also central and the cervix undilatable.

The lessons that must be drawn from the discussion on this communication are: that Cæsarean section has a place in the management of placenta prævia when (*a*) this complication occurs under circumstances that would in any case demand the operation; (*b*) when the character of the presentation and form of the attachment, with other conditions, render the classical methods of treatment impossible to carry out, or their practice more dangerous than operation. It would appear that the risk from placenta prævia to the mother, which must be our first consideration, has been much exaggerated by those who advocate the step. I have not referred to the relative influence on the child, as I consider the records bearing on this appear to be very variable and uncertain.

Cæsarean Section in Eclampsia Gravidarum.

The third communication in this group is that of Dr. Frederick McCann on "Cæsarean Section in Eclampsia Gravidarum." He was instigated to bring the subject forward by the successful issue of a case in which he performed the operation where there was no possibility of emptying the uterus, and when a fatal issue seemed imminent. The statistics quoted of previous operations were certainly not encouraging. Sir Halliday Croom appears to have been the only obstetrician who had performed the operation for eclampsia in the United Kingdom. In his two cases the operation was done under desperate conditions: the mothers died, one three days after parturition; one child was saved.

Dr. McCann suggested that the indications for the operation as a means of rapid delivery included: (1) the severity and rapidity of the fits; (2) the time of their occurrence, before labour or after its commencement; (3) if there be considerable disproportion between the head and pelvis; (4) an undilatable cervix; (5) a moribund mother and a living child. Bumm's exceptionally favourable results from Cæsarean section (reduction of mortality from 25 and 30 per cent.—surely a very high rate—to 2 to 3 per cent.) were quoted in support of the operation. The discussion virtually turned on these points: (*a*) the advisability of rapid emptying of the uterus; (*b*) does Cæsarean section under certain conditions lower the present mortality? (*c*) the relative advantages of the abdominal and vaginal routes. With regard to the first and second of these indications opinion was divided, but the leaning was rather to regard the rapid emptying of the uterus, though often indicated, as by no means universally desirable; still, that in given cases, as that of the author, it was justifiable to employ Cæsarean section.

An unbiased verdict would be, I think, that, in exceptional cases, Cæsarean section has a place in the treatment of eclampsia, and these are all included in the indications laid down by the author of the paper.

Two remarks I might here make. I do not consider that Bossi's dilator, properly applied, is the useless instrument that some who have never used it conceive it to be. Also, I believe that in a special type of case, particularly if a post-partum one, pilocarpine judiciously administered is a most useful agent. Certainly I have directly saved life by it on at least two different occasions.

Dr. Amand Routh, at the recent Congress in St. Petersburg, brought forward the whole subject of Cæsarean section, summarizing every obstetrical indication for this step, and illustrated his paper by the statistics of over 1,282 cases performed by living operators in the United Kingdom since 1867, 1,632 of these being for contracted pelvis.[1] From the details of 482 of these, he concludes that Cæsarean section should never be done if previous attempts to deliver have been made before the patient comes under notice. If the membranes have ruptured, and examinations have been conducted, special precautions should be taken before operation.

He does not favour the extra-peritoneal operation. Of 1,254 cases operated upon since 1890, the total mortality has been 11·6 per cent: but from the statistics of the last five years he places the present total mortality from Cæsarean section at 8·1 per cent., though in favourable cases for contracted pelvis it is only 2·3 per cent. Improved methods of technique, added to early recognition of indications, have reduced the mortality so as to bring the step much more frequently within the range of justifiable obstetric procedures in parturition. His conclusions with regard to the performance of the operation in placenta prævia are practically those arrived at in the discussion on Dr. Jellett's paper, and, virtually, that resort to this step in eclampsia is only justifiable on the grounds advanced by Dr. McCann.

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[1] *Zentralbl. f. Gynäkol.*, Leipz., 1910, xxxiv, p. 1446.

Professor Bumm, at the Congress, summarized the relative advantages and disadvantages of the supra-symphysic and the classical operation: in the former, the avoidance of the peritoneum and the easier suture of the cervix; on the other hand, greater difficulty of performance, greater risk to the bladder. the delivery of the fœtus more difficult, and certain disadvantages in the cicatrix, leading to displacement of the uterus and rendering a second operation more difficult.

Pestalozza considers the classical Cæsarean section preferable when asepsis is certain, and early in labour. When there are septic complications he advocates embryotomy and, in extreme degrees of contraction, Porro's operation. He adopts Fritsch's transverse incision of the fundus in the classical operation, and in the extra-peritoneal Latzko-Döderlein's technique. He deprecates resort to the operation in eclampsia, and only very rarely considers its performance justifiable in placenta prævia.

Sellheim prefers the transverse incision of Pfannenstiel when haste is not called for, though he recognizes its greater difficulty in technique. He is more conservative in his views, believing that, with improved management of labour, resort to operation will be less frequent, and that, if infection is prevented, no operation is warrantable to save the life of the child that is dangerous to the mother.

Malignancy in Ovarian Tumours.

Mrs. Scharlieb brought before the Section an analysis of 150 cases of ovarian tumour, out of which 25 exhibited various types of malignancy. Altogether, including returns sent to myself by Professors Schauta, Jacobs, Winter, Schottländer and Mangiagalli, the proportion of malignant tumours of the ovary was shown to be 350 out of 3,481 —that is, 9·9 per cent. Though it is not possible to say exactly, still there can be no doubt that a fairly large proportion of such malignant growths were primary, many, however, being metastatic. The discussion made it clear that much has yet to be done regarding the ætiology and differentiation of malignant tumours of the ovary and their classification.

Dr. Arthur Wallace, in a contribution on "The Life History of Uterine Fibro-Myomata," made an exhaustive analysis[1] of the signs and symptoms, local and constitutional, in ten women in whom disappearances or shrinking of the tumour had occurred to a remarkable extent. These clinical histories only served to convince Dr. Wallace "that palliative treatment was inapplicable when fibroids caused symptoms and the condition of the general health justified operation." It is notorious that some women enjoy fair health and suffer little from a myomatous tumour; others eke out an existence for years, and live a life of chronic invalidism from various causes. But with the great majority life ends prematurely in pain and misery, from some of the many complications that attend on the growth. The myth of a curative menopause has disappeared, and the statistics of the authorities quoted by me in the discussion, including reference to the records of the results of examination of some 6,000 myomatous tumours, prove that danger to life is increased directly in proportion to the advance in years after from forty to fifty.

Ruptured Tubal Pregnancy.

Mr. Carwardine's epidiascope demonstration of microphotographs from a case of *ruptured tubal pregnancy* beautifully illustrated the cell processes of the syncitium and the diversity in the character of the epithelial covering of the villi. An interesting point brought out was that pathological appearances seen in the nuclei arising from the syncitium had a significant resemblance to the correlated structures seen in chorio-epithelioma.

Complications of the Puerperium.

Sir W. Japp Sinclair's paper had its use for those in general practice who, either through ignorance or carelessness, are inclined to overlook or lightly treat some of the common post-partum complications of pregnancy or the abnormal uterine and pelvic conditions which are present during the puerperium. He took rather a pessimistic view of the advance of modern obstetrics. If Ould, Chamberlain, or "Big-paw" Smellie were appointed visitors to the Rotunda, Queen Charlotte's, or the Glasgow Maternity, by a celestial medical council, I fancy they would report that the art of the eighteenth century had not stood still, but had been elevated into a science. They might even console the latter-day sojourners for the loss of their practice in midwifery by telling them that even in this corner of the "mournful star" the labours of Braxton Hicks, Barnes, Simpson, Hamilton, Duncan, Denman, Beatty, McClintock and others, were still bearing fruit.

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[1] Vide *Proceedings*, 1910, iii, p. 208.

His remarks elicited from Sir Francis Champneys a notable defence of the modern midwife. He showed statistically that since the Midwives Act had come into operation there had been a considerable reduction in maternal and infant mortality in cases attended by those who were qualified by the Midwives Board.

I should like to emphasize one point arising out of Sir W. Japp Sinclair's paper—that we should clearly distinguish between those cases in which ventro-suspension of the uterus is indicated rather than ventro-fixation. The latter method should, in my opinion, never, save under exceptional circumstances, be adopted during the child-bearing period of life.

Clinical Histories.

We turn now to the clinical histories which were brought before the Fellows. Mr. Herbert Paterson's case of *rupture of the uterus, treated by suture*, applied *per vaginam*, with drainage, was instructive. It proved the possibility of successful treatment of the utero-vaginal rent as an alternative to hysterectomy, and the value of proctoclysis which was continued for sixty hours after the operation.

Dr. Philip Turner's *hysterectomy, performed sixty-three hours after labour*, was very interesting. The justification of the decision not to interfere, after an exploratory incision had been made to determine the nature of the growth at the end of the second month, was established, and the correctness of the prompt performance of total hysterectomy, when the signs and symptoms of sepsis arose after labour, was clear. Here it is important to note that the infection was due to necrosis of the subperitoneal fibroid.

In connexion with this case reference must be made to that of Dr. Russell Andrews, of *myomectomy for an intramural fibroid in the third month of pregnancy*. Here the enlarged retroverted uterus could not be raised out of the pelvis. Pregnancy went to full term, and a living child was born.

Hæmatocele.

Dr. William Alexander showed how an exceptionally *large hæmatocele might complicate the presence of a uterine fibroid* without any traumatism having occurred. The extra-peritoneal collection of blood in this case was enormous. It had occurred before operation, and its source was uncertain. Aseptic precautions, with drainage after removal of the uterus, resulted in cure.

A very important communication was that of Dr. Cuthbert Lockyer, on *pelvic hæmatocele.* Two cases of Mr. Stanley Boyd and one of Dr. Amand Routh served to illustrate the occurrence of an ovarian hæmatocele apart from ectopic gestation. The liability to mistake lutein hæmatoceles for ovarian gestation is obvious, and this error occurred in Dr. Routh's case and in one recorded by Dr. Giles. It is clinically interesting to note that the symptoms in both of Mr. Stanley Boyd's patients closely simulated those of appendicitis.

Specimens.

In alluding to the various specimens exhibited, we may divide them under the heads of *uterine*, *ovarian*, *tubal*, and *vaginal*. We may thus summarize those which more especially had a pathological importance.

In the necrobiotic fibroid incarcerated in the pelvis that complicated pregnancy, Dr. Stevens showed that there was extensive thrombosis of the veins of the capsule; hysterectomy having been successfully performed by Dr. Eden.

A perfect example of obstetric surgery was afforded in the successful dual issue to mother and child of Dr. Herbert Spencer's *posterior Cæsarean section followed by hysterectomy for fibroids.*

The operation was performed in the thirty-eighth week, and, the 7 lb. living fœtus with the placenta having been extracted, the degenerated and greatly thickened uterus was removed. In Dr. Griffith's case the large myomatous tumour which grew from the posterior wall of the uterus, and distended the vagina, was removed with safety, during labour at full term, to mother and child.

Dr. Lewers's specimen of a *cystic tumour of the right broad ligament springing from the uterus*, and apparently developed from Gartner's duct, was believed by Mr. Doran to be homologous with Untenberger's tumour,[1] which was undoubtedly of Gartnerian origin.

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[1] *Monatschr. f. Geburtsk. u. Gynäkol*, Berl., 1909, xxix, p. 587.

Complete rupture transversely of the fundus uteri is rare. In the uterus shown by Dr. Lewers there was no degeneration, and nothing to account for the transverse rupture, unless, as was surmised by Dr. Maxwell, it might be explained by a rather unusual arrangement of the muscle fibres parallel to the place of rupture.

Mr. Targett's specimen of *keratoid degeneration associated with columnar-celled carcinoma of the uterus* was of pathological interest, as was the other specimen he submitted, as exhibiting the very rare occurrence of malignancy in a uterine adeno-fibromatous polypus. Though this was not altogether substantiated by the Pathological Committee, it was at least on the borderland of malignancy. I am indebted to Professor Schottländer for drawings which I have just shown of the sections of a cervical polypus in which the malignant change was sarcomatous.

The rarity of malignant change in complete prolapse of old standing was illustrated by the discussion on Dr. Russell Andrews's case, in which there was also a contact carcinoma of one labium.

The unusual occurrence of a *spheroidal-celled carcinoma of the cervix* in a case of Dr. Griffith's was demonstrated in the specimen exhibited by Mr. Barris.

A giant fibroid of the cervix, weighing 23½ lb., was removed successfully by Dr. Giles. My largest tumour, which sprang from the cervix. and broad ligament, weighed 28½ lb. The patient made a permanent recovery.

The advantage of systematic microscopical examination of growths after removal was evidenced in the specimen of sarcoma shown by Dr. Jellett. The growth was supposed at the time of operation to be a simple myoma. The same lesson was taught by the post-operative examination of Mr. Ernest Shaw's dermoid cyst of the ovary, which proved to be a squamous-celled carcinoma. In the highly suspicious specimen that I exhibited, all clinical symptoms and macroscopical appearances pointed to sarcoma. Dr. Lockyer proved its benignity.

The accident of a uterine or ovarian tumour, or even an ectopic gestation, being mistaken for displacement was apparent from the history of the patient who came under Dr. McCann's care. She had worn a ring pessary for thirteen years. The tumour was a degenerating necrotic myoma of the cervix, which involved the vagina. The rather uncommon complication, torsion of the uterine tumour, was attended in Dr. Griffith's patient by morning pains, sweating, and vomiting. There did not here appear to be any definite cause for the torsion, such as an ovarian growth or pelvic deformity. An habitually loaded rectum might possibly act on the direction taken by the growth.

Malformations.—Dr. Russell Andrews performed hysterectomy on a woman five months after the death of a fœtus at full term. The tumour proved to be the enlarged uterus, and the pregnancy occurred in one half of a septate body with a single cervix, the ovum possibly reaching the cavity, as suggested by Dr. Maxwell, "by external migration."

Dr. Lewers's specimen of atresia in one half of the double uterus proved how such a condition might remain an undiscovered cause of dysmenorrhœa.

Ovaries.

Some affections of the ovaries of considerable pathological interest were brought before the Fellows.

Giant Adeno-carcinoma.—The *rare* occurrence of a disagreement among pathologists was exemplified in the report on Dr. Giles's specimen of giant adeno-carcinoma, Drs. Giles and Cuthbert Lockyer, in a minority report, adhering to their original opinion that the characters in the grouping of the cells denoted malignancy, while the other members of the Committee decided adversely to this.

Primary carcinoma of the ovary was illustrated by Dr. Garrett Anderson. Dr. Tate's *myxo-fibromatous ovary*, removed under spinal anæsthesia, demonstrated the advantage of this method in a case of the kind where serious lung complications prohibitory to general anæsthesia were present. The same remark applies to the case of Dr. Inglis Parsons, where, in consequence of advanced cardiac disease, spinal anæsthesia was resorted to in an extensive colpo-perineorrhaphy.

Mr. Barris and Dr. Williamson ascribed an abscess in an ovary removed by Dr. Williamson to the presence of *Amœba coli*, similar cells with amœboid movements being found in the fæces.

Dr. Hastings Tweedy added another instance to those already recorded of undoubted *ovarian pregnancy*. Mr. Ernest Shaw brought an example, rare in occurrence, of *actinomycosis of the ovary*, secondary to appendical suppuration. Mr. Victor Bonney showed an intestinal *implantation teratoma*, secondary to a cystic teratoma of the right ovary, which had been removed, the left ovary being healthy.

Malignant degeneration of dermoid cysts of the ovary is a subject of great interest, clinically and pathologically. Indeed, it may be said that the transitional phases in all forms of ovarian cystomata, from the benign type of cell to the malignant, are but indifferently understood.

The specimen removed by Dr. Blacker, exhibited by Mr. Ernest Shaw, of a squamous-celled carcinoma of a dermoid cyst of the ovary, was an example of such degeneration. This and Mr. Targett's case proved the advisability of careful microscopical examination of such cysts. I have a very large dermoid cyst which I removed, where there was at the time no suspicion of malignancy, though the woman died within a year of malignant metastases to the bowel.

Fallopian Tube.

"*Papillary Columnar-celled Carcinoma* of the Fallopian Tube" conveyed the decision of the Pathological Committee on Dr. Tate's specimen, and the discussion on it, with instructive observations by Mr. Alban Doran, disclosed the fact that since Orthmann described the first case in 1888 just a hundred others have been added.

The extreme importance of early operation in *tuberculous salpingitis* was emphasized in the remarks made by Mr. Paterson. Three instances of permanent recovery from primary tuberculous invasion had been previously brought by me before the Section.

Tubal Pregnancies.

Mr. Hey Groves exhibited a *tubal mole* with a normal fœtus inside the tubal cavity. Menorrhagia was the only symptom present before rupture in Dr. Aarons's case of tubal pregnancy. There were some interesting particulars disclosed at the operation in Dr. McCann's case of *tubo-abdominal pregnancy* in the triple attachment to the uterus, the floor of the pelvis, and the sigmoid colon, with a special increase in the vascular supply to the placenta through the meso-sigmoid.

Dr. Drummond Maxwell, through the specimen (obtained from Dr. Lewers's patient) of *molar pregnancy*, showing extensive œdema of the connective tissue of the villi, introduced the questions of repeated tubal pregnancy and the dependence of tubal gestation on inflammatory changes in the tube. He also emphasized the very practical point of the difficulty in diagnosis often experienced between these two conditions.

Dr. Glendining found a justification for the unusual term "*tuberculous adeno-myoma*" in the rare combination of adeno-myomatous degeneration with tuberculous invasion in a Fallopian tube (removed by Dr. Inglis Parsons).

There were two interesting *vaginal specimens*. Dr. Russell Andrews showed a fibroid of the vaginal wall the size of a cricket ball; Dr.

Stevens supported his view that adeno-myomata of the vaginal wall have a Wolffian and not a Müllerian origin, by microscopical sections of a tumour of this character.

From this résumé I think you will agree with me that the work accomplished in the last session has been worthy of the Royal Society of Medicine. The credit is entirely due to those Fellows whose names I have mentioned, and to those who have so ably and frequently contributed to the success of the meetings by their presence and the part they have taken in the discussions. I cannot sufficiently express to them my thanks for the support they have given me in the chair. May I venture to hope that the same enthusiasm and scientific zeal may be seen in the present session, and that the *Proceedings* of the Section will be enriched with original contributions as valuable as those which have already filled their pages.

Adenomatosis Vaginæ: a hitherto undescribed condition.

By Victor Bonney, M.S., and Bryden Glendining, M.S.

We venture to bring forward the following case illustrating a condition of which we can find no account in medical literature, and of which both the symptoms and pathology are remarkable.

E. H. was seen by Mr. Victor Bonney in the out-patient department of the Chelsea Hospital for Women in February, 1910. She was aged 42, and had had five children, the last being eight years ago. The confinements were stated to be normal. Menstruation began at the age of 13, and had always been regular, the loss lasting from four to six days. For twelve years she had suffered from a continual vaginal discharge of a transparent sticky nature. It began insidiously, and had been increasing ever since. For three years she had noticed a lump in the right groin. She complained of no pain, but the continual discharge had produced soreness of the external parts. For the last four years she has had to wear a diaper continually. She was a healthy woman in appearance, and nothing abnormal could be found except in the parts complained of.

The vagina presented the peculiar appearance illustrated in the accompanying plate. The entire mucosal surface, including the vaginal surface of the cervix, was of a singular red colour, and had a granular, finely honeycombed texture. It was studded all over with small cysts from the size of a pin's head to that of a large pea, and from it poured a continuous stream of mucous fluid like the white of a hen's egg. The lump in the groin was fluctuant, and suggested a lipoma, a hydrocele of a hernia sac, or some cystic condition of one of the inguinal glands.

She was taken into the hospital, and the amount of discharge lost *per diem* was measured at six fluid ounces, the specific gravity being 1014. Under an anæsthetic Mr. Victor Bonney further investigated the condition of the vagina and vaginal cervix, and removed portions for microscopical examination. The uterus and appendages were healthy. The lump in the groin was cut down upon and proved to be a hydrocele of the sac of a femoral epiplocele. The omentum contained in the sac was cut away, the sac removed, and the femoral ring was closed by deep sutures. She left the hospital with the wound in the groin healed, but the vaginal condition unaltered.

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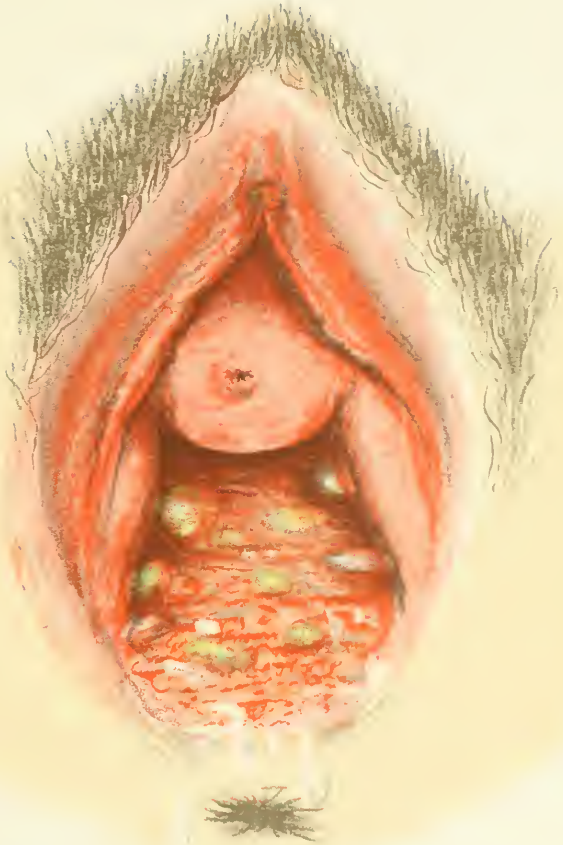
Section)
Vol. III No. 7



BONNEY & GLENDINING: Adenomatosis Vaginæ

PATHOLOGY.

The material removed from the vagina and vaginal surface of the cervix consisted of five separate pieces of different sizes. Three of these were fixed in acetic alcohol, and two in formol-Müller. They were then embedded in paraffin, and cut at right angles to their vaginal surface. All the sections present practically the same appearance, which is as follows:—

The vaginal surface exhibits a poorly stratified epithelium, reduced in many places to the thickness of a single cubical cell. The tissue of the vaginal wall underlying it consists of a fibro-muscular stroma in which the muscular elements are predominant though ill-formed. Imbedded in this stroma, and inwards throughout a large proportion of the thickness of the section, are numerous glandular spaces. These glands are everywhere lined by a single layer of tall columnar epithelium, actively mucinogenetic, as proved by their reaction to mucicarmine. They are small and most discrete immediately under the epithelium, being here separated from one another by considerable masses of the stroma, whilst deeper down they become exceedingly numerous and large, so that over considerable areas practically little interglandular connective tissue is seen. With increase in size their form becomes irregular, owing to infoldings of the wall, which appear in section as

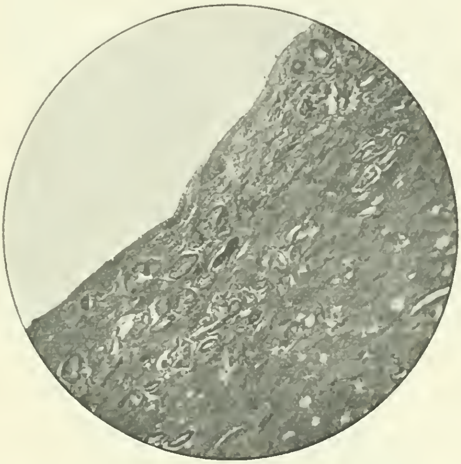


FIG. 1.

Section of portion of vaginal wall, showing disseminated glands in wall. (Obj. 1 in.)

simple papillary processes. Some of them have been converted into retention cysts. Here and there the communication of these glands with the surface can be demonstrated, the columnar cells lining the ducts of exit becoming continuous with the superficial squamous or

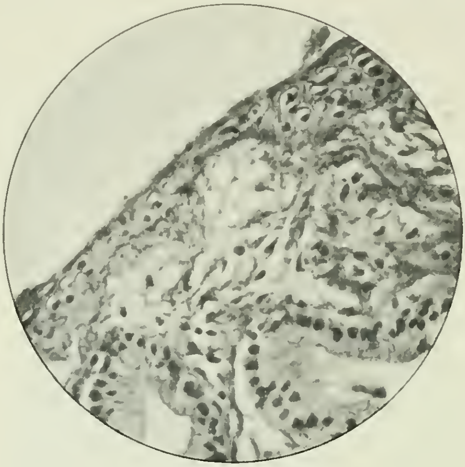


FIG. 2.

Glands subjacent to epithelial surface. (Obj. $\frac{1}{6}$ in.)

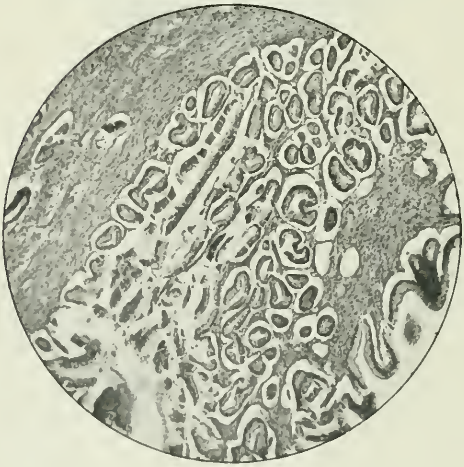


FIG. 3.

Section deep to the epithelium, showing adenomatous condition of glands in vaginal wall. (Obj. $\frac{1}{6}$ in.)

short cubical epithelium. At these points all gradations between the two types of cell can be traced.

The surface epithelium at intervals exhibits small solid downgrowths, which here and there appear to be excavating to form glandular crypts.

The cellular elements of the interglandular stroma are chiefly ordinary connective-tissue cells, with an oval or spindle-shaped nucleus. No plasma cells can be demonstrated by Pappenheim's or Bonney's staining methods, and very few lymphocytes and no polynuclear leucocytes are present.

Weigert's stain reveals complete absence of elastic fibres under the surface epithelium, but it is present in about normal proportion around the vessel walls. Its connexion to the glands varies. Some of them have a remarkably complete and well-formed envelope, whilst others in immediate proximity to them show complete absence of any such investment.

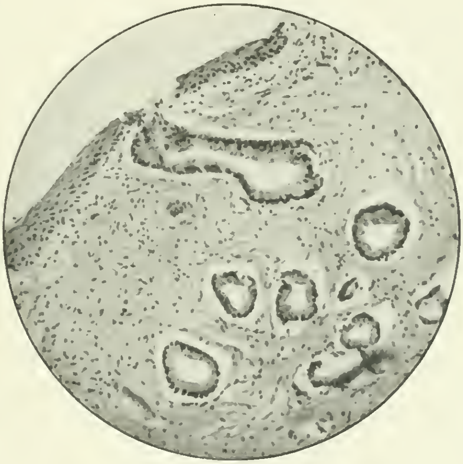


FIG. 4.

Section of vaginal surface, showing gland opening externally. (Obj. $\frac{1}{6}$ in.)

HISTORICAL NOTE.

Medical literature contains only few serious articles dealing strictly with cystic affections of vagina.

In February, 1867, Dr. Hall Davis read a paper before the Obstetrical Society of London on a cyst of the vagina, which is recorded in that Society's *Transactions.*[1] It was of the size of a pear, contained an ounce of olive-coloured fluid of the consistency of mucus; it presented at the vagina and was mistaken for prolapsus. It was removed, as after previous incision it had filled up again within a week. He quotes Dr. West as having twice seen cysts in the vagina, one on the posterior and the other on the anterior vaginal wall, each the size of a chestnut and containing the same mucoid fluid. He notes, too, that he had also encountered small cysts containing transparent fluid and with thin translucent membranous wall. Upon their pathology he quotes Virchow, who attributes their origin "to obstruction of the follicles of the vaginal wall."

In the *Transactions of the Obstetrical Society* for 1891 there is a paper read by Dr. Rutherford in which cysts of the vagina are classified and all recorded cases previous to that date collected. The nomenclature is probably faulty, from the fact that in many of the cysts the epithelial lining had not been properly fixed, and consequently was not found in the microscopic section. This has led to undue augmentation of the class of cysts ascribable to injury, or blood cysts.

In 1877 von Preuschen[2] published a description of glands found in the vagina which do not appear to have proliferated, as in our case. Briefly, the case recorded by him was that of a woman who had a cyst of the vagina which was removed, and proved to be a cyst lined by columnar epithelium. Six years later the patient died from some intercurrent malady, and he was fortunate in securing the vagina post mortem. He prepared sections and made drawings from them, which are equal to the best productions of to-day. They depict typical mucous-secreting glands opening upon the squamous surface, which are exactly comparable to certain sections of the vaginal wall in the microscopical specimens shown to-night. It is surprising that the possibility of the existence of glands in the vaginal wall should have received so little attention. Von Preuschen's paper leaves little doubt in the mind of the reader that he is describing a condition of glands in the vagina previous to hyperplasia. Clinical details are not given.

A discussion of the nature and origin of the condition described involves the consideration of certain possibilities, as follows:—

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[1] *Trans. Obstet. Soc. Lond.* (1867), 1868, ix, p. 32.

[2] *Archiv. f. Path. Anat. u. Phys.* (*Virchow*), Berl., 1877, lxx, p. 111.

(I) MÜLLERIAN DERIVATION.

In this it is suggested that the rudiments of the glands were laid down in the course of the embryonic evolution of the vagina, and that from them the diffuse adenomatous condition was in later life derived by glandular proliferation.

In considering this possibility the development of the vagina must be referred to. In sections of a female fœtus at or about the termination of the third month, the two Müllerian ducts are found to have fused in their lower half to form a single and common canal, which is the uterus and vagina. The canal may be traced into the urogenital sinus. At a later date the lower half of this common canal is found filled with a mass of squamous epithelium, and the wall has developed longitudinal folds. It is then definitely recognizable as the vagina. In this and in the cervix no glands are recognizable at birth. In the uterus there may be diverticula, and folds of the single layer of columnar epithelium which are the first step towards this formation. In the first to the fifth year after birth glands are found in the cervical canal.

Now it seems not unreasonable to suppose the extension of this process to a position lower than usual, as an accident in the evolutionary cycle might occur. It is to be remembered that the uterus, cervix, and vagina are developed from the same structure, that they are but differentiated portions of the Müllerian ducts, and that the line of demarcation of one portion from another, microscopically unrecognizable in the early stages, might easily be shifted up or down.

In the normal vagina no glands ordinarily exist, but microscopically small spaces lined by a layer of cubical cells, and containing granular matter, are often found in the squamous epithelium. It is from these intra-epithelial spaces that the small pea-like cysts not uncommonly found in the vaginal wall are probably developed. Sections from the case under consideration also show these spaces.

The history of the case, however, shows that, though the rudiments of glands may have been present at birth, their excessive development only took place long after adult life had been entered on.

(II) DERIVATION BY METAMORPHOSIS, INFLAMMATORY OR REVERSIONARY.

It is possible that the condition marks a reversion of the stratified squamous epithelium of the vagina to a glandular type, in virtue of propensities inherited from the cells lining the Müllerian ducts. That such metamorphosis is possible is proved by a study of cervical erosion, in which many new glands are formed by downgrowth from the previously squamous and glandular epithelium for some way around the external os. These new glands are formed by the excavation of ingrowths from the epithelium, and it is an interesting point as to why the process should be limited, as it always is, to a comparatively small area around the external os. It may be suggested that the gland-forming power of the epithelium, actively exercised in the development of the cervical canal, extends for some way beyond the external os, and, though normally held in abeyance there, is developed under the stimulus of chronic inflammation.

The absence of elastic tissue from the interglandular stroma, in the sections from our case, strongly suggests old inflammatory change, but whether this was the primary event, as in cervical erosion, or merely secondary to the glandular hyperplasia cannot be decided. Certainly the patient had none of the ordinary signs of chronic vaginitis.

The likeness to the glandular phase of erosion of the cervix is, however, very close. The glands, like those of the erosion, are muciniferous, as proved by their reaction to mucicarmine, and differ totally both in this respect and in the shape and character of their cells from the corporal glands. Further, the same loss of surface keratinization and tendency for the epithelium to expend its energy in downgrowth rather than in upgrowth is present as in cervical erosion.

(III) Wolffian Derivation.

The derivation from elements of the Wolffian tubules present in the vaginal wall is negatived on three counts: firstly, that the condition extends over the entire vaginal surface—i.e., in positions where no vestiges of these ducts are found; secondly, that direct communications with the lumen of the vagina by narrow ducts are seen; thirdly, that the glands are truly muciniferous, as proved by the character of their discharge and their staining reaction to mucicarmine, whereas the epithelium lining the Wolffian ducts and all derivatives from them is not muciniferous, but secretes a watery fluid, as exemplified by the contents of a parovarian cyst. (The authors here showed illustrations of a cyst of Wolffian origin in which the Wolffian body appeared in intimate relation with the cyst and enclosed in the same musculature.)

CONCLUSIONS.

A careful consideration of these possibilities leads us to believe that the theory best supported by evidence and likelihood is that which assumes that in this patient a congenital peculiarity of the cells lining the vagina existed, in virtue of which they possessed facultative gland-forming powers similar to those covering the vaginal cervix in immediate proximity to the external os; and that this power lay dormant until activated by an excitant, probably inflammatory and connected with child-bearing, the process being analogous to that by which a glandular erosion of the cervix is formed.

DISCUSSION.

Mr. ALBAN DORAN could not feel certain whether Mr. Glendining considered that the cyst-like spaces in his sections had developed in the Wolffian body or in its duct. As they lay in close relation to the vagina, Mr. Doran concluded that they were dilatations or offshoots of the duct. The parovaginal portion of the Wolffian duct as seen in *Bos* and *Ovis* was that to which alone the name of Gartner should be given. Mr. F. Balfour and Dr. A. Keith had recently warned anatomists who felt too certain about the homologues of the Wolffian and Müllerian ducts. The fimbriæ and ostium of the Fallopian tube, with "Kobelt's tubes" and other annexes of the parovarium, were, in their opinion, derived from the pronephros seen in the shark. The "parovarian cyst," they further maintained, had nothing to do with the parovarium or Wolffian body, but arose in a cystic structure, the homologue of the rete testis. There were yet greater difficulties and sources of fallacy, Balfour and Keith maintained, about the authentication of the homologues of paravaginal cysts. Hence Mr. Glendining's researches, where nothing was taken for granted, were important as throwing light on a very dark subject.

Dr. GRIFFITH remarked on the great interest of this curious disease, especially on its possible relationship to the extremely common and little understood condition known as erosion of the cervix. He was satisfied, from his own observations, that an erosion has no necessary dependence on inflammation, but that it is a developmental variation—a local and clearly-defined replacement of a portion of the vaginal cervix by adenomatous tissue similar to that found in the cervical canal. A difference between it and the condition under discussion seemed to be that the adenomatous growth replaced the epithelium and the papillary connective tissue in the case of the erosion, whereas in this case the microscopical sections showed epithelium and connective tissue, though thin, still intact, over the glandular growth.

Some Observations on Vaginal Secretion in Infants.

By G. Drummond Robinson, M.D.

The series of observations on which the following paper is based was commenced in 1889 at a time when the acidity or alkalinity of the secretion found in the vagina was thought, by some, to be of considerable pathological importance. From various causes the series was not completed until views had considerably changed on this subject, and at the present moment the question possesses little more than a physiological interest.

It is generally admitted that in the majority of cases in the adult the reaction of the cervical secretion is alkaline, whilst that of the secretion found in the vagina is acid. Döderlein thought that the normal vaginal content is originally, like the secretion of the cervical canal, alkaline in reaction, and that its acidity in the adult is due to the action of micro-organisms which find their way into the vagina. He described a "vaginal bacillus" which he believed to be the cause of the acidity of the vaginal secretion of the adult, and he distinguished between a "normal" acid and a "pathological," neutral, or alkaline secretion. Gow, in his paper on "Vaginal Secretion," read before the Obstetrical Society of London in 1894,[1] showed that the secretion of the vagina, after total extirpation of the uterus, is acid in reaction, and he recorded an experiment which led him to write as follows: "This experiment points strongly to the conclusion that the secretion of the vagina is really alkaline, and that the acidity normally found to be present depends on the subsequent decomposition of the material secreted. This experiment would also seem to show that the acid formation is the result of the presence and growth of micro-organisms."

It seemed to the writer that light would be thrown on this subject by observations made on the sterile vaginal secretion of infants. Accordingly, fifty female infants were examined at the British Lying-in Hospital, and the results of the examinations are here recorded. A small glass tube was passed to the top of the vagina after the manner of a Ferguson's speculum, and through this a wire, bearing on its end a small portion of cotton wool, was inserted, and after the wool had been pressed against the vaginal walls it was withdrawn and the secretion adhering to it was examined. The tube, wire, and wool were previously sterilized by heating them in an autoclave (superheated steam). An attempt was made in all cases to sterilize the vaginal orifice. This was considered important, as it has been shown by Whitridge Williams and others how easily, in the adult, the micro-organisms of the vulva may be carried up on the end of a speculum, and so vitiate the results of bacteriological observations on the secretion found in the vagina. The narrowness of the vaginal orifice in infants would make this method of contamination even more common in them than in adults.

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[1] *Trans. Obstet. Soc. Lond.* (1894), 1895, xxxvi, p. 52.

The labia were separated, and the vaginal orifice and surrounding parts were thoroughly washed with soap and water and then with a solution of biniodide of mercury in methylated spirit (1 in 1,000). A piece of sterile cotton wool was next soaked in the alcoholic solution of biniodide of mercury, and pressed against the vaginal orifice for five minutes. The parts were finally thoroughly washed with a watery solution of biniodide of mercury (1 in 1,000).

It was found that if before the above procedure was employed the vaginal orifice was touched with sensitive litmus paper (such as is used for bacteriological purposes) an "acid reaction" was practically always produced. Whether this "acid reaction" was due to the normal secretion of the parts or to the presence of urine was not ascertained. Lest any of this acid secretion should be carried up into the vagina on the speculum, the parts—after an attempt had been made to sterilize them in the manner already described—were thoroughly treated with a strong sterile solution of bicarbonate of soda, which was finally washed away with sterile water before the observation was made.

It was at first thought that any but the very mildest measures to insure sterilization of the parts at the vaginal orifice would produce inflammation in the delicate tissues of infants, and the strength of the biniodide of mercury solutions and the vigour of the washing were only very gradually increased, as it was found by experience that no harm was done by this very thorough treatment. In two of the earlier observations (Nos. 31 and 37 in tables), when it was feared to employ too energetic measures to insure sterilization of the parts at the vaginal orifice, organisms were cultivated from the secretion removed from the vagina sixty-six hours and five days after birth respectively, while later observations conducted on the same cases on the thirteenth and seventh day respectively, after more thorough attempts at sterilization had been made, proved that the secretion was then sterile. This seemed to show that the organisms cultivated at the time of the first observations had really been introduced from the vaginal orifice on the glass speculum, and emphasized the importance of attempting to obtain sterility of the parts in this region before making culture experiments. The external genitals were found to be sterile for some hours after birth, but in from twenty-four to forty-eight hours many organisms (which had possibly come from the water used for the first bath) were found in this region. Cultures were taken from the vaginal secretion in broth, on gelatine and agar-agar, and also in and on the media (containing sugar) specially recommended by Döderlein for the cultivation of his "vaginal bacillus."

Repeated observations showed that the vagina was sterile during the first forty-eight hours of life. In only one instance, in a child aged less than 48 hours, was any organism found in the vaginal secretion. In this case, that of a child aged 36 hours (No. 25 in the table), a yellow staphylococcus, which liquefied gelatine, was cultivated from the secretion, but the vaginal orifice was so small that the introduction of the glass tube was a matter of difficulty, and to prevent the introduction of organisms from the vulva seemed to be almost an impossibility. As the age of the child examined increased, organisms were more frequently cultivated. In no case was Döderlein's bacillus found.

Sterile secretion was found (in one instance) as late as the thirteenth day, and it seems probable that in all the cases examined the vaginal secretion was really sterile, and that when organisms were found in it they had been carried up into the vagina on the speculum. A reference has already been made to two cases in which the secretion examined on the third and fifth days was not sterile, but that examined on the thirteenth and seventh day, was sterile. Probably during the first observations organisms had been inserted on the speculum. It is common knowledge that the sterilization of any easily accessible skin surface—such as that of the hands—is no easy matter, and the sterilization of the skin of the vulval region, with its numerous folds and depressions, is obviously still more difficult.

In all cases the reaction of the secretion was tested with sensitive litmus paper, such as is used in bacteriological work. The results are recorded in the following tables (*see* p. 29).

An analysis of the above table gives the following results: *Acid* vaginal secretion was found in 42 out of 50 cases—i.e., in 84 per cent.; this secretion was *strongly* acid in 23 cases, acid in 17 cases, and *faintly* acid in 2 cases. *Neutral* vaginal secretion was found in 3 cases—i.e., in 6 per cent. *Alkaline* vaginal secretion was found in 5 cases—i.e., in 10 per cent.; this secretion was *strongly* alkaline in 1 case, alkaline

No.	Age of child	Reaction to litmus paper	Remarks
1	30 minutes	Strongly acid	Sterile
2	3 hours	,, ,,	,,
3	3 ,,	,, ,,	,,
4	3½ ,,	,, ,,	,,
5	5 ,,	,, ,,	,,
6	5½ ,,	Acid	,,
7	7½ ,,	Strongly acid	,,
8	7½ ,,	Acid	,,
9	8 ,,	Strongly acid	,,
10	8 ,,	,, ,,	,,
11	10 ,,	Acid	,,
12	10 ,,	,,	,,
13	12 ,,	Strongly acid	,,
14	14 ,,	Acid	,,
15	14½ ,,	Strongly acid	,,
16	17 ,,	,, ,,	,,
17	17 ,,	,, ,,	,,
18	20 ,,	Acid	,,
19	24 ,,	,,	,,
20	24 ,,	Strongly acid	,,
21	24 ,,	,, ,,	,,
22	24 ,,	Acid	,,
23	28 ,,	Strongly acid	,,
24	36 ,,	,, ,,	,,
25	36 ,,	Acid	Yellow staphylococcus, liquefied gelatine; orifice very small
26	44 ,,	Strongly acid	Sterile; secretion abundant
27	45 ,,	,, ,,	Sterile
28	48 ,,	Acid	,,
29	48 ,,	,,	,,
30	50 ,,	,,	,,
31	66 ,,	Strongly acid	White staphylococcus, not liquefying gelatine: second culture ten days later sterile
32	3 days	Faintly acid	Non-sterile; second culture ten days later non-sterile
33	3 ,,	Acid	Sterile
34	3 ,,	,,	,,
35	3 ,,	Strongly acid	No observation
36	4 ,,	Acid	,, ,,
37	5 ,,	,,	Non sterile; second culture two days later sterile
38	5 ,,	Strongly acid	Sterile
39	5 ,,	,, ,,	Non-sterile
40	5 ,,	Faintly acid	Sterile
41	8 ,,	Strongly acid	,,
42	14 ,,	Acid	No observation
1	1·3 hours	Alkaline	Sterile
2	2·8 ,,	Faintly alkaline	,,
3	3·48 ,,	Strongly alkaline	Sterile: about ʒi mucus from vagina, strongly alkaline; two days later still much mucus, but now tinged with blood; blood lasted three days; on twelfth day still much alkaline mucus
4	4·48 ,,	Faintly alkaline	Sterile
5	5·5 days	,, ,,	Sterile; much thick mucus from vagina
1	1·6 hours	Neutral	Sterile
2	2·12 ,,	,,	,,
3	3·14 ,,	,,	,,

in 1 case, and *faintly* alkaline in 3 cases. If all cases are excluded in which the vaginal secretion was not demonstrated to be sterile, then there are 44 cases available with the following results: The secretion was acid in 36 cases—i.e., in 81·8 per cent.; alkaline in 5 cases—i.e., 11·3 per cent.; neutral in 3 cases—i.e., 6·8 per cent.

From these observations it may be concluded that:—

(1) The vaginal secretion is sterile during the first forty-eight hours of life, and in the majority of cases (if not in all) for a much longer period.

(2) The reaction of the vaginal secretion is "acid" in the great majority of cases, from the time of birth onwards, and that this "acidity" is a normal quality of the secretion and is not due to the action of micro-organisms.

It may be asked how the presence of the neutral and alkaline specimens of secretion can be explained. To this question the following may perhaps be the answer: In the adult the vaginal secretion is acid, the cervical alkaline. If this be true of the infant also, it is possible that an excessive secretion of cervical alkaline mucus may give the vaginal contents a neutral or alkaline reaction. It may be noted that in three out of the five cases of alkaline secretion observed, the alkaline reaction was *very faint*. In support of the explanation suggested is the fact that in two of the alkaline series a considerable quantity of clear, viscid, mucoid secretion, having the naked-eye characters of adult cervical mucus, escaped from the vaginal orifice. It is of course possible that the vaginal secretion of adults contains organisms whose growth increases its acidity, but these observations show that the sterile vaginal secretion of young infants is in the majority of cases (81·8 per cent.) acid.

On looking up the literature of the subject, I find two papers published: Vahle[1] came to the conclusion that the vaginal secretion of infants was never sterile after the third day, and that it contained pathological organisms in a considerable number of cases. He found also that the reaction of the secretion was more often alkaline than acid or neutral. Stroganoff, quoted by Vahle, thinks that the vaginal secretion is seldom sterile after the fifth day. He found the secretion nearly always acid. As neither of these observers attempted to sterilize the parts at the vaginal orifice, their bacteriological observations are not reliable. However, they both found that the secretion was sterile during the first two days after birth.

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[1] *Zeitschr. f. Geburts. u. Gynäk.*, Stuttg., 1895, xxxii, p. 368.

DISCUSSION.

Mr. GLENDINING remarked that he did not think that the change in reaction could be accounted due to the mucous secretion from the glands of the cervix, as in the fœtus at term there were no glands to be found. He had examined a number of cervices of children at term, and had never encountered glands beneath the squamous epithelium.

Dr. HERBERT SPENCER had often met with viscid mucous secretion in the vagina, cervix, body and tubes in post-mortem examinations of new-born children. He was astonished at Mr. Glendining's assertion that there were no glands in the new-born child's cervix, as he (Dr. Spencer) had often seen well-marked cervical glands, sections of which he would bring before the Section.

Dr. GRIFFITH pointed out that the term "vaginal secretion" might be interpreted in two senses: strictly, secretion by the vagina, if any existed, which Dr. Robinson had not proved; or it might mean secretion in the vagina coming from parts of the uterus and Fallopian tubes. The latter was probably what Dr. Robinson meant. All observations on these points were of value. Further investigations were needed on the source of the vaginal contents in infancy as well as in adults.

Cases of Uterine Polypi undergoing Malignant Degeneration.

By H. MACNAUGHTON-JONES, M.D.

(For Professor SCHOTTLÄNDER.)

CASE I.

Polypus of the Cervical Canal undergoing Sarcomatous Degeneration.

THE PRESIDENT, having referred to Mr. Targett's specimen shown during the last session of a "carcinoma in a fibro-adenomatous polypus of the uterus," said that he had seen in the late Professor Rosthorn's clinic at Heidelberg a cervical polypus removed in which there was sarcomatous degeneration. Professor Schottländer had kindly sent him microscopical drawings of sections of the growth showing: (*a*) a cervical gland in its periphery becoming sarcomatous; (*b*) the surface of the polypi covered with a layer of proliferating, metaplastic cylindrical epithelium; (*c*) an island of cervix mucosa with many-layered glandular epithelium filling the gland lumen (arousing the suspicion of early carcinoma) surrounded by a sarcomatous stroma. The patient was aged 70; she would not submit to any further operation.

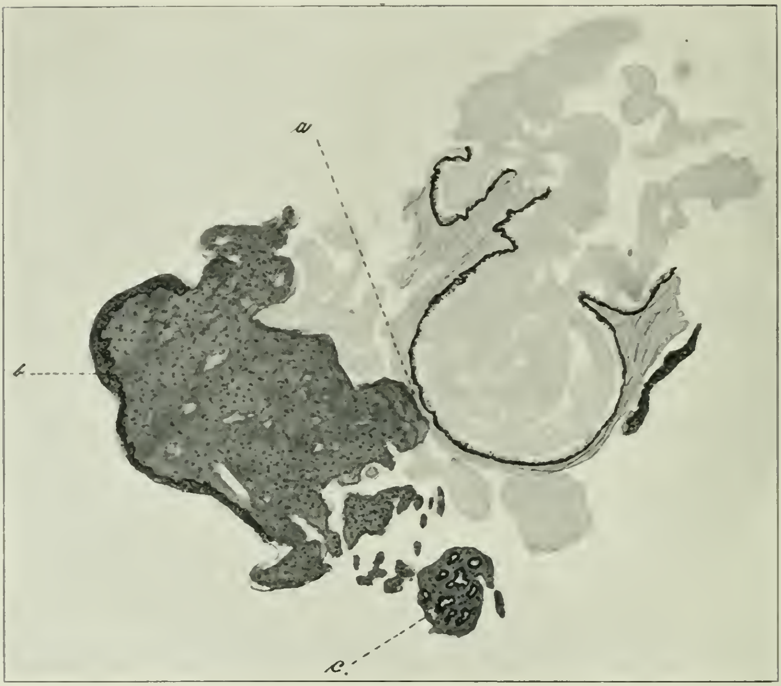

Fig. 1.

Case I.—General view of the polypus—low magnification. (*a*) (*b*) (*c*) Sections taken at these points indicated in this drawing.

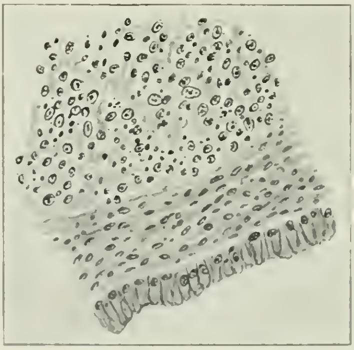

Fig. 2.

Case I (*a*).—Taken through the periphery of a cervical gland surrounded by a cellular stroma becoming sarcomatous.

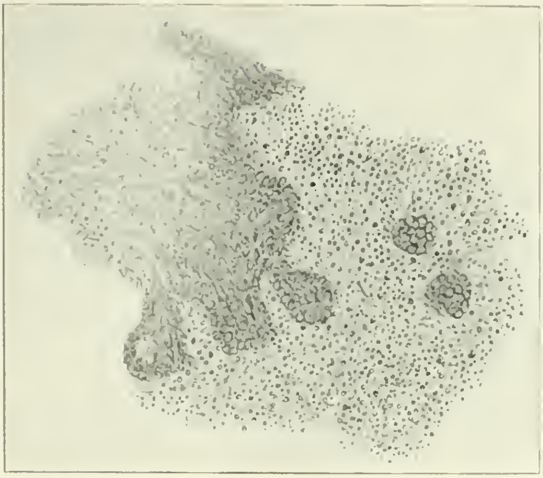

Fig. 3.

Case I (*b*).—Taken from the surface of the polypus. On the surface is a layer of metaplastic cylindrical epithelium, which proliferates inwards. Several detached single alveoli appear separated from the main mass.

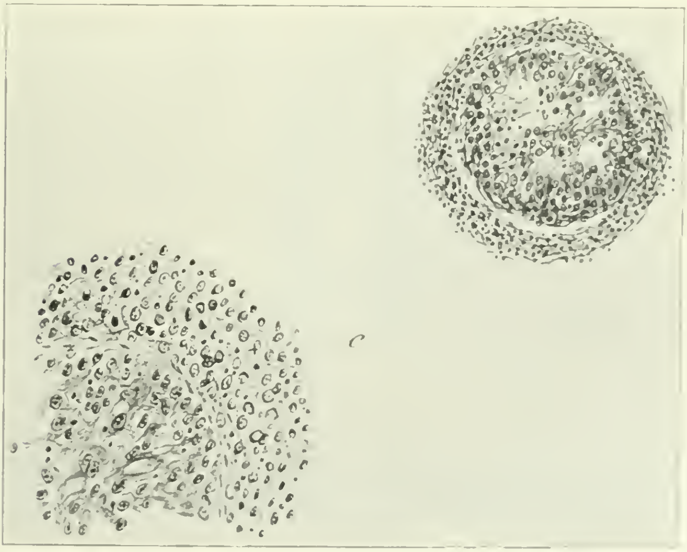

Fig. 4.

Case I (*c*).—An island of cervix mucosa with many-layered glandular epithelium filling the gland lumen and arousing the suspicion of early carcinoma surrounded by a sarcomatous stroma.

Case II.

Carcinomatous Polypus of the Fundus Uteri—the Cervix alone being Carcinomatous.

The President showed drawings of the sagittal section of an entire uterus, the cervix of which was destroyed by a carcinomatous crater, and in the fundus, growing from its summit, a mucous polypus. Section of the latter showed a solid carcinoma. There was no connexion whatever between the polypus and the carcinoma of the cervix; the mucosa of the fundus and body being quite free from carcinoma. The patient was a woman aged 57.

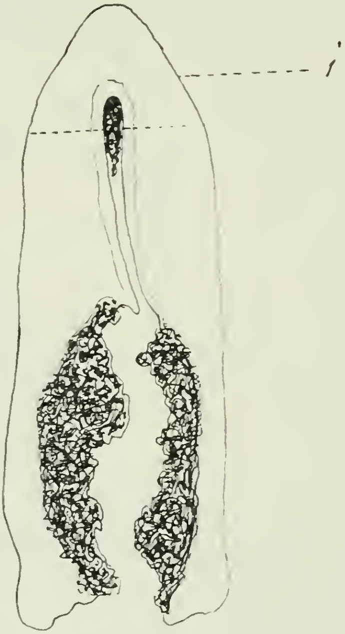

Fig. 5.

Case II.—The cervix shows a carcinomatous crater for which a radical operation has been carried out. A small mucous polypus is seen at the fundus, and sections have been made through the fundus at the level of the dotted line. The sections have not passed through the level of attachment of the polypus, which therefore seems to lie free in the uterine cavity. The polypus is seen to be invaded by strands of columnar-celled epithelium, some of which are solid and others alveolated. The endometrium of the corpus is atrophic; most of the gland epithelium has disappeared or dropped out of the lumina of the glands during preparation.

Fibroma of Ovary with Twisted Pedicle.

By MARY A. SCHARLIEB, M.S.

A. P., AGED 61, single. Always delicate and constipated, with a feeble digestion. Since February, 1910, she has had four or five attacks of very severe abdominal pain, accompanied with feverishness. She is said to have lost flesh and strength. The abdomen up to the level of the umbilicus is occupied with a very hard growth, which projects more to the right than to the left of the mid-line. It is movable from side to side, smooth in outline, and tender. Diagnosis thought to lie between an inflamed or degenerating fibromyoma, or possibly an inflamed ovarian cyst with very thick walls.

Operation, September 9, 1910: A dark red, very hard tumour was exposed, which looked like a degenerating fibroid. It sprang by a pedicle from the position of the left ovary; the pedicle was twisted two-and-a-half times from right to left. Uterus senile and apparently healthy. Right ovary senile; the tube was swollen and its walls were œdematous.

Examination of specimen: The specimen was a dark red solid tumour about the size of an adult's head, but longer from side to side than from before backwards or above downwards. The left tube was intimately attached to its surface, the ovarian fimbria being densely adherent to it. An incision into the tumour showed a dark red surface, which suggested that it was a fibroid tumour infiltrated with blood under pressure. The Fallopian tube contained clotted blood.

Pathological Report.—This solid ovarian growth measures 19 in. by 15 in. in the two circumferences taken at right angles to each other. The Fallopian tube and meso-salpinx are engorged with blood due to strangulation by a twist. The tumour on section is seen to be discoloured by blood. The central part is nearly black, whilst between this and the capsule is a zone of red pigmentation. The tumour has the structure of a simple fibroma. Beneath the capsule are many large vessels engorged with blood, whilst in the black central area there is free hæmorrhage in the tissues. The tumour is benign.

Dr. FAIRBAIRN said he had removed a very similar specimen only a few weeks ago, and this was the first time he had met with an ovarian fibroma with a twisted pedicle. The patient had had an acute abdominal attack—

diagnosed as peritonitis—which first drew attention to the solid tumour in the abdomen. Bimanually it could be distinguished from the uterus, and was thought to be an ovarian cyst distended with blood from torsion of its pedicle. It turned out to be a solid tumour—on section an ovarian fibromyoma—engorged with blood so as to be almost black in places.

Mrs. SCHARLIEB also showed a *Specimen of a uterus containing fibromyomata with a breaking-down adenomatous growth, probably malignant.*

Obstetrical and Gynæcological Section.

November 3, 1910.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

The Symptoms and Treatment of Chronic Endometritis, with Special Reference to the Results of Curetting.

By Archibald Donald, M.D., and W. Fletcher Shaw, M.D.

In the whole domain of gynæcology there are no cases so common as those which generally go by the name of "chronic endometritis." This term has been commonly used to denote a class of cases which are well known clinically but difficult to define. They are usually understood as cases of disordered uterine secretion (this term being used in a wide sense), which are not associated with any great enlargement of the uterus, or any indication of fibroid or malignant growth, or any marked alteration in the uterine appendages. We find, however, that this title has been employed to denote two groups, which appear, on the surface at least, to differ in their ætiology, although the symptoms are the same. In the first group the symptoms seem to date from a confinement or a miscarriage, and these have generally been regarded as the result of some inflammatory change; in the second the patients are either unmarried or have never been pregnant, and any inflammatory change of the usual septic or specific origin seems out of the question.

That the whole subject is still in a state of confusion is apparent to every one whose duty it is to try and give a clear account of minor gynæcology to medical students. At the present time one turns in vain to the pathologist for light. Concerning the pathology of the endometrium there is still comparatively little definitely known. The rôle which is played by the various uterine structures in the production of the ailments which we are considering is still unsettled. Some attribute the symptoms mainly to alterations in the endometrium, although they differ as to what these alterations consist in; others believe them to be caused by abnormalities in the mesometrium, and these, again, differ as to whether the muscular tissue, or the connective tissue, or the blood vessels are mainly at fault. From this confusion we cannot hope to emerge completely till uterine pathology has made great advances. Further investigations may reveal some change, associated with the symptoms which indicate an inflammatory condition; but the absence of well-marked microscopic changes, taken in conjunction with the history of many of the cases, seems to indicate that real chronic inflammation of the endometrium is rare. Nevertheless, we propose in this contribution to continue the use of the name "chronic endometritis," with the definite statement at the outset that we do so merely as a matter of convenience. Until the pathology of the conditions grouped under this heading has been more fully worked out, no other course seems open.

It may be well at first to state the objects of our inquiry, the clinical material at our disposal, and the method of obtaining the information.

The main object of the inquiry was to ascertain the result of the direct treatment of the endometrium by curetting, and curetting alone, and the comparison of the results so obtained in different types of cases. Further, we wished to find out the relative frequency of the various symptoms and the result of treatment in each individual symptom.

The clinical material consisted, in the first instance, of 255 hospital and 163 private cases, in all of which notes had been made before treatment; but this material was reduced by the fact that definite information as to the later results of treatment was only obtained in 172 hospital and 95 private cases. We have thus 267 completed cases, a study of which forms the basis of this paper. The inquiry was limited to cases of chronic endometritis only; no case was included in which other complications existed, such as descent of uterus or vaginal walls, changes in tubes or ovaries, or evidence of chronic pelvic peritonitis. Further, only those cases were made use of in which a period of at least twelve months had elapsed since the treatment.

The information was obtained by sending out a list of printed questions to each patient. Most of these questions were framed so that a "Yes" or "No" was all that was required in the way of answer. All the patients were treated by one of the authors (Donald), but the inquiries in the hospital cases were made by the other (Shaw). In this way it was hoped we might eliminate replies of too favourable a kind, due to the desire to please. In regard to the private patients, inquiries were made by the operator. The comparison of the two sets of replies shows a remarkable similarity in result.

SYMPTOMS.

The symptoms of chronic endometritis are menorrhagia, metrorrhagia, leucorrhœa, dysmenorrhœa, and aching (discomfort or dragging) in the lower abdomen, or more rarely in the sacral region. This latter symptom is referred to afterwards as "chronic pain." All these symptoms are not present in every case; as a rule, one or two are well marked, and the others are absent or exist in a minor degree. Excessive hæmorrhage and leucorrhœa are the symptoms generally recognized as those of chronic endometritis, and in our opinion (for reasons which are stated later) dysmenorrhœa may also be due to an abnormal condition of the endometrium. The chronic abdominal pain we believe to be due to the enlargement of the uterus. It is most pronounced and most frequent when the increase in size is well marked, but even a moderate amount of enlargement of the body will cause it, if there exists also an extreme flexion. There are some other local troubles which seem undoubtedly caused by chronic endometritis, such as vaginismus, dispareunia, and irritability of the bladder. In some of our cases these were noted as being present along with the other symptoms, but, as accurate notes on these points were not kept in all cases, we think it better not to notice them more fully. Further, we have not noted still more remote disturbances, such as general ill-health, dyspepsia, headache, &c., as it is impossible to be sure that these are really symptoms of the uterine condition.

In the following table we give the frequency of the various main symptoms. We have classified the patients into three classes: (1) Parous, (2) married sterile, (3) virgins:—

TABLE I.

	105 married, parous		60 married, sterile		90 virgins	
	No.	Per cent.	No.	Per cent.	No.	Per cent.
Menorrhagia	67	63·8	31	51·6	43	47·7
Metrorrhagia	44	41·9	11	18·3	17	18·8
Leucorrhœa	78	74·2	35	58·3	54	60·0
Dysmenorrhœa	66	62·8	51	85·0	77	85·5
Chronic pain	50	47·6	20	33·3	40	44·4

From this table it will be seen that menorrhagia, dysmenorrhœa, and leucorrhœa are most frequent in all classes, menorrhagia and leucorrhœa being most frequent amongst women who have had children, and dysmenorrhœa most frequent amongst nulliparæ and virgins. Metrorrhagia is comparatively rare except in women who have had children.

PHYSICAL SIGNS.

The physical signs of chronic endometritis are mainly enlargement of the uterus, with undue tenderness on bimanual examination. The uterus is practically always enlarged; it may sometimes feel small if it is in a position of acute anteflexion, but the sound when passed is found almost invariably to go half or three-quarters of an inch beyond the normal, sometimes even an inch or more. The effect of the enlargement is to cause exaggerated anteversion, or if the isthmus is unduly pliant an acute anteflexion results; sometimes there is retroversion or retroflexion. The following table shows the relative frequency of the forward or backward position of the fundus. No distinction is made between flexion and version:—

TABLE II.

	Married, parous	Married, sterile	Virgins	Total
Anteflexed	25	30	46	101
Retroflexed	46	20	21	87

It will be noticed that the parous women and the virgins show a remarkable contrast. Anteflexion is twice as common in virgins as retroflexion, while in the parous woman the conditions are reversed. The married sterile class seems intermediate, with a tendency to anteflexion. In virgins the so-called "cochleate" uterus is frequently found associated with chronic endometritis; the lower portion of the uterus as high as the internal os is infantile in type, and runs backwards so as to simulate a retroversion. The body of the uterus, on the other hand, is well developed, and is bent sharply forwards. In all three classes erosion of the cervix is not uncommon, especially in cases in which leucorrhœa is a prominent symptom. The erosion generally affects both lips of the cervix, but sometimes is found only in one. The situation of the erosion was not found to bear any relation to the position of the uterus.

It will be noticed that we have made no distinction between cervical and corporeal endometritis. In the class of cases that we are dealing with cervical endometritis was always regarded as being the result of the trouble in the corporeal endometrium, and no difference was made in regard to the treatment.

RESULTS OF CURETTING.

General Result.—It will be noted that each patient was asked to state her opinion as to the general result of the operation. While we fully admit that in some circumstance the conclusions from such an inquiry might be regarded as of small value, there are two considerations which appear to us to make the results thus obtained fairly reliable. Firstly, each patient who was asked to give the result of the operation was also asked to give the result as regards each special symptom, and in this way her reply to the general question could be corroborated. Secondly, the number of cases is sufficiently large to reduce the source of error to a small proportion:—

TABLE III.

	81 married, parous		75 married, sterile		111 virgins		Total (267)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	70	86·4	65	86·7	95	85·6	230	86·1
Not improved ...	11	13·6	10	13·3	16	14·4	37	13·9

The result would have come out still better if allowance had been made for a certain number of cases in which there had been an error in diagnosis; some cases have been included in which such an error has since been discovered. Perhaps the most common mistake was made in cases in which there was a small intramural fibroid; the symptoms in these cases are often precisely the same as those of chronic endometritis, and it is not possible always to detect the early stages of a fibroid by bimanual examination. In two of the private cases the uterus has since been removed by other surgeons for uterine fibroid, and in three other cases we have found a fibroid some years later. In these cases the symptoms are more likely to be aggravated than relieved by curetting; the thinning of the endometrium lessens the amount of tissue between the uterine cavity and the tumour, and the latter is therefore more likely to be driven towards the cavity, and hæmorrhage and pain are thus increased. In our opinion it is probable that 4 per cent. or 5 per cent. of our cases were really cases of small intramural fibroids. Then, again, some cases were no doubt cases of so-called chronic metritis; and when this trouble has been of long standing, nothing short of a hysterectomy gives relief. Further, there were cases in which the uterine troubles occurred in women of a neurotic temperament, and some of these replied in general terms that they were no better even when they admitted that two out of three of their old symptoms had disappeared. It might be urged that against these might be placed some neurotic cases in which the uterine symptoms had been mainly subjective, and in which benefit had occurred partly as the result of suggestion. After all, the general results are only slightly affected by these exceptions, and the figures only confirm the general impressions which we had formed from long-continued and careful observation.

Date of Improvement.—The large majority of the patients did not obtain the full benefit until some months after the operation. From this fact two deductions may be reasonably made: firstly, that the improvement was not due to dilatation; and, secondly, that it was not the result of mere suggestion, as in both of these conditions the improvement would have followed at once. In this connexion we may note that the question of the treatment of a neurasthenic patient by resting, nursing, and feeding does not apply in any of our cases; they were practically never under our care longer than a week. We cannot explain why the full benefit of the operation should be delayed; it may be that the endometrium takes some time to be normally constituted after the scraping—a similar state of things holds good, we understand, in regard to the progress after an operation for post-nasal adenoids—or it may be that some gradual involution of the muscular tissues takes place as a result of the operation.

Relapses.—The following table shows that of the 230 cases which were improved as the result of curetting, forty-one had some return of symptoms at a later date. Many of these cases are not really relapses, but fresh attacks dating from another confinement or miscarriage; but to obtain the permanent results of the one curetting it is necessary to subtract all cases in which the symptoms had reappeared, and to ignore all special causes. The following table therefore gives the final results:—

TABLE IV.

	81 married, parous		75 married, sterile		111 virgins		Total (267)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Cured	57	70·4	55	73·3	77	69·4	189	70·8
Not cured ...	24	29·6	20	26·7	34	30·6	78	29·2

As to the cause of the real relapses we know nothing. It may be that the curetting was not sufficiently thorough in these cases, or it may be that the original cause of the local trouble remained still active.

A second curetting was done in six cases in which the operation had been unsuccessful, and five of these showed marked improvement. In four cases a second operation was performed, because the old symptoms had recurred without obvious cause after a time. This second operation was followed by marked relief in every case. All of these latter patients had been relieved for some years by the first operation, and were eager to have it repeated.

THE RESULT OF TREATMENT IN REFERENCE TO EACH INDIVIDUAL SYMPTOM.

(1) *Dysmenorrhœa.*—This was the most frequent symptom in the nulliparous patients and one of the most frequent in the parous women, and, as will be seen from the table, was one which was most amenable to treatment. The percentage of good results was very similar in all three classes of cases—parous, sterile, and virgins :—

TABLE V.—DYSMENORRHŒA.

	53 married, parous		58 married, sterile		91 virgins		Total (202)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	43	81·1	49	84·5	75	82·4	167	82·6
Not improved ...	10	18·9	9	15·5	16	17·6	35	17·4

The ætiology of dysmenorrhœa is still somewhat obscure. It may undoubtedly be caused by conditions outside of the uterus. It is a common complaint amongst women whose tubes and ovaries are matted together as the result of an acute inflammation, and in these cases it is doubtless due to the congestion of organs which are bound down by adhesions. But we do not know the cause of dysmenorrhœa in cases in which the uterus and its appendages are quite mobile, and where there is no new growth. If we reason by analogy from pain which occurs in other hollow organs containing non-striped muscle, we must conclude that it is due to irregular or painful contractions. This still leaves the question unsettled as to why these irregular contractions should be set up; certainly narrowness of the cervical canal is in itself not sufficient to cause painful menstruation. Every gynæcologist has seen cases of extreme narrowing of the external or marked flexion at the internal os, where there has been no complaint of dysmenorrhœa; and, on the other hand, acute dysmenorrhœa may exist in cases where a large sound passes with ease into the uterus. Probably the most acute pain during menstruation is suffered by some patients who are the victims of what is called "membranous" dysmenorrhœa. We are therefore justified in concluding that some abnormal conditions of the endometrium, or of its secretions, may cause severe dysmenorrhœa. The results of curetting go to confirm this: more than 80 per cent. of all cases were entirely cured or much relieved by this means. We have already pointed out that the full benefit was generally delayed for some months, and that this proves that the relief was not due to dilatation of the canal. For this reason we believe that mere dilatation is not sufficient in cases of uterine dysmenorrhœa; curetting must also be performed. We have no doubt that there exists a distinct type of case in which some abnormality of the endometrium co-exists with an extreme flexion at the internal os. In the majority of cases of so-called endometritis in virgins this type of uterus was found. The condition seems to be due to faults in development; there is an unequal development of the upper and lower portions of the uterus; the cervix in these cases seems to remain infantile in type, while the body undergoes development, and an acute flexion is in this way produced. In the great majority of cases in our tables classified as "married, sterile," the same type of uterus was found. The condition may even persist in women who have had children, but in this group this type of uterus was not commonly found. A study of our cases strengthens the opinion that intramural fibroids at an early stage of their development may cause acute dysmenorrhœa. It was a notable symptom in the cases in which a uterine fibroid was found at a later stage, and in these no relief of the dysmenorrhœa followed the curetting.

Leucorrhœa.—The table below shows that this symptom is not quite so amenable to treatment as dysmenorrhœa:—

TABLE VI.—LEUCORRHŒA.

	50 married, parous		36 married, sterile		51 virgins		Total (137)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	36	72	28	77·7	33	64·7	97	70·8
Not improved ...	14	28	8	22·3	18	35·3	40	29·2

The results are good as regards married women, but in rather more than a third of the cases in virgins there was no improvement. There are some writers who seem to hold the opinion that leucorrhœa in these virginal cases is mainly vaginal in origin; one might almost infer that they believed that a sort of transudation of serum occurs in young women who are anæmic or in a weak state of health. There was no evidence in any of our cases in support of the vaginal origin of the discharge; it could always be demonstrated that it came from the cervical canal or from the cervix, or, in other words, that there was a real catarrh of the uterus. In none of the cases was there any reason, either from the history or physical signs, to suspect gonorrhœal infection; in no case did the vaginal mucous membrane show any sign of inflammation; all doubtful cases were excluded. In some of the cases cervical erosion was very extensive, and the discharge was of the thick, glairy variety. More commonly there was quite a narrow red ring, or erosion, round the external os, and the discharge could be seen coming from the cervical canal and was thinner in consistence. In the cases of cervical erosion which are considered in this paper, no treatment beyond curetting was adopted; in other cases, which we are not at present considering, where there was marked hypertrophy of the cervix, excision of the diseased tissue was employed.

Chronic Abdominal Pain.—From this table it can be seen that quite good results followed the curetting:—

TABLE VII.—CHRONIC PAIN.

	38 married, parous		30 married, sterile		39 virgins		Total (107)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	32	84·2	26	86·6	32	82	90	84·1
Not improved ...	6	15·8	4	13·4	7	18	17	15·9

It is impossible to say whether this pain is merely the result of mechanical dragging, or whether it is a "referred" pain caused by an abnormal condition of the endometrium; but it disappeared after curetting in about 84 per cent. of all the cases.

Menorrhagia.—The results as regards menorrhagia were also good, as will be seen from the table:—

TABLE VIII.—EXCESSIVE MENSTRUATION (MENORRHAGIA).

	44 married, parous		24 married, sterile		44 virgins		Total (112)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	34	77·3	21	87·5	34	77·3	89	79·5
Not improved ...	10	22·7	3	12·5	10	22·7	23	20·5

Metrorrhagia was not a frequent symptom, but the table shows that when it occurred in virgins it was cured in almost every case by the operation:—

TABLE IX.—METRORRHAGIA.

	19 married, parous		9 married, sterile		14 virgins		Total (42)	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	14	73·7	7	77·7	13	92·8	34	80·9
Not improved ...	5	26·3	2	22·3	1	7·2	8	19·1

In comparing the results of treatment in the various symptoms, it will be seen that they were best as regards dysmenorrhœa and chronic abdominal pain, and were least satisfactory as regards leucorrhœa.

THE POSITION OF THE UTERUS.

Our collected cases gave us the means of answering the inquiry as to whether the results of the operation were influenced by the position of the uterus. The practical importance of this question is obvious when we consider that a great many gynæcologists believe that the symptoms we have just been discussing, and which we have called those of chronic endometritis, are often produced by backward displacement of the uterus. We have been unable to find, however, an explicit statement as to how (step by step) a retroversion can cause dysmenorrhœa, menorrhagia, or leucorrhœa.

Great importance is attached by some to a passive congestion which is said to be caused by the uterus strangling itself in its own ligaments. This is pure theory; there is no direct evidence of this venous congestion; the cervix is not bluer in these cases of retroversion, nor is there any bleeding of a darker colour than the ordinary uterine hæmorrhage. Further, exactly the same symptoms are found in cases where the uterus is anteverted or anteflexed, so that even if retroversion is one cause, it is not the only cause of the trouble which we have classified as chronic endometritis. Not one of the many ardent believers in "fixation" or "suspension" operations has ever been bold enough to advocate these measures in cases of heavy anteversion or anteflexion. An erosion of the cervix, if associated with retroversion, is treated by these enthusiasts by ventrofixation or Alexander's operation, but an erosion of the cervix in an anteverted uterus has a different kind of treatment, or none at all.

The effect of treatment by curetting only must cast some light on the causation of the symptoms; if they are produced by a retroversion, then curetting should have no effect, or a very temporary one at best, unless it were followed by some operation to pull up the retroverted fundus. When we come to examine our cases, we find in our series of hospital cases that 101 patients had anteflexed or anteverted uteri, and 87 had retroflexed or retroverted uteri. Seventy-five of these latter 87 with backward displacement replied to our questions, and the result was that 66 (or 88 per cent.) were improved, while nine (or 12 per cent.) were no better:—

TABLE X.—RESULTS IN RETROFLEXED UTERUS.

	32 multiparæ		19 married, sterile		24 virgins		Total (75	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Improved ...	28	87·5	18	94·7	20	83·3	66	88
Not improved ...	4	12·5	1	5·3	4	16·7	9	12

This result is even better than that for all cases taken together (see Table III), which gives 86·1 per cent. improved and 13·9 per cent. not improved. In other words, curetting in cases of retroversion was followed by results which are rather better than those in cases of anteversion. In many cases the symptoms entirely disappeared while the so-called displacement remained. In other cases it was found later on that the uterus had assumed a position of anteversion.

We are aware that good results—as regards the cure of symptoms—are claimed by those who practise ventrofixation. We can well believe that, if there is some degree of prolapse associated with the other changes in the uterus, the sensation of weight or dragging may be relieved by this operation. But we have no statement from these operators as to the exact results on the individual symptoms, and, on the other hand, we have cases to show that dysmenorrhœa, menorrhagia, and leucorrhœa commonly persist after the uterus has been fixed up.

In the discussion on endometritis at the annual meeting of the British Medical Association in 1909 there was one speaker who expressed himself as tired of the discussion as to whether curetting or fixation was to be preferred in the treatment of retroversion, and said that in these cases he always did both. This is no doubt characteristic of the attitude of a good many. But, in the first place, it is surely the aim of every man who takes a scientific interest in his work to find out what is at the root of symptoms; and, secondly, if curetting alone is sufficient to cure a large proportion of cases, why should all these patients be subjected, in addition, to what is, after all, a serious operation, and one which in quite a considerable proportion of cases causes trouble in after-life. We believe our cases prove that curetting is successful in nearly 90 per cent. of cases of backward displacement not associated with prolapse. Of the remaining 10 per cent., some may be cured by a second curetting, some will prove to be associated with an intramural fibroid, and others to be cases of chronic metritis. It is our custom, when a minor degree of prolapse exists as well, to perform a plastic operation on the posterior vaginal wall after curetting. These cases, however, come under rather a different category, and we have purposely not included them in this present inquiry.

The Effect of Curetting on Sterility.

Amongst the symptoms of chronic endometritis we did not include sterility, as there must always be an element of doubt as to the cause of this condition. Nevertheless, we find it frequently associated with the other symptoms: of the 156 married women in our list, 75 (or 48 per cent.) were sterile. This point was not investigated in detail, so that we cannot give the actual figures relating to the duration of the sterility; but the operation was performed in only a very few cases within a year of marriage, and most of the patients had been married for at least three years. After curetting, 22 of the 75 (or 29·3 per cent.) became pregnant. That is to say, nearly one woman in three of those who were previously sterile became pregnant after the operation—a result which compares favourably with results of dilatation only, or of bisecting the cervix, as given by other authors. There was one striking case in which a patient was sterile after thirteen years of married life, and suffered from severe dysmenorrhœa. After curetting she had no more dysmenorrhœa and became pregnant after two periods.

THE OPERATION.

It is unnecessary to give details as to the method of operating, but we wish here to express the opinion that the treatment we have advocated must be thorough if it is to be of any benefit. A mere superficial and irregular scratching is worse than useless, as it tends to give rise to the impression that curetting is an operation of no value. Both anterior and posterior surfaces of the uterus must be scraped, firmly, evenly, and methodically, with a sharp curette. Of all gynæcological operations it is the one which most demands a practised hand. But when it is done thoroughly, and in suitable cases, there is no more valuable measure, as we think the results of our inquiry go to prove.

DISCUSSION.

Dr. AMAND ROUTH considered that the authors were really speaking of cases of chronic metritis. He did not believe that cases of chronic endometritis ever occurred apart from changes in the vessels and muscle of the uterine body, nor did he believe that such cases as these had true inflammation of the endometrium, but rather that there was overgrowth of the endometrium arising from a previous pathogenic or saprophytic infection or associated with subinvolution. In these cases, as the authors themselves admitted, the body of the uterus was enlarged and often retroverted and prolapsed. Curettage cured many of these cases because it removed the overgrowth of adenoid tissue and led to absorption of the congestive and inflammatory products in the muscular and vascular tissues of the uterus. Thus lightened in weight, the prolapse, or retroversion, was often spontaneously rectified during the subsequent rest in bed, though no doubt in suitable cases these results could be hastened by appropriate temporary help from pessaries. He considered ventrofixation very rarely required, and agreed with the authors that a congenital retroversion or retroflexion *per se* was often asymptomatic. It was not either necessary or desirable to curette all cases of dysmenorrhœa requiring treatment. There were many causes of dysmenorrhœa, both constitutional and local, and if these causes were absent an empirical dilatation would relieve quite as well as curettage.

Dr. T. G. STEVENS did not agree that the pathologists could offer no suggestion as to the nature of the lesion in the so-called condition of endometritis. He considered that the thickening of the uterine mucous membrane observed in these cases was truly the result of inflammation, and in most instances due to infection. At the same time he believed that the same appearance might occasionally occur as a result of long-continued congestion. Inflammation did not consist solely of a leucocytic infiltration, but also comprised local overgrowths of the fixed tissues of the part. In endometritis there was rarely leucocytic infiltration, but Dr. Stevens held that the appearances so well known represented the overgrowth moiety of a chronic inflammatory process. Dr. Stevens also was glad to hear that Dr. Donald believed that curettage could cure dysmenorrhœa. This naturally followed in cases of endometritis, but Dr. Stevens also considered that it was the proper treatment after dilatation in cases of spasmodic dysmenorrhœa in which no endometritis could be demonstrated. He did not believe that dilatation alone would cure many cases of dysmenorrhœa, but agreed that temporary benefits for a few periods might be the result. Dr. Stevens agreed that endometritis occurred in virgins, and pointed out that minor degrees of erosion of the cervix were often found in cases of dysmenorrhœa. Such cases were always accompanied by excessive menstrual losses.

Dr. HUBERT ROBERTS thought that the definition of endometritis as given by the authors was rather vague and based only on symptoms, and that it would have been of more value if microscopical evidence had also been produced. The question of endometritis was always a difficult one; it seemed not merely an inflammatory condition, but one often associated with adenomatous overgrowth; further, the endometrium in cases described by the authors was not the only part of the uterus affected, but the musculature, vessels and lymphatics must often be involved. Regarding the question of dysmenorrhœa, Dr. Roberts took the condition to be a painful colic or spasm of the uterus, and, as pointed out by Champneys and Herman, a sharp distinction must be made between true primary dysmenorrhœa with pain *at the time*, and pain *in between* the periods which often depended on some pathological pelvic product, and therefore was not true dysmenorrhœa at all. Dr. Roberts did not agree with the authors that flexions and versions, as such, could be the *cause* of endometritis or of dysmenorrhœa. Vedeler's statistics had proved this. Further, in virgins, anteversion was the *normal* position of the uterus, while retroversion and retroflexion were signs of uterine descent. Such descent was frequent after much child-bearing, and such uteri were often large and "subinvoluted." Dr. Roberts regarded the subinvolution as the cause rather than the result of any concomitant endometritis. As to the question of curetting in dysmenorrhœa, Dr. Roberts thought that it was rather the dilatation than the curetting that did good; he also expressed his doubts as to occurrence of real membranous dysmenorrhœa. Dr. Roberts did not think that true dysmenorrhœa could occur in parous women. He had always been under the impression that parturition was the only and best cure for dysmenorrhœa, and that painful menstruation in parous women often depended on some pathological condition in the pelvis resulting from parturition, such as inflamed ovaries or tubes. Dr. Roberts asked the authors if they could explain the occurrence of cervical erosions in virgins, as they certainly occurred in cases where any question of infection, such as gonorrhœa or tubercle, could be eliminated.

Mr. TARGETT thanked the authors for their interesting and valuable communication. He thought that the form of endometritis which occurred in young girls and caused severe menorrhagia was allied to adenoid vegetations in the nasopharynx, and histologically the curettings consisted largely of lymphoid tissue rather than glandular hypertrophy. He did not advise curetting as a routine treatment in spasmodic dysmenorrhœa, because in cases of scanty menstruation due to deficient uterine function there was a very real danger of causing complete stoppage of menstruation—much to the detriment of the patient. Where the menstrual function was normal, except for the severe pain, he considered curettage a necessary part of the treatment by dilatation of the cervix.

The PRESIDENT (Dr. Macnaughton-Jones), having thanked the authors for the discussion of a subject which was of interest to all practitioners, said that everything depended on the type of endometritis they had to deal with. Were there collateral conditions present associated with or causing the endometritis? What was the nature of the discharge, and what were the microscopical results as shown by the curetting? He made it a rule after all curettage operations to have the débris reported on. In a large number of these, when the conditions were benign and suppuration was absent, the pathological reports were singularly alike—mainly showing glandular and cellular changes. Occasionally, however, a suspicion of malignancy was discovered or the malign or gonorrhœal origin was established. Many cases were associated with tubal and ovarian affections or displacements, or some intra-uterine growth and severe erosions. To differentiate chronic endometritis from what was loosely spoken of as metritis, where the uterus was enlarged, was not possible. The two were frequently co-existent. He could understand a permanent exaggerated anteversion or an anteflexion being a source of future trouble after curettage, but that a retroflexed uterus should be regarded more favourably from the point of view of prognosis he could not. "Leucorrhœa" was a term often loosely applied, and, if the discharge were not examined microscopically, might mean anything. He did not believe in dilatation without curettage for the cure of a dysmenorrhœa associated with endometritis. The canal quickly reverted to its original state. Dysmenorrhœa he had seen many times present in a parous woman. This depended on the cause of the dysmenorrhœa. It might, for example, have a central nervous origin and the genitalia be found quite normal, as in the instance of some neurasthenic women. He would like to know whether the displacements remaining after curettage were subsequently treated by the authors. Assume, for example, that an ovary was prolapsed at the same time as the uterus was curetted.

Dr. FLETCHER SHAW, in reply, said several of the speakers had discussed endometritis from a pathological standpoint. This was entirely outside the scope of the paper. One criticism raised was that the cases of dysmenorrhœa were cured by the dilating, not by the curetting. He thought it was due to the curetting, because (1) Dysmenorrhœa seldom occurs alone, but generally along with some change in menstruation; no amount of dilating would cure menstrual changes, but curetting did; so very probably it is the curetting which also cures the dysmenorrhœa. (2) Many cases of dysmenorrhœa occur without a stenosed cervix. (3) Many cases of dysmenorrhœa commence after a confinement, when the cervix has received its maximum dilation. (4) The cases of dysmenorrhœa which he had seen treated by dilatation alone had not been nearly so successful as those treated by curetting. (5) Most of our cases steadily improved for some months after the operation. Dr. Roberts was quite mistaken in attributing to the authors the opinion that endometritis or dysmenorrhœa was due to a flexion or version: one of the main objects of the paper was to uphold the contrary. Another criticism raised was the old bogey of retroflexion. He did not consider retroflexion caused the symptoms, because (1) the symptoms occurred in normally placed, anteflexed and retroflexed uteri; (2) retroflexed uteri often occurred without symptoms; (3) in the cases quoted in the paper, the results were slightly better in retroflexed uteri than in normal and anteflexed uteri. He considered that no patient with the symptoms of endometritis ought to be submitted to the risk of ventrofixation until curetting had been given a thorough trial.

Dr. DONALD, in reply, said that although Dr. Shaw and he had tried to make it perfectly clear at the beginning of the paper that the subject which they were bringing forward merely concerned a certain clinical group of cases, and that they were merely using the term "endometritis" as a matter of convenience, nevertheless many of the criticisms were only concerned with the question of nomenclature. It was a difficult thing to prevent a discussion about words and names, but they had done their best to clear the ground at the very beginning of the paper. The only other point that he wished to refer to was the President's inquiry as to what other procedure was adopted in cases of retroversion associated with prolapsed ovary or ovaries. He wished it to be clearly understood that the cases discussed in his paper were those in which the uterus was mobile, in which there was no prolapse of the pelvic floor, and in which there was no disease of the appendages. The point of the treatment was that the position of the uterus was absolutely ignored, and no attempt was made after the operation to alter a retroversion into an anteversion. If a prolapsed ovary was found, provided there was no evidence of cystic disease or adhesions, this condition was also ignored. He wished again to draw attention to the fact that the results in cases of retroversion were really better than those in which the uterus was anteverted.

A Case of Pseudohermaphroditism.

By Percival P. Cole, F.R.C.S.

F. S., aged 23, was operated on by Mr. Jordan Lloyd in April, 1908.

Family history: Father is dead; the mother married again. The patient is the only child of the first marriage; the mother is healthy, and has had four children by the second husband; of these, two are girls, their ages being 15 and 12 respectively. As far as the patient knows there is nothing abnormal about them, but she has no knowledge of their menstrual history.

Personal history: Patient has always been a strong girl and noticed nothing abnormal till the age of 15, when she began to suffer pain in the right and left side of the lower abdomen. This pain was very acute, lasted some minutes, and occurred frequently but without regular periodicity. The attacks were unaccompanied by any other symptoms suggesting menstrual molimina; the breasts did not swell nor become tender; there was no nausea nor headache. These pains were shooting in character, and became so bad that the patient was admitted to the Birmingham Hospital Infirmary, where in April, 1908, she underwent an operation. Micturition was painless, and there was no discharge from the anus. She has been much troubled on account of entire absence of menstruation.

Post-operative history: Since the operation the pains have become less severe and frequent, but have not altogether ceased. They have been confined to the right side of the lower abdomen, and cease as suddenly as they commence.

Result of examination carried out in March, 1909 (I am indebted to Mr. Jordan Lloyd for his courtesy in permitting me to make this examination): The patient is of average height and entirely female in appearance; her breasts are very well developed, but the nipples are small and do not protrude; the mons veneris is not so prominent as might be expected in a girl so well developed; there is entire absence of pubic hair; the vulva is small; the labia majora are small, as also are the nymphæ, but these are otherwise normal, forming a prepuce for the clitoris, which itself is very small. The urethral orifice is normal, and situated just behind this is the orifice of the vagina, presenting posteriorly a crescentic ridge—the hymen. The vagina on inspection is seen as a shallow conical canal, the mucous membrane being everywhere smooth and presenting no trace of any orifice. It admits the forefinger without pain or difficulty, and the roof of the canal can be pushed up till the finger lies in the canal up to the first joint. The posterior commissure is present, the perineum is short; the face is female and is quite smooth; the hair on the head is long and has been longer. In the middle line of the abdomen, below the umbilicus, is the operation scar, which now measures a little more than an inch. The patient has never menstruated.

Operation: On account of the severe pain so frequently experienced by the patient, which prevented her effectually earning her living, Mr. Jordan Lloyd decided to operate. On opening the abdomen no broad ligaments were noticeable, but lying slightly to the left of the middle line and on the floor of the pelvis was a rounded mass occupying the situation of the uterus between bladder in front and rectum behind. This mass was connected to the vault of the diminutive vagina, for traction on it caused a corresponding rise in the level of the roof of that canal; no tubes and no ovaries were discovered. Mr. Jordan Lloyd is emphatic in stating that this mass which he removed was the sole representative of the internal genitalia. The following are the pelvic measurements: interspinous, 10 in.; intercristal, 10¾ in.; antero-posterior, 8 in.

Mr. Jordan Lloyd handed over the specimen to me for examination and a description is here appended.

Description.

The specimen represents the entire internal genitalia. The rounded central mass, shaped somewhat like a uterus, smooth in contour, firm in consistence, measures 4½ cm. long by 2½ cm. broad. It is flanked by two folds containing a tube-like structure, that on the right being much longer and more definite. On the right side this tubule can be traced into the lateral margin of the central mass below, but above is lost in the loose areolar tissue, at the summit; its upper limit is marked by a small, rounded, bud-like projection which is obviously cystic in character and translucent. On the left side the fold can be traced into the central mass both above and below. The fold on each side is separated from the central mass by an interval which forms the mouth of a cul-de-sac 1·25 cm. deep on the right side, but much shallower on the left. These pouches extend in an antero-posterior direction. Below and on either side are seen the cut surfaces marking the site of operation. Nothing resembling ovary can be detected, nor is there any trace of round ligament or broad ligament. The cut surfaces of the mass, divided in

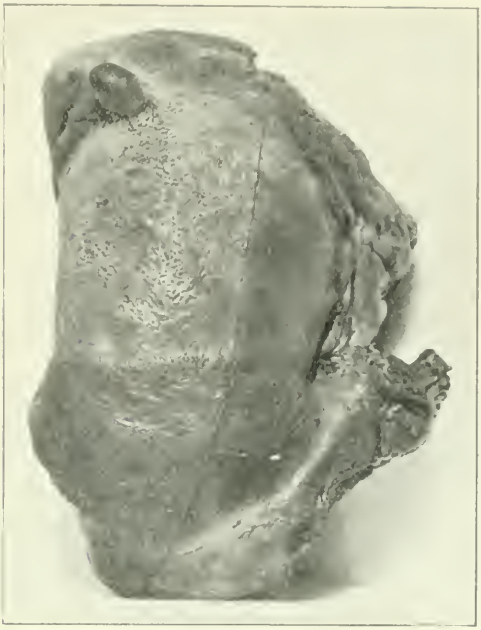

FIG. 1.
The specimen as removed, viewed from the front.

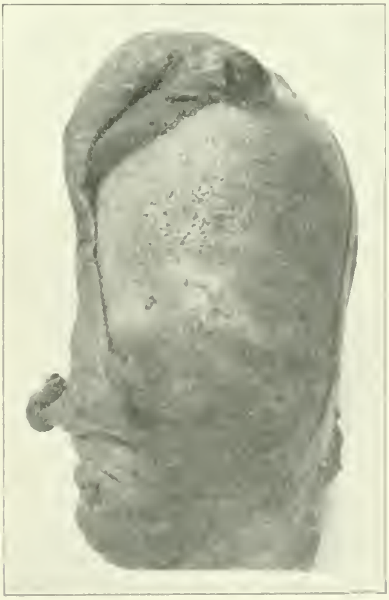

FIG. 2.
The specimen as removed, viewed from the right side.

saggital section, exhibit a series of whitish encapsuled areas which resemble small fibroids, but lack the characteristic whorled appearance; they vary in size from that of a pea to that of a small nut. These areas are separated from one another by septa, which are continuous with a definite cortical layer everywhere present, but varying in thickness. The microscopical characters of the central mass are as follows: The cortical area previously referred to is seen to consist of fibro-muscular tissue, the muscle being scanty and irregularly scattered among the fibre bundles. This area is sharply marked off on account of its difference in structure from the underlying tissue, which consists of numerous areas irregular in shape, and unequal in size. These areas are separated from each other by a fibrous network which exhibits no trace of muscular tissue, but which is, however, practically continuous with the fibro-muscular layer. This continuity can only be demonstrated occasionally, but the accompanying micro-photograph demonstrates this fact (fig. 3). The spaces are beset with tubules cut across in different directions. Each tubule has a definite, unbroken limiting membrane stained red by Van Gieson's stain. The membrane supports the lining cells, which are irregularly columnar or polygonal. These cells in some tubules constitute a single peripheral layer resting closely on the limiting membrane, but in others they are more numerous, bridging across the lumen, and in some cases almost obliterating it. As the lumen is approached all outline of cell body, rarely clearly defined, vanishes entirely, the appearance being that of nuclei surrounded by faint fibrils, which feather off and finally disappear. In some tubules the cells are detached from the membrane and lie free in the lumen; but this effect may be artificial, and due to post-mortem changes. In no instances do the cells transgress the limiting membrane, nor is there any indication of small cell infiltration around those tubules in which proliferation has led to almost total occlusion. Lying outside the tubules, and separating them from the fibrous septa, are occasional groups of cells whose polygonal outline is very definite. Their significance is difficult to determine. They may be due to the fact that the section has passed through the cells lining the lumen of a tubule, as a vertical section through the wall of a house would give a view of the bricks but would enable nothing to be seen of the interior. In this case the type of cells should be similar to that found in the tubules. On the other hand, these cells may have no connexion with the tubule, but be a definite mass of interstitial cells lying in the stroma (fig. 4). A section through the fold on the right of the central mass demonstrates the existence of a definite unbroken tube, lying in its free margin. To the naked eye this had no lumen, nor could the finest bristle be passed along it. It is seen to consist of layers of circular and longitudinal muscle fibres lined by a single layer of large cubical cells which are apparently not ciliated. The lumen is filled with secretion, and in places encroached upon by small bud-like invaginations which are apparently sections of longitudinal folds :(fig. 6). The small knob-like projection previously mentioned exhibits essentially the same structure as that of the tubule, and is obviously a small cyst-like dilatation in connexion with it (fig. 7). Arguing from analogy, the left fold is structurally similar, though, owing to the extreme friability of the tissue and the shortness of the fold I have been unable to obtain a satisfactory section. The outstanding features appear to be :—

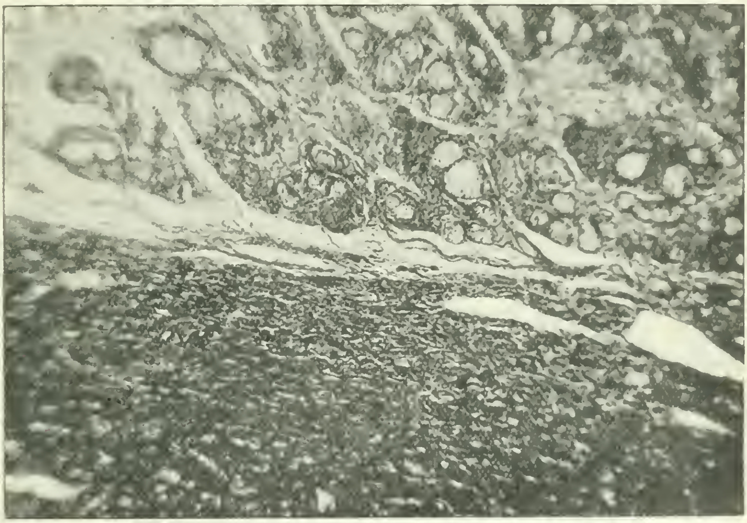

Fig. 3.

Photomicrograph showing continuity of cortex with fibrous septa of glandular portion.

(1) Apparent complete absence of normal genital glands.

(2) Absence of normal Fallopian tubes.

(3) Absence of round ligament.

(4) Absence of broad ligament.

(5) The central position of the removed mass and its relation to bladder and rectum.

(6) Its shape and size somewhat resembling that of the uterus.

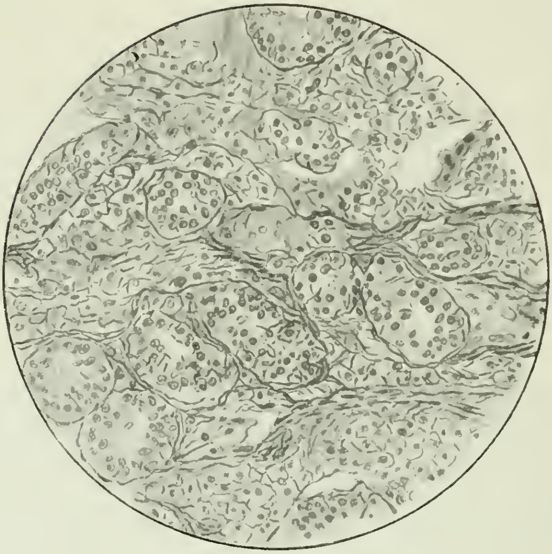

FIG. 4.

Drawing (camera lucida) showing structure of glandular portion.

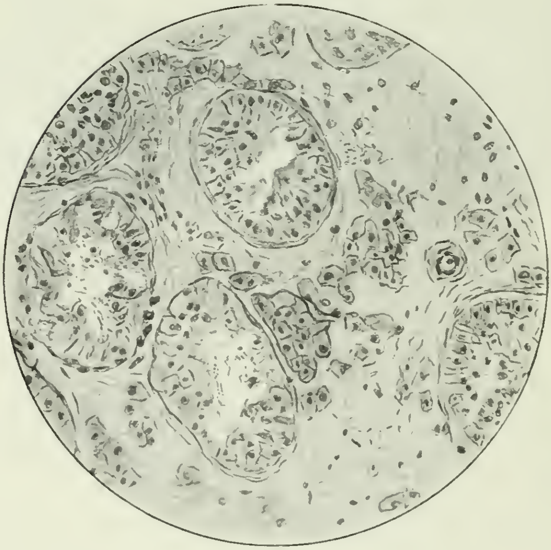

FIG. 5.

Drawing (camera lucida) showing similar tubules and interstitial cells from section of undescended testicle.

(7) Its definite division into (*a*) cortex, (*b*) glandular portion.

(8) Recurrent attacks of pain partially periodic and presumably connected with the genital system.

(9) Almost total absence of vagina associated with

(10) Marked secondary sexual characters—for example, build, appearance, breasts.

(11) Normal position and development of both ureters and kidneys.

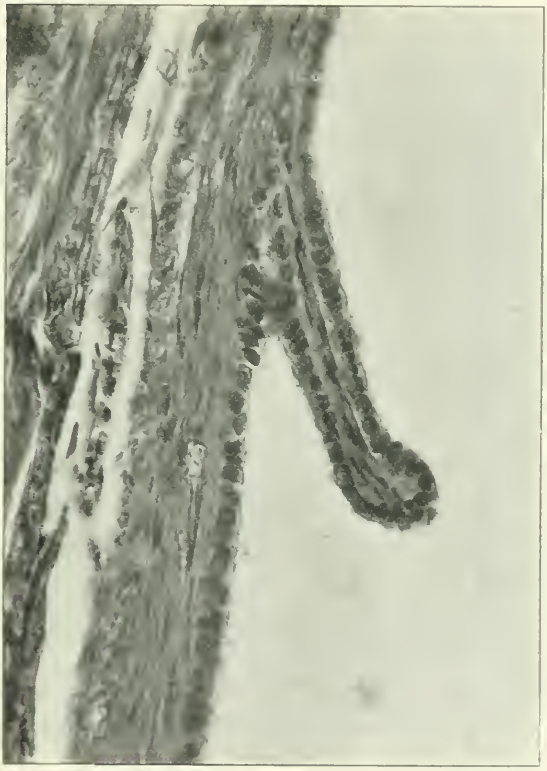

FIG. 6.

Photomicrograph of wall of fold highly magnified to show type of lining epithelium.

With regard to the specimen thus described two problems require solution: (1) the nature of the central mass; (2) the nature of the flanking tubular structure, with its terminal cyst-like dilatation.

The structure of the central mass, with its tubules and characteristic interstitial cells, indicates clearly a testicular origin. A precisely similar structure is seen in functionless undescended testicles, the interstitial cells being much more numerous than in the normal functional organs (fig. 5). The central position of this testicular body and its relation to bladder and rectum is an outstanding feature of the case. It may be explained by assuming that a maldeveloped testicle has followed the track of the genital cord, or that a testicular "rest" has become embedded in its mesoblast. The relation of the genital cord to bladder and rectum is shown in fig. 8, which is a photomicrograph of a section taken from a nine weeks' human fœtus. The section has been kindly lent me by Dr. R. J. Gladstone, of the Middlesex Hopital. The Müllerian ducts have refused to form a single tube lying in the middle line, while the Wolffian ducts are seen on either side.

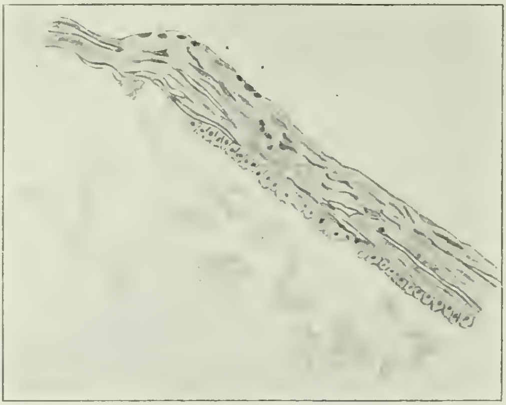

Fig. 7.
Drawing (camera lucida) to show structure of wall of cystic dilatation.

The significance of the flanking folds is not so easily determined. Arguments may be adduced in favour of their origin from either Wolffian or Müllerian duct. In the male the Müllerian duct is normally represented by the sinus pocularis and the hydatid of the testicle, the intervening portion undergoing total atrophy. In this case the diminutive vagina may represent the lower ends of the Müllerian ducts. On the right side the upper end may be represented by the small cystic dilatation corresponding to the hydatid of the testicle. Between the upper and lower ends some atrophy has taken place, but a portion of the duct has persisted in the form of the flanking fold. The conditions on the left side are due to more complete atrophy. On the other hand, the close association of these folds with the genital gland, the testicular nature of the gland, and the fact that no other indication of its own proper ducts is present, suggest that the folds are representatives of the Wolffian ducts. That the early development of the Wolffian ducts was normal is indicated by the presence of normal kidneys and ureters. The Wolffian hypothesis is the more likely, and in my opinion the correct one.

Many cases have been recorded in which male internal genitalia have been associated with more or less normal female external genitalia, but I have been unable to find a case which presented the remarkable features exhibited by the foregoing.

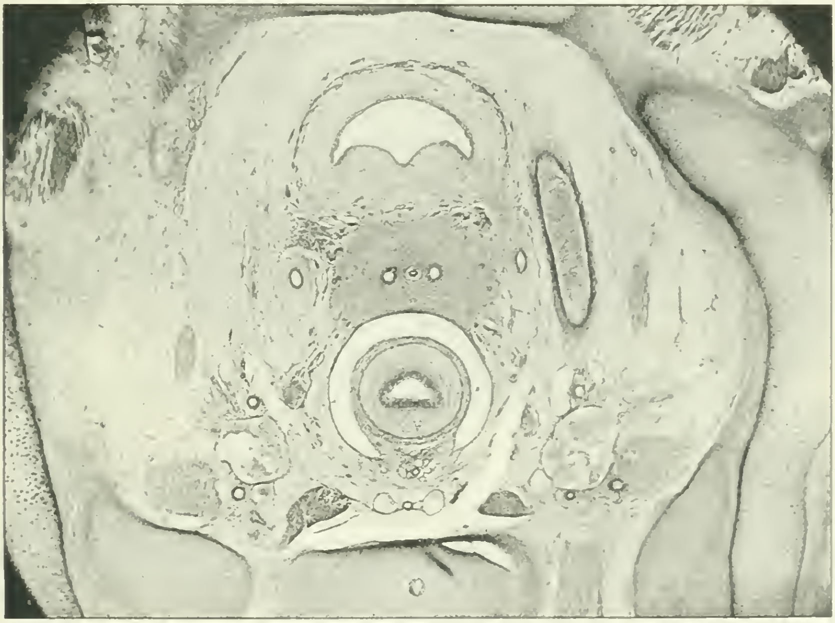

FIG. 8.

Photomicrograph to show relation of genital cord to bladder and rectum.

I am indebted to Professor Robinson, Mr. Targett, Dr. Bryce, and Dr. Stevens for expressions of opinion; and to Dr. Teacher for the loan of sections.

The PRESIDENT (Dr. Macnaughton-Jones) said that this appeared to be a case of pseudo-hermaphroditism, but some doubt existed as to the structure of the testicular body. The whole subject was one of great interest, but as to the embryological explanation of the sexual malformation they were as yet more or less in a cloud. There had been no more exhaustive paper published in this country than that read by Dr. Blacker and Mr. Lawrence before the Obstetrical

Society in 1896,[1] when they brought their case forward; they collected a complete list of thirty-one cases previously recorded, with analyses of their sexual characteristics. From that time to the present the most valuable contribution to the subject was that of Mr. Lawrence in the *Transactions of the Pathological Society* in 1906.[2] Accepting Klebs' division into "lateral," "unilateral," and "bilateral," and the essential co-existence of specific tissue of ovary and testes in the same individual, the indisputable claim to instances of true hermaphroditism, proved both anatomically and histologically, could only be advanced in comparatively few cases, perhaps not more than seven in all. He (the President) had brought for exhibition a specimen of an eight months' monster which he had shown at the Obstetrical Society. It was removed alive from a fibroid womb after hysterectomy. Here the internal genitalia were complete, while the male organ was perfect, and all the external appearances were those of a male child: but the testes were absent. He might refer to a remarkable case shown at the Australasian Medical Congress of 1908[3] by Mr. Arthur Palmer. The child had been brought up as a girl until it was aged 13. Previous to its death, at that age, a doubt had arisen as to its sex. In this case all the external characters were male, and it was thought to be a male child with undescended testes. At the post-mortem, however, it was found that the pelvic organs were those of a female, but there was a well-marked prostate gland, which was traversed by the vagina, which opened into the urethra in a sinus pocularis. Here, again, the testicles were absent.

Columnar-celled Carcinoma of the Ovary.

By Lieut.-Col. Dimmock, I.M.S.

This patient was a young unmarried woman, aged 22, who on admission had a painless uniformly rounded solid tumour in the hypogastrium, extending to ½ in. below umbilicus. She gave a history of having noticed a lump in the right iliac region two and a half months before admission. The menstruation was normal, except during the last month, when she menstruated twice within the month. It was diagnosed as a "possible carcinoma of the right ovary."

At the operation a large tumour was seen to arise from the right broad ligament extending upwards and to the left up to an inch beyond the umbilicus. The tumour was removed. The left ovary was found to have three follicles projecting on its surface, which were punctured.

The pathological examination pronounced it to be an "alveolar sarcoma." I should be glad of an opinion on the nature of the tumour.

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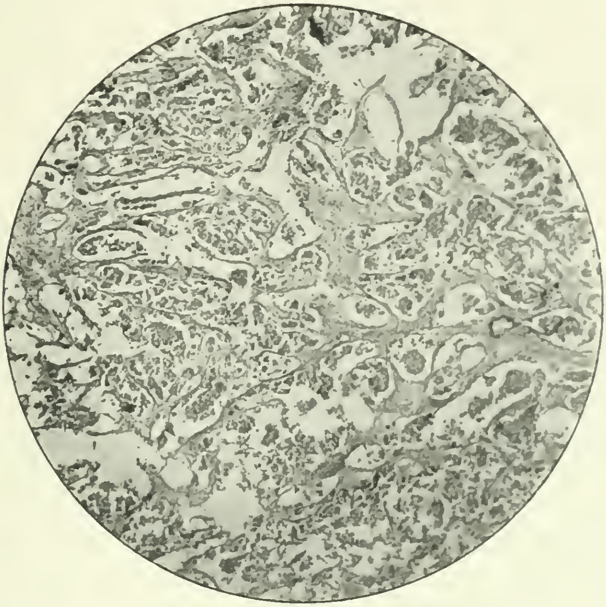
[1] *Trans. Obstet. Soc. Lond.* (1896), 1897, xxxviii, p. 265.

[2] *Trans. Path. Soc. Lond.*, 1906, lvii, p. 21.

[3] *Trans. Australas. Med. Congr.* (Melbourne, 1908), Victoria, 1909, ii, p. 209.

Report of Pathology Committee.—"We are of opinion that the sections exhibited by Lieut.-Col. Dimmock as sarcoma of the ovary have all the characters of a columnar-celled carcinoma of the ovary, which we consider the tumour to be."



Columnar-celled carcinoma of the ovary.

Fibromyomatous Polypus of Uterus in a Girl aged 17.

By George Blacker, M.D.

The occurrence of a fibromyomatous tumour in a patient below the age of twenty-five years is so uncommon that I thought the following case worthy of being placed on record. The patient, A. M., a nullipara and single, was admitted into University College Hospital in September, 1906, complaining of continuous hæmorrhage from the vagina, which had been present for two months, and an offensive discharge which she had noticed for one month. Her general health had always been good, and her periods, which commenced at 15 years of age, had occurred regularly at intervals of four weeks and had lasted for seven days until three months before her admission. She had not lost flesh and complained of no pain.

The patient was a healthy-looking but anæmic girl, aged 17. Abdominal examination revealed nothing abnormal. On vaginal examination a polypoid mass, measuring about $2\frac{1}{2}$ in. by $1\frac{1}{2}$ in. in diameter, was found in the vagina projecting from the cervical canal. Its surface, greyish red in colour, was breaking down and ulcerated. Its consistence was firm but not hard, and its pedicle could be traced up through the cervical canal and was inserted on the posterior wall of the uterus just above the internal os. Under an anæsthetic the pedicle was divided with scissors, the tumour removed, and the uterus curetted. No other tumours could be detected in the uterine walls, and its cavity was only a little elongated. The patient made a good recovery, and at the present time, four years after the operation, is in good health.

Microscopic examination shows the tumour to have the appearance of a rapidly growing œdematous fibromyoma, containing numerous blood vessels. In parts there is a good deal of œdema and hyaline degeneration, but the undegenerated portions of the tumour show numbers of spindle cells with large nuclei and well-marked nucleoli, and numerous blood vessels with well-formed walls distinct from the tissue of the tumour. The mucous membrane covering the tumour has the structure of that of the body of the uterus, and is somewhat hypertrophied.

Very few fibroid tumours of the uterus have been recorded in patients of this age. In a paper read before the Obstetrical Society of London,[1] by Dr. Herbert Spencer, in which an abstract is given of forty cases of fibroid tumours occurring in women under twenty-five years of age, there is only one case in which the patient was younger than this girl. It is a case recorded by Howitz, in which the patient is said to have been only thirteen years of age; but, as Dr. Spencer points out, it is very likely that the tumour was in reality ovarian in origin. There can be no doubt, therefore, that the presence of a fibroid tumour, proved to be such by microscopic examination, in a woman before the age of twenty years, is an exceedingly rare occurrence.

Report of Pathology Committee.—"We have examined sections of a fibromyomatous polyp removed from the uterus of a patient, aged 17, exhibited by Dr. Blacker, and agree with the description of the author."

Sarcoma of the Vulva.

By Arthur H. N. Lewers, M.D.

E. R., A MARRIED woman, aged 59, was admitted into the London Hospital under Dr. Lewers's care on July 7, 1910. She had had one child twenty-nine years ago, and no miscarriages. The menopause occurred at the age of 46. Since then she had "seen nothing." She complained that there was "something wrong with the front passage," also of pain on sitting down, and of a discharge. She was quite certain that there had been nothing wrong up to nine weeks prior to her admission into the Hospital. Since then she had noticed the symptoms above-mentioned.

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[1] *Obstet. Soc. Trans.*, xl, 1898, p. 228.

Examination of the abdomen discovered nothing abnormal. One definitely enlarged gland was felt in the right groin. On examination of the vulva, a hard, irregular, dusky red mass, the size of a hen's egg, was seen projecting from the posterior two-thirds of the right labium majus. The growth had a sharp overhanging edge, and the surface was covered by a thin, offensive, slightly blood-stained discharge. Clinically, there was nothing to distinguish the case from one of carcinoma of the vulva.

Operation (July 12, 1910): The patient was anæsthetized, and placed in the lithotomy position. The growth was held up and dissected out freely with Paquelin's cautery, the line of incision being separated by a good width of apparently healthy tissue from the edge of the growth. Posteriorly, the incision passed just in front of the anus and a short distance up the posterior vaginal wall. Several bleeding points were controlled by understitching with catgut. The wound was dressed with gauze soaked in 1—20 carbolic oil. The lymphatic glands were then removed from both groins. The enlarged gland from the right groin was seen on section to be obviously affected by secondary new growth. This was subsequently confirmed on microscopical examination. Both inguinal wounds were drained by a small tube projecting at the inner end of each.

After-progress: On the third day fæcal matter came away from the vagina; this was found to be due to a small recto-vaginal fistula, 1 in. up the vagina, caused no doubt by the separation of a slough made by the cautery. The fistula closed spontaneously before the patient left the Hospital. At the end of three weeks from the date of the operation all the sloughs had separated, and the vulvar wound showed healthy granulation tissue with the epithelium spreading in at the edges. Convalescence was apyrexial throughout.

Dr. Lewers said that he considered removal of malignant growths of the vulva by the cautery, followed by sloughing and healing by granulation, gave the patient a better chance of non-recurrence than removing them with the knife, and healing by first intention.

Dr. Lewers saw and examined the patient on November 3, 1910.

when she came up to the London Hospital to report herself. The vulvar wound was soundly healed, as also were the wounds in each groin, and nothing abnormal was to be found. The patient expressed herself as feeling quite well.

MICROSCOPICAL EXAMINATION OF THE GROWTH.

Four sections were prepared by Dr. R. D. Maxwell, Obstetric Registrar to the Hospital. Sections 1 and 2 were through the edge of the primary growth; section 3 was of the gland in the right groin; and section 4 from a gland in the left groin.

Dr. Maxwell reports sections 1 and 2 as showing the structure of a large oval and spindle-celled sarcoma of the subcutaneous tissues of the vulva. He says: "A few of the sarcoma cells are multinucleated, and a few patches of necrosis are seen, but the main mass of the growth is extremely uniform in appearance. The growth is very vascular. It extends up to and ulcerates through the skin, the deeper layers of which in the vicinity are normal. No trace of pigmented growth is seen."

As regards section 3, he says "it shows the presence of secondary sarcoma in a lymphatic gland. The gland shows a rounded nodule of growth with a fibrous capsule. In this nodule the cells are much more loosely set, and are rather more elongated than in the primary growth. Numerous red blood cells are seen in the meshes of the growth."

As regards section 4, "the section from the gland in the left groin shows no evidence of sarcomatous deposit."

Report of Pathological Committee.—"We have examined the sections of a case of sarcoma of the vulva submitted by Dr. Lewers, and agree with the author's description."

Melanotic Sarcoma of the Vulva.

By CLIFFORD WHITE, M.D.

THIS tumour was removed from a single woman, aged 44. The symptoms complained of were discharge from the vulva and slight pain on micturition, which had been present for three months. The patient herself had not noticed any swelling or pruritus. On examination there was a purple ulcerated mass, $1\frac{1}{4}$ in. in diameter, growing from the labium minus on the right side. The growth involved the clitoris and extended outwards to the inner border of the labium majus, but was not fixed to the bone. The vagina and pelvic viscera were healthy. No glands could be definitely felt in the groins owing to the thickness of the subcutaneous fat. The liver and lungs were healthy. No moles were present on the skin of the vulva. A note was made of the peculiar colour of the growth, which was thought to be an atypical epithelioma. I removed the fat containing the lymphatic glands from the inguinal regions and Scarpa's triangles, and then continued the incisions to embrace the clitoris, thus attempting to remove the growth and glands without cutting across the lymph tracts. The patient did well till the fifth day, when she died suddenly with all the classical symptoms of pulmonary embolism. I was unable to obtain an autopsy.

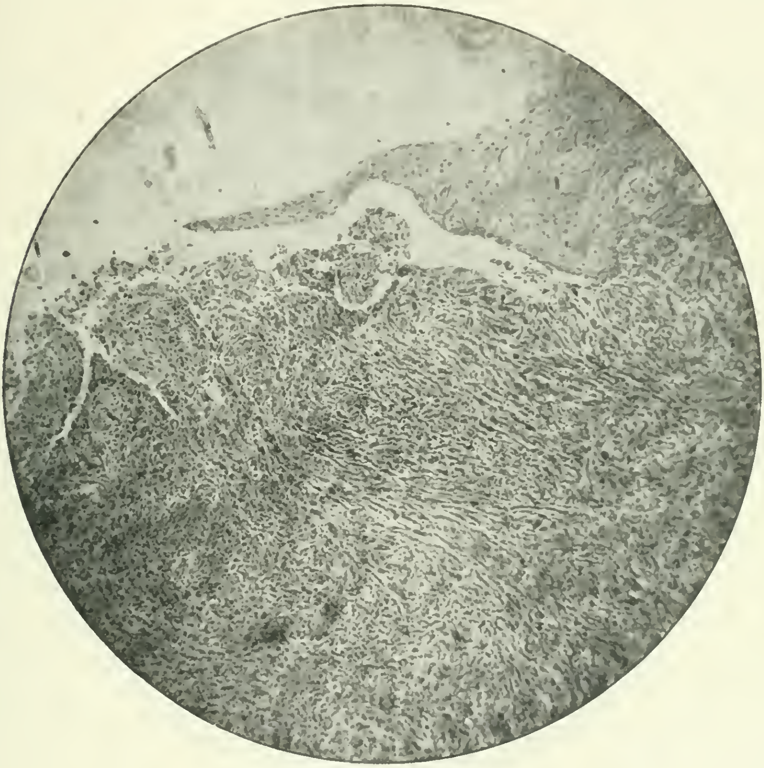

FIG. 1.

Microscopical section of the tumour.

The specimen consists of the clitoris and surrounding skin, which is infiltrated by the growth, with the glands from both sides. The glands from the left side and the inguinal glands from the right side are normal, but one of the femoral glands on the right side is enlarged by new growth. It is $\frac{3}{4}$ in. long and of a dark purple colour. Sections of the primary growth show that the skin is ulcerated, the papillæ being eroded by a growth which appears to have originated deep to them. This growth contains melanin granules and has the usual characters of the tumour commonly called a melanotic "sarcoma." Sections of the gland show it to be infiltrated by malignant cells containing a large quantity of melanin.

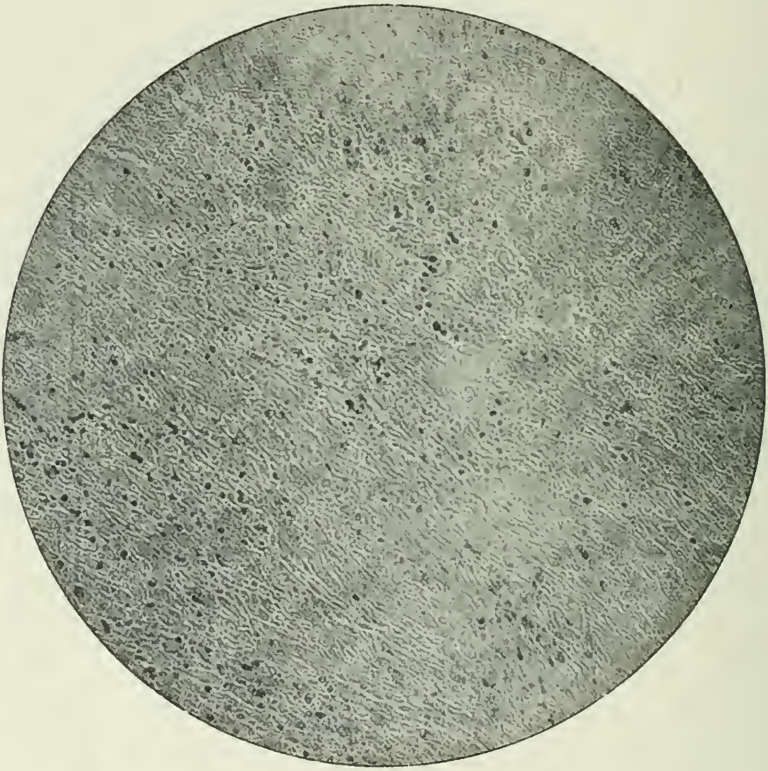

Fig. 2.

Section of gland showing melanin granules.

Report of Pathology Committee.—"We have examined fresh sections of the tumour exhibited by Dr. Clifford White as 'melanotic sarcoma of the vulva,' and are of opinion that the tumour, like some of those recently described, presents certain histological difficulties, but on the whole appears to be rather of connective-tissue than epithelial origin."

Obstetrical and Gynæcological Section.

December 1, 1910.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

On the Pathological Bases of Operations for Cancer of the Uterus.

By Archibald Leitch, M.B.

Eight years ago a famous surgeon said that "no radical operation is worth performing when the disease extends beyond the limits of the uterus into the connective tissue about the cervix or into the utero-sacral or broad ligaments. In such cases prolonged immunity, to say nothing of the permanent benefit from an operation, is out of the question." We have travelled far in these eight years; the brilliant results that have attended advanced operations, such as that of Wertheim, have quite changed the comparatively hopeless outlook. The surgery of cancer in general is advancing rapidly, and one would be rash to delimit its future scope. In a very short time what was considered the limit of operation has been surpassed, and what was formerly a "radical" operation becomes in the light of experience incomplete. Instead of appealing to results which may take years to accumulate, we can base our judgment of the probable completeness of any operative technique on the surer ground of pathological observation. The more the surgical pathology of cancer is appreciated, with a consequent adaptation of operative procedure, the better will the outlook become; but the first essential in advance is a more accurate knowledge of the pathological processes at work.

No one would deny the utility of earlier diagnosis of cancer of the uterus and the educative measures proposed for that purpose; but if earlier diagnosis implies restricted operative measures, then there is danger of retrogression. How is it possible to ascertain when we have to deal with an early case?

Early with regard to time of duration may be late in respect to time of dissemination. The warning signs and symptoms that call attention to cancer of the uterus vary greatly in their severity, and we cannot calculate with any degree of exactness how long the lesion has existed. The evolution of some cancers is rapid, of others slow, and of others again rapid growth and slow growth may follow each other. The rate of growth of malignant tissue follows no laws that we know. The apparent size of a primary lesion visible to our eyes or demonstrable to palpation may be far from the actuality. Probably every surgeon has had the experience of removing small growths so thoroughly in relation to their size as to give every hope of success, only to find a speedy demonstration of failure, and, on the other hand, has met with large growths, treated in a relatively restricted manner, which have never recurred. Experience in post-mortem examination soon teaches us that a small focus of origin is by no means infrequently associated with a very extensive dissemination, and, on the contrary, that the malignant process may be locally confined to the site of its origin. In clinical examinations it follows that we cannot invariably, or even usually, say that a small original lesion denotes an early case, for beyond that lesion the cancer cells, unseen and impalpable, may be permeating the lymphatic tracts. In cancer of the uterus, of the cervix, or of the body, another process may be at work—septic absorption—which may considerably complicate a judgment of the real extent of the cancer, leading one to conclude that the cancerous process is extensive when in reality it may be limited. I shall refer to this later.

The histological examination of a portion of the growth cannot give us sure information with regard to the rapidity of the process. I am aware that such a statement is at variance with the views of some pathologists. It is not uncommon to read in histological reports that the tumour is growing rapidly or slowly, or even that the case is considered to be an early one. Such an opinion takes no account of certain fallacies—namely, that a particular section of a certain portion at a particular time may exhibit considerable variation from the appearances found in another section from an adjacent part. It is based on two considerations, the relative amounts of connective-tissue stroma and epithelial cells, and on the numbers of mitotic figures found in the portion examined. In the experimental investigation of mouse cancer, where we can take all these factors into account, we find that the histological criteria ōn which such opinions are founded are quite untrustworthy. We actually find that some tumours with a relatively large preponderance of stroma over epithelial elements may have a speedier growth and a higher percentage of successful inoculations than a tumour of high epithelial cellularity. Of course, the opposite may obtain; there is no relationship between structure and degree of malignancy. It has also been shown that some slow-growing tumours of mice may exhibit a surprising amount of mitotic figures.

Taking all such factors into consideration, the impossibility of determining from clinical or histological examination the exact extent of the disease in any given case, we may justifiably conclude that the only way of encompassing a radical extirpation, within reasonable limits of course, is to apply to every case a technique which is designed for the most advanced. Such a technique does not mean a haphazard removal of surrounding tissue, but an extirpation of definite danger zones. The proposition which I wish to submit for your consideration to-night is that these danger zones are the structures which in the autopsies of advanced cases we actually find the most often affected; that is to say, the terminal picture gives the key to the partially-revealed processes during life.

In the fifth report from the Middlesex Hospital Cancer Research Laboratories[1] I published a statistical article on "Cancer of the Uterus," compiled from the records of that hospital, and from the large number under review I was able to choose 915 complete post-mortem accounts of cancer of the cervix. These autopsies were performed and recorded by several pathologists, so that when collected together they gave us perhaps a more unbiased view of the salient facts than would the records of one man. They are supplemented by the records of over a hundred further autopsies performed by the special cancer research staff, and collected by Dr. MacCormac.

One of the most striking and unexpected features revealed by my figures—and Dr. MacCormac's statistics exactly agree with them—was that no less than 55 per cent. of cases were found to be free from any visceral or glandular metastasis. If we accept that as a true indication of the potential spread of the disease, and make allowances for the fact that in earlier stages the liability to metastasis must be lower, we can account for the effectiveness claimed for such restricted operations as the Schauta-Schuchardt vaginal hysterectomy or other measures that leave the pelvic glands untouched. But though these figures may be accepted as fairly accurate for extra-pelvic metastases, it may be doubted if they give a true estimate of the frequency with which the pelvic lymphatic glands are involved. It must be borne in mind that in approximately a quarter of the cases that come to the post-mortem table the pelvic organs are so matted together, either from uterine and tubal septic processes or from perforation of the pouch of Douglas, that accurate descriptions of the state of the intra-pelvic structures is out of the question. The lymph glands lying between the external and internal iliac arteries would therefore probably escape examination in a great many cases. The pelvic contents in advanced cases with perforation, disorganization, and fœtid débris, do not invite long and painstaking examination, nor do our usual methods for their removal enable us to be exact on the point. Again, the utility of exact observation of the intra-pelvic glands has only recently been evident. If these glands have been enlarged and have escaped examination, it would be impossible to say whether they were the seat of deposit or the result of the septic processes. But, at any rate, the observations can be taken as fairly accurate of the dissemination beyond the brim of the pelvis, and up to that point glandular extirpation is practicable in the modern operation. When cancer gets beyond the wall of the uterus into the paravaginal tissues and broad ligaments it travels to the lateral walls by lymphatic tracts. Most of our anatomical text-books are very incomplete in their descriptions of these tracts. Poirier, Delamere, and Cuneo mention three tracts from the cervix: one directed outwards draining into the inter-iliac lymph glands, a second running obliquely outwards and backwards to glands on the various branches of the internal iliac, and a third descending at first and then ascending to the pararectal glands. The last set course through the utero-sacral ligaments, but it is questionable if they are really uterine lymphatics; they are not reckoned as such in our English text-books. And yet how commonly do we find in cases of cancer of the uterus the clinical note, "involvement of the sacro-uterine ligaments"! How many cases are denied operation on this ground alone! I have often been struck with the fact that where this was recorded in the clinical notes no apparent involvement of these folds could be found at post-mortem examination. The records to which I have alluded were made before the anatomy of the lymphatic system was as well recognized as it now is, but, still, involvement of the lateral sacral glands into which these lymphatics drain would occasionally have been noticed; but there is no mention of such an occurrence. In this case the matting of the pelvic structures would not hide them. In Dr. MacCormac's paper they were found to be affected once only in 107 cases. Personally I have never seen them enlarged in uterine cancer. In an operation specimen of my colleague, Dr. R. C. Buist, this particular tract of tissue was removed because it was considered to be involved, but careful microscopic examination of various parts of it failed to demonstrate any cancer cells. I should not like to deny the presence of such a lymphatic tract, much less to say that "involvement of the sacro-uterine ligaments" is always a false observation; but, in view of the disagreement between clinical and post-mortem findings and the fear that some otherwise operable cases may be denied operations on that account, I think too much has been made of it. Lymphangitis from uterine septic trouble, closely following these sacro-uterine tracts and embracing the rectum, has been observed by Ernest Miles, and is considered by him to produce the so-called "syphilitic stricture of the rectum."

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[1] *Arch. of Middlesex Hosp.*, 1906, vii (Fifth Report from Cancer Research Laboratories), p. 165.

The middle tract of Poirier draining into lymph glands on the branches of the internal iliac is omitted from most text-books. I have been unable to verify this tract by injection, and in the few complete dissections I have made of pelves with cancer of the uterus I have failed to discover enlarged glands in the situation figured. More investigation of this is required. Supposing the observation of Poirier and his collaborators is verified, it will mean that, if nothing is to be left to chance, probably the anterior division of the internal iliac would have to be cleaned out, if not removed at operation. At the present time we have no evidence that these lymphatic glands are frequently the seat of cancerous deposits.

The inter-iliac glands are important (fig. 1). They vary in number, usually three, and are supplied by lymphatics from both the cervix and the corpus. In some books they are figured as being supplied from the cervix only, the principal tract from the body running over the end of the common iliac and turning up to the lumbar glands; but the two tracts are very intimately connected, and as a matter of fact we find the inter-iliac glands not infrequently affected in cancer of the uterine body, while, on the other hand, we sometimes find the lumbar glands affected in cancer of the cervix with no apparent involvement of the inter-iliac glands. The inter-iliac glands, as their name implies, lie between the external and internal iliac arteries, the uppermost being usually partly hidden by the ureter which passes over it. These glands in turn communicate by a connexion over the artery with another set of three or four which lie in close association with the outside of the common iliac artery (fig. 2). The lowest gland of the latter is very apt to escape detection, as the artery may have to be displaced inwards to show its presence even when it is enlarged. Enlarged glands do not necessarily indicate invasion by cancer, for we not infrequently fail to detect any epithelial cells in such cases, whilst, on the other hand, glands may be infected without being enlarged or even hardened; still, it may be taken as a working rule that an enlarged gland is prima-facie evidence of cancerous deposit, and should be removed, whilst the routine extirpation of pelvic glands in cancer of the cervix obviates the risk of leaving behind a focus for recurrence. When the inter-iliac glands are definitely enlarged it will obviously be risky to stop short at their extirpation, and

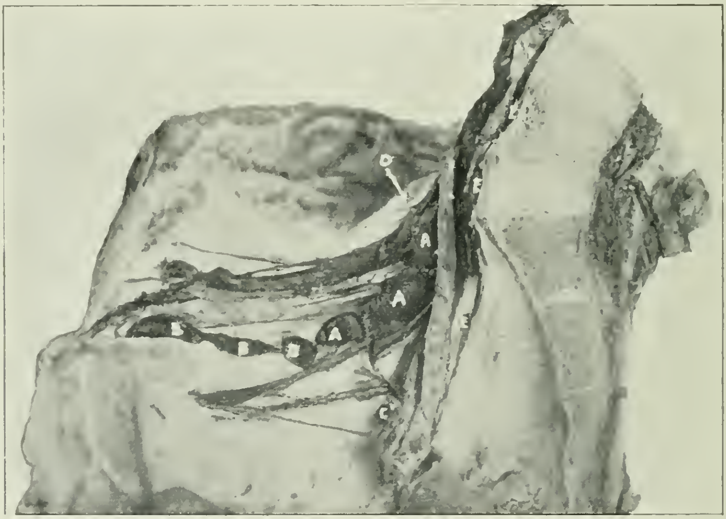

FIG. 1.

Dissection of pelvis in a case of cancer of the uterus to show the lymph glands. **A A A**, the inter-iliac glands, the uppermost being crossed by the ureter. The arrow, **D**, points to the communication between the highest inter-iliac gland and the lowest gland of the common iliac group, which lies to the outer side of the artery. **B B B**, glands lying below the external iliac vein communicating above with the inter-iliac glands and below with the inguinal glands. **E E E**, the lateral sacral glands and lymphatic tract—not involved.

the higher set lying to the outer side of the common iliac, especially the gland which I have mentioned as being hidden behind the artery below its bifurcation, should be removed also. The most distal enlarged gland does not necessarily mark the limit of dissemination any more than the visible surface margin of a growth denotes the extent of invasion. The lesson has been learnt and successfully applied in the surgery of breast cancer, and the same principle should be applied to cancer of the uterus; the area through which the common iliac and its divisions run is the "axilla" of the pelvis. Closely connected with the inter-iliac group of lymph glands, if not actually to be reckoned one of them, is what is normally a large gland lying between the external iliac vein and the obturator nerve. This gland can be injected from the cervix, and from it the obturator gland may become affected in cancer of the uterus. In a few advanced cases—approximately 5 per cent.—we find the inguinal glands to be enlarged, but probably in a very small number only is that enlargement due to cancerous infiltration. There is said to be a lymphatic tract from the uterus through the round ligament to

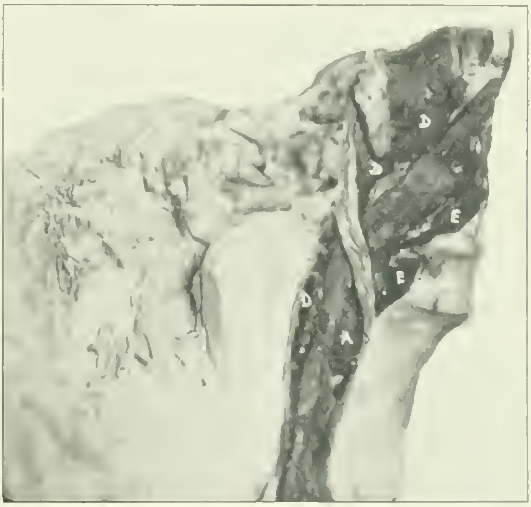

FIG. 2.

Half of dissected pelvis in case of cancer of the uterus, anterior view. **A**, the highest inter-iliac gland, partly hidden by the ureter. **D D D**, the common iliac glands; the lowest gland lies on the opposite side of the external iliac artery from **A**. The artery has been displaced inward in order to show this gland. **E E**, two lateral sacral glands.

the inguinal region, but though I have frequently prepared microscopic sections across the ligament, I have not been able to see invaded channels within it. That does not, of course, negative such a route of dissemination, for there may be embolic spread to the inguinal gland. But it is possible that the inguinal glands may become involved from the chain of glands lying along the external iliac vein which communicate with the interiliac group (fig. 3). It must be remembered that lymphatics permeated by cancer-cells do not coincide with the channels mapped out by artificial injection. In their growth the cells block the lymphatics, destroy the lymph current, and make their way by the paths of least resistance through tributary channels, it may be, against the direction of the lymph flow.

Contained within the broad ligaments are the tracts from the uterus to the pelvic glands; they are numerous and extensively communicating. Parallel to the long axis of the uterus and close to it is a vertical channel of communication between the lymphatics of the corpus and those of the cervix, so that for all practical purposes in cancer of the uterus the lymphatic spread outside the walls of the organ is the same for both. It may be that it takes longer in general for cancer to penetrate the wall of the body than it does to pass beyond the confines of the cervix (I doubt it), but once it does so the course is the same for both; but it is open to question, especially when we take into account the dangers of wide implantation and embolic dissemination entailed by the preliminary diagnostic curetting usually practised, if cancer of the body should be treated by less extended operation than cancer of the cervix. (The average duration of life in cases of cancer of uterine body is 1·83 years, compared with 1·74 years in cases of cervical cancer. Lymphatic glands are involved in 45 per cent. of cases. Viscera are affected by deposits in 48·5 per cent. Hydronephrosis occurs in 33 per cent.) The involvement of the parametric tissues may only be revealed by the microscope, and even escape detection altogether. Embolic involvement of the lymph glands must be admitted, but for practical purposes the tissues must be treated as if there were always continuous lymphatic spread within them. There are no means of determining at operation which of the two modes of dissemination is at work in a particular case, and even in embolic spread the emboli may still be in course of passage at the time of intervention. It follows, then, that in removing the dangerous structures glands and broad ligaments should be taken away in one continuous piece, for otherwise one runs the risk of producing artificial grafts of cells with the knife.

The ureter passes through a danger zone. Round it in its course through the broad ligament the lymphatic tracts pass on their way to the pelvic walls, and close to it is a knot of these channels—the juxta-cervical knot—which, when involved, often appears like an affected gland. By contraction of the fibrous tissue in the cancer-invaded parametrium, by fibrotic stricture consequent on secondary parametritis, and by infiltration of the wall by direct spread of cancer, or by lymphatic permeation, the ureter becomes constricted and its lumen obliterated. The result is hydronephrosis and all its train of baneful effects ending in death from uræmia, one of the most common terminations of the disease even though the hydronephrosis be unilateral. I have shown, too, that the ureter may be invaded without hydronephrosis. My statistics show that hydronephrosis occurs in no less than 75 per cent. of all cases; Dr. MacCormac's statistics give 73 per cent. These figures are sufficient to show the liability of the parametrial portion of the ureter to involvement and raise the question whether the practice of stripping the ureter is the wisest procedure. For a fuller consideration

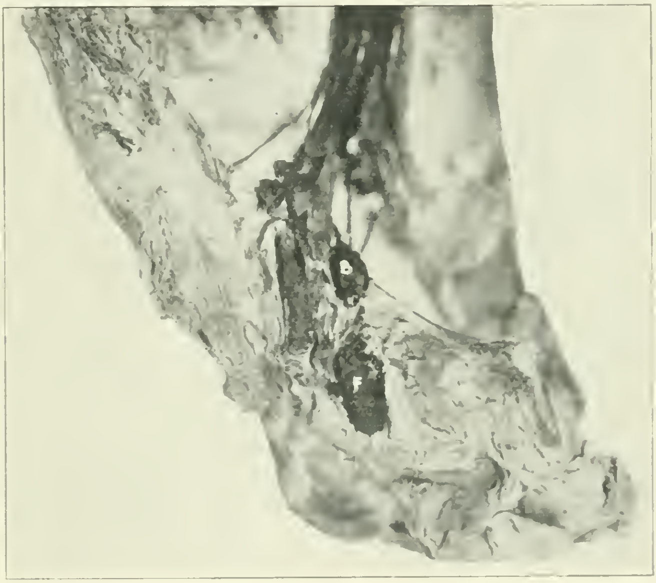

Fig. 3.

Anterior view of dissected pelvis. **F** is an enlarged inguinal gland. **B** is the lowest intra-pelvic gland of a chain communicating with the inter-iliac glands.

of the pathology of the ureters in cancer of the uterus let me refer you to a paper of mine in the *Journal of Obstetrics and Gynæcology* of April, 1910, and to a paper by my colleague, Dr. R. C. Buist, in the same number.[1]

When the cancer originates in the cervix the liability to involvement of the vagina is very great. In our post-mortem cases only a very small number (2·5 per cent.) escape. The involvement may be evident to the naked eye as sub-epithelial nodules or ulcers. Ulceration of the vagina, where it is in contact with a cancerous cervix, is fairly common, and it is usually cited as an example of contact transference or auto-inoculation. Such a process is always open to suspicion. The necrotic condition of the surface of a cervical tumour and the septic condition of the material are almost certain obstacles to tumour grafting. The anastomosis is so rich between the lymphatics of the upper part of the vagina and those of the cervix that downward spread is quite common. Hence an apparently independent nodule or ulcer may be, or may have been, in direct continuity with the parent growth through the underlying lymphatics, though separated from it on the surface by unchanged epithelium. Even where there is no naked-eye evidence of involvement of the vagina we may have it revealed by the microscope. Thus in Wertheim's operation the vaginal cuff made to enclose the septic cervix serves also to remove a highly dangerous tissue. From the vagina, by continuity of tissue, the bladder and the rectum become involved, the former in approximately two-thirds of post-mortem cases with fistulæ in 44 per cent., and the latter in one quarter of the cases with 16 per cent. of perforations. The timely removal of the upper part of the vagina would prevent the occurrence of these distressing and hopeless sequelæ.

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[1] *Journ. Obstet. and Gyn. of the Brit. Emp.*, 1910, xvii, pp. 314, 320.

Twenty-three Cases of Wertheim's Abdominal Panhysterectomy.

By Charles P. Childe, F.R.C.S.

Table A supplies brief details of 23 cases in which Wertheim's operation has been performed in removing the uterus, its adnexa, and the adjacent parametrium. In 19 the operation was undertaken for carcinoma involving the cervix; in Cases 8 and 9 the same disease was situated in the body of the uterus; in Case 10 the condition was diffuse adenomyoma with pyometra, the uterus containing previous to operation about ½ pint of very foul pus, and numerous pus foci being present in the uterine wall; in Case 18 a malignant adenomyoma involved both the cervix and body, the uterus in this instance also being distended with foul pus.

The cases are consecutive and have been selected only in the sense that those which were manifestly inoperable by any method have been rejected. As a reference to the table will show, I have attempted operation in every case in which there appeared to be an outside prospect of completely removing the palpable disease.

I shall first make a few remarks upon each of the headings contained in the table (*see* pp. 80-85).

(1) Age.

I have not rejected any patient on account of age. The oldest operated upon was 65 years (5), there were two of 60 years (9, 20), one of 59 (19), two of 58 (10, 21), and one of 57 (18). Some authorities are of opinion that Wertheim's is too severe an operation for patients of 60 years or thereabouts. This I have not found to be the case. All of these patients, over or about 60 years of age, recovered from the operation; five of the six stood it remarkably well; one only suffered severely from shock, from which, however, she rallied and made a good recovery. The operation in this case was exceptionally difficult and occupied a long time, which, I think, has more to do with shock than the age of the patient.

TABLE A.—BRIEF DETAILS OF TWENTY-THREE CASES

No.	Patients' initials	Age	Medical attendant	Duration of symptoms in months	Examination under anæsthetic	Stage of disease	General condition	Microscopic report (clinical research)
1	H. F.	41	Dr. Mulcahy	9	Large papillary growth occupying vagina; uterus movable	Moderate	Anæmia and loss of weight	Carcinoma
2	G. M. A.	46	—	7	Interior of cervix occupied by growth; uterus partially movable; deposits both laterally and behind	Advanced	Sallow; has lost 1½ st. in weight	Carcinoma
3	A. F.	35	—	8	Papillary growth protruding from cervix; uterus movable; no external deposits	Moderate	Good; thinks she has lost weight	Carcinoma
4	E. A. W.	49	Dr. Wright	15	Interior of cervix occupied by growth; uterus partially movable; deposit on right side	Advanced	Anæmia; thinks she has lost weight	Carcinoma (Mr. Ridout's report)
5	P. M. J.	65	Dr. Blackwood	24	Uterus enlarged 1½ in.; soft growth curetted from interior; movable	Moderate	Good health	Glandular carcinoma of body
6	M. G. L.	51	Dr. Jeans	(?)	Growth on anterior lip of cervix, extending to anterior vaginal wall; uterus movable; no external deposits	Moderate	Good; not lost weight	Epithelioma of cervix
7	W. M.	34	Dr. Kyffin	10	Cervix occupied by growth; uterus fairly movable; no external deposits	Moderate	Good; has lost weight	Carcinoma
8	K. M.	55	Dr. Gavin Brown	3	Cervix occupied by growth; uterus fairly movable; deposit on right side	Advanced	Good; not lost weight	Carcinoma
9	B. E.	60	Dr. McEwan	24	Uterus enlarged 1 in. and interior contained growth; fairly movable; deposits both laterally and behind	Advanced	Good; not lost weight	Carcinoma of body

IN WHICH WERTHEIM'S OPERATION WAS PERFORMED.

Date of and notes on operation	Shock	Progress of wound	Immediate result	Remarks
November 17, 1905; no enlarged glands, no macroscopic cancer outside uterus	Severe	Slight suppuration	Recovery	—
July 25, 1906; Rectum adherent and bladder involved in growth; portion of bladder excised; ureters with difficulty isolated from cancerous masses in broad ligaments; glands removed along iliac vessels; abdominal wound drained	Moderate	—	Death	Death four days after operation; post mortem, acute suppurative peritonitis. Too advanced for removal; abdomen should have been closed after exploration
August 8, 1906; no enlarged glands	None	Primary union; H.T., 100° F.	Recovery	—
November 3, 1906; bladder involved and portion excised; glands removed on left side	None	Primary union; H.T., 99° F.	Recovery	—
January 15, 1907; no enlarged glands	None	Primary union	Recovery	—
January 29, 1908; bladder very adherent; no pelvic deposits; no enlarged glands	Severe	Slight serous oozing from wound; H.T., 100·5° F.	Recovery	—
July 15, 1908	None	Slight suppuration	Recovery	—
December 21, 1908; large mass infiltrating right broad ligament; right ureter greatly dilated and involved; uterus and disease removed as far as possible, but not a complete extirpation	None	Not stated	Recovery	Unsuitable case for removal; disease undoubtedly left behind in pelvis
February 14, 1909; uterus adherent to ileo-pelvic colon; right ureter dilated; cancer involved broad ligaments and pelvic cellular tissue; uterus and disease removed as far as possible, but not a complete extirpation	None	Suppuration; H.T., 100·4° F.	Recovery	Unsuitable case for removal; urine contained pus after operation, and a communication existed between bladder and vaginal vault; nothing further attempted

H.T. = Highest post operative temperature.

TABLE A (*continued*).—BRIEF DETAILS OF TWENTY-THREE

No.	Patients' initials	Age	Medical attendant	Duration of symptoms in months	Examination under anæsthetic	Stage of disease	General condition	Microscopic report (clinical research)
10	H. E.	58	Dr. Kyffin	24	Uterus enlarged 3 in.; pyometra; growth at junction of body and cervix; uterus fairly movable; deposit behind	Advanced	Fair; has lost 1 st.	Diffuse adenomyoma, with pyometra; many pus foci deep in muscular wall
11	B. A. E.	42	Dr. Claremont	14	Old tear in cervix, occupied by soft growth; uterus rather fixed	Advanced	Fair; weak heart; has lost weight	Epithelioma of portio vaginalis, secondarily involving cervical canal
12	M. V.	54	Dr. Reid	8	Cervix occupied by soft growth; uterus partially movable; external deposits	Advanced	Fair; has lost weight	Epithelioma of portio vaginalis, secondarily involving cervical canal
13	S. E.	37	Dr. Welsh	24	Cervix occupied by growth; no external deposits	Moderate	Fair; has lost weight	Squamous epithelioma
14	G. K.	34	Dr. McEldowney	4	Large growth protruding into vagina; uterus movable; no external deposits	Moderate	Good; has lost weight	Epithelioma of papillary type
15	T. A.	52	Dr. Gibson	2	Cervix occupied by growth; uterus movable; no external deposits	Moderate	Has lost weight	Carcinoma
16	L. C.	48	Dr. Norman	18	External os raw; soft growth within cervix; uterus movable; nodules at back of cervix	Moderate	Good; has lost weight	Atypical epithelioma; no definite cell nests
17	B. E.	46	Dr. Mayhew	12	Large fungous growth occupying vagina; uterus movable; no external deposits	Moderate	Has lost weight	Squamous epithelioma; contains multinucleate cells in parts

CASES IN WHICH WERTHEIM'S OPERATION WAS PERFORMED.

Date of and notes on Operation	Shock	Progress of wound	Immediate result	Remarks
March 15, 1909; dense pelvic adhesions; bladder very adherent to fundus and separated with difficulty; abdominal wound drained	None	Urine from abdominal wound due to sloughing of bladder wall; catheter tied in bladder	Recovery	Urine ceased to flow from wound on April 4, 1909; good recovery
March 25, 1909; bladder adherent; right ureter with difficulty isolated; severe shock during operation; uterus and disease removed as far as possible, but not a complete extirpation; no enlarged glands	Severe	Suppuration; H.T., 100° F.	Recovery	Rapid local recurrence in two months; probable transplantation of cancer cells into freshly-cut tissues during operation
May 15, 1909; extensive cancerous deposits in broad ligaments; extirpation not completely satisfactory; troublesome hæmorrhage from pelvic veins; no enlarged glands	None	Slight suppuration; H.T., 100° F.	Recovery	—
Cervix adherent to bladder; right ureter accidentally divided, and uretero-ureterostomy performed	None	Primary union; H.T., 100·2° F.	Recovery	Urinary fistula into vaginal vault; segregation determined right ureter at fault; October 4, 1909, right lumbar nephrectomy; good recovery
September 6, 1909; old pelvic adhesions involving tubes and ovaries	None	Primary union; H.T., 99·2° F.	Recovery	
October 25, 1909; bladder very adherent	None	Slight suppuration; H.T., 99° F.	Recovery	—
December 6, 1909; nodules protruding on posterior surface of cervix; no parametric deposits; no enlarged glands	None	Primary union; H.T., 99·4° F.	Recovery	—
January 17, 1910; no enlarged glands	Slight	—	Death	Temperature irregular and patient did not appear well; sudden pain in chest followed by collapse and death at end of a week; post mortem, pulmonary embolism; sepsis in pelvis

H.T. = Highest post-operative temperature.

TABLE A (*continued*).—BRIEF DETAILS OF TWENTY-THREE

No.	Patients' initials	Age	Medical attendant	Duration of symptoms in months	Examination under anæsthetic	Stage of disease	General condition	Microscopic report (clinical research)
18	J. L.	57	Dr. Carling	24	Uterus enlarged 2½ in.; pyometra; soft growth in cervix and body; uterus movable; no external deposits	Moderate	Good; has lost weight	Malignant adenomyoma in both cervix and body
19	J. M.	59	Dr W. Green	3	Large growth filling vagina; deposit on left side of uterus	Advanced	Good; not lost weight	Malignant endothelioma suggesting perithelial origin
20	A. M. D.	60	Dr. Morley	5	Os swollen and raw; growth at left side of cervix; uterus not freely movable, and thickening in left broad ligament	Early	Sallow; has lost a little weight	Very early squamous epithelioma of portio vaginalis
21	R. W.	58	Dr. Gedge	1	Interior of cervix occupied by soft growth; uterus fairly movable; no external deposits; a tumour above and to left of uterus	Moderate	Good; has lost weight	Squamous epithelioma
22	T. E.	48	Dr. McEldowney	4	Cervix enlarged and occupied by growth; uterus fairly movable; no external deposits	Moderate	Fair; chronic bronchitis; has lost weight	Carcinoma
23	R. M.	54	—	4	Cervix enlarged and interior occupied by growth; uterus movable; thickening felt in left utero-sacral ligament	Moderate	Good; very stout, not lost weight	Squamous epithelioma

CASES IN WHICH WERTHEIM'S OPERATION WAS PERFORMED.

Date of and notes on operation	Shock	Progress of wound	Immediate result	Remarks
January 18, 1910; no pelvic deposits or enlarged glands	None	Primary union; H.T., 99·6° F.	Recovery	—
February 24, 1910; considerable infiltration of broad ligaments, especially the left; no enlarged glands	Severe	Suppuration; H.T., 102° F.	Recovery	Brevity of symptoms compared with the advanced stage of the disease
May 7, 1910; pelvic adhesions; inflammatory thickening in left broad ligament; no enlarged glands	None	Primary union; H.T., 98·2° F.	Recovery	—
June 6, 1910; left ovarian cyst removed; old pelvic adhesions; cervix adherent to bladder; no enlarged glands	None	Slight suppuration; H.T., 99·8° F.	Recovery	—
September 26, 1910; left ovary cystic; no macroscopic cancer in pelvis; no enlarged glands; cervix torn at junction with body where growth had almost penetrated the uterus; bladder adherent	None	Primary union; H.T., 102° F. (due to bronchitis after operation)	Recovery	—
October 26, 1910; no macroscopic cancer in pelvis; no enlarged glands; two Spencer Wells's forceps left on bleeding veins in pelvis; bladder adherent to the supravaginal cervix	Severe	Suppuration; H.T., 102° F.	Recovery	—

H.T. = Highest post-operative temperature.

(2) DURATION OF SYMPTOMS.

The duration of symptoms before women come to operation remains a very unsatisfactory feature. Of the 21 cases of cancer, in 20 the length of time the disease had existed was quite definitely ascertained; in one (6) it was doubtful, and is omitted. It averaged 9·4 months. The shortest period was one month; the longest twenty-four months, of which there were three. In only 8 cases was the duration less than six months (8, 14, 15, 19, 20, 21, 22, 23), being in these three, four, two, three, five, one, four, four months respectively. Taking the cases of shortest duration of symptoms—viz., less than six months—even in most of these the disease was found to have made considerable progress. In Case 20 only was it noted as early; in Cases 14, 15, 21, 22, and 23 it was moderately advanced; while in Cases 8 and 9 it was advanced. So that out of 8 cases where symptoms had existed attracting the notice of the patient for such short periods as three, four, two, three, five, one, four, four months, which comprises the earliest periods at which patients have come under my care, and is about as early as I think we may in the present state of education expect to get them, in no less than 7 the disease was advanced or moderately advanced.

The above facts demonstrate how insidiously the disease progresses without giving rise to any symptoms likely to attract the notice of its victims. Case 19, for instance, was a most intelligent and observant woman, belonging to the educated class. She was quite positive that bleeding had only been present for three months, and that she had had no other symptom. She had passed the menopause twelve years previously, so that there was no room for error on that account. Yet a very large growth filled the vagina, which must have been present many months without giving rise to any symptoms whatever, and the parametrium was considerably infiltrated; the case, in fact, was advanced. Case 8 was of a similar nature. It is evident of what vital importance it is that women should know the possible significance of the very first bleeding. In attempting the better education of women in this matter, they fall into two classes: those who have not passed the menopause, and those who have. With regard to the former, the subject is a very difficult one, and women will, I am afraid, continue to view with equanimity irregular bleeding before or about the menopause. But it should not be such a difficult task to make it a matter of common knowledge that bleeding, however trifling, occurring after the menopause in all probability means cancer. This is such a definite, simple fact that it should not be hopeless to educate women to its knowledge. No fewer than 13 of these 21 women had definitely passed the menopause, all of them many months, many of them many years, yet 12 in whom the duration of unmistakable symptoms was ascertained averaged 9·2 months after the onset of these unmistakable symptoms before they came to operation.

(3) EXAMINATION UNDER ANÆSTHETIC.

Before advising a patient abdominal hysterectomy she should always be examined under general anæsthesia. This has been done in my cases about a week before the operation. With the patient in the lithotomy position under deep anæsthesia, the degree of mobility of the uterus, evidence of spread of the disease to the parametrium or beyond it, the presence of enlarged glands or of enlargement of the kidneys, may be ascertained with greater accuracy, and all of these have their obvious significance in determining the likelihood or possibility of successful radical treatment. At this sitting the disease is thoroughly cut and curetted away, and the wound is swabbed with iodized phenol or 10-per-cent. formalin. The growth removed is then submitted to expert microscopical examination.

(4) STAGE OF THE DISEASE.

The cases have been divided into three classes: early, moderately advanced, and advanced. This division was arrived at by the clinical examination before operation, and was confirmed or modified by the condition actually found at operation. The stage of the disease is noted as early where the local focus was small in extent and there was no palpable evidence of spread beyond it. Only Case 20 complies with this definition. It is noted as moderately advanced where the local growth was considerable and had evidently been in existence a length of time, yet the uterus was movable, and there was no palpable or, at operation, macroscopical evidence of spread to the parametrium or of secondary infection of glands. There were 13 of these. In four the carcinoma was of the papillary type, a large tumour occupying the vagina. In cases in which the growth thus expends itself externally, it is, I believe, generally held that its inward spread is less rapid. In this class, from an operative point of view, are included the case of diffuse adenomyoma with pyometra (10), and the case of malignant adenomyoma involving both cervix and body (18). The advanced cases are those in which there was fixity of the uterus, evidence of spread to the parametrium or bladder, pressure on the ureters, or secondary infection of glands. There were seven of these.

(5) General Condition of the Patient.

Women with this disease maintain their general health remarkably well until the late stages are reached, and even then, especially if they are advanced in years, in some instances it seems to make little impression upon them. Of the early and moderately advanced cases of cancer, numbering 14, 9 were in quite good health at the time of operation, 3 were in fair health, in 2 the general condition is not stated (it was probably good). Ten were positive that they had lost weight, 2 that they had not lost weight, 1 was doubtful but thought she had lost weight, and in 1 there is an omission from my notes on the point. Of the advanced cases, numbering 7, 3 were in good health and were positive that they had not lost weight. The ages of these were 55, 59, and 60 years. The remaining 4 were in fair health. Of the total number of cases of cancer (21), 14, or two-thirds, had lost weight, whether they were otherwise in good health or not.

(6) Microscopical Report.

In 22 out of the 23 cases the microscopical report was furnished by the Clinical Research Association. In Case 4 the report was made by Mr. Ridout, who, in 1906, was holding the appointment of Pathologist to the Portsmouth Hospital. Although the curette or finger will tell with almost certainty if cancer be present, no patient should be submitted to radical treatment on a supposition, however probable. Every case should be previously confirmed by expert microscopical examination.

(7) Notes on Operation.

In Cases 8, 9, 11, and 12, although after opening the abdomen I believed that a thorough removal might be practicable, I found as I proceeded, for reasons stated in the table, that complete extirpation was impossible, and the operation was brought to as satisfactory a conclusion as the circumstances permitted. In advanced cases it is not always easy, even after opening the abdomen, to be sure that a thorough extirpation will be possible, and unforeseen obstacles may arise either in the course of the operation or owing to the general state of the patient. In dealing with cancer, wherever situated, if the case is on the border-line and uncertainty exists as to whether operation should be undertaken or not, it may be laid down as an almost universal rule that the difficulties will be not less than anticipated. It is seldom in my experience that the reverse is the case, and that the operation turns out easier or less severe than expected. This should be borne in mind before commencing operation in a doubtful case. In Cases 2, 8, 9, 11, and 12 it might possibly have been better to close the abdomen without proceeding further. Case 2 died as the result of the operation. Case 12 had an early and rapidly growing recurrence, very probably due to implanting cancer cells into freshly-cut surfaces during the operation. Of Case 8 I have no after-record. On the other side of the picture, however, Case 9 is alive twenty months after the operation, and Case 12 seventeen months after; and, though both of them have recurrence, one had twelve months' good health and the other is still in good health. Neither of them suffers any pain to speak of. Advantage, therefore, sometimes accrues from operating in very advanced cases; relief is obtained for a time, and death finally comes under less distressing circumstances.

The ureters are best isolated as the first step in the operation, before any oozing of blood into the pelvis has occurred. The greatest gentleness should be exercised in their separation to avoid disturbing their vascular supply. Dilatation of the ureters from pressure is an unfavourable sign. In both cases (8 and 9) in which this was present there was great infiltration of the parametrium with cancer, and it was found impossible to thoroughly extirpate the disease. In a future case with this complication I should consider very carefully before proceeding further with the operation. In one case (13) the right ureter, after having been isolated, was divided, and unless great care is taken this is not at all an impossible accident. Uretero-ureterostomy was immediately performed. A week after urine came from the vaginal vault. Segregation confirmed the fact that the right ureter was leaking. As the fistula showed no sign of closing after three months, and the patient was very uncomfortable, right lumbar nephrectomy was performed for its cure, with a good result. If a ureteral fistula refuses to close after a reasonable time and the opposite kidney is healthy, the above seems to be the right treatment.

The bladder may be found implicated in two ways. The growth may have actually involved the bladder wall. This had occurred in Cases 2, 4, and 9. Case 2 died as the result of the operation. In Case 4 the portion of bladder wall involved was excised, the wound was closed, and no further trouble on this account resulted. In Case 9 the operation could not be satisfactorily completed owing to the extent of the disease in the pelvis; urine came from the vagina a few days after the operation, and a communication was found to exist between the bladder and vaginal vault. Nothing further was attempted. More frequently the bladder, without being actually implicated in the disease, is very adherent to the supravaginal cervix, due to the inflammatory tissue reaction and fibrosis at the circumference of the advancing cancer, which represents Nature's attempt to hem it in. This was so in no fewer than 7 cases (6, 11, 13, 15, 21, 22, 23). There may here be great difficulty in separating the bladder from the cervix without tearing a hole in it. The best method is by careful gauze stripping, accompanied, when necessary, with short snips with blunt-pointed scissors kept close to the cervix. When the separation is completed, portions of the muscular wall of the bladder will frequently be seen adhering to the front of the cervix. Subsequent necrosis of the bladder wall may occur, as happened in Case 10 (diffuse adenomyoma), urine flowing a few days after the operation from the abdominal wound, which had been drained owing to the infected condition. A catheter was tied in the bladder and the fistula was healed within three weeks, the patient making a good recovery. The upper rectum may be found adherent from the same cause and require separation in a similar way. This occurred in Cases 2 and 9. I have had no instance of recto-vaginal fistula following operation. Troublesome hæmorrhage is noted as having occurred from veins in the pelvis in Cases 12 and 23. A pair or two of long Spencer-Wells's forceps may have to be left on for twenty-four hours when it is found impossible to apply a ligature, as happened in Case 23; or gauze packing may be necessary. I have had no case of reactionary or secondary hæmorrhage following operation.

A great deal has been written and said about the propriety of removing glands. I have made no systematic attempt to extirpate glands in my operations. No microscopic examination has therefore been made of the regional glands, and without it no reliable conclusions can be drawn. In the early case (20), in all the moderately advanced cases (1, 3, 5, 6, 7, 13, 14, 15, 16, 17, 21, 22, 23), and in two of the advanced cases (12, 19) the glands were not palpably enlarged—i.e., in sixteen out of twenty-one cases. In these no attempt at glandular dissection was made, and the only evidence that they were not infected is that they were not palpably enlarged. For what it is worth, this evidence supports the observation that glandular infection is late. Of the remaining advanced cases, in two (2, 4) cancerous glands were removed, and in three (8, 9, 11) mention of the condition of the glands is unfortunately omitted from my notes. Probably they were not enlarged; otherwise it would have been stated. It seems to me that the systematic dissection of glands is to be discountenanced for the following reasons:—

(1) It must greatly prolong the operation and increase the primary risk.

(2) Inasmuch as glandular infection, as a rule, is late, this risk may frequently be added without there being any necessity for it.

(3) As it is impossible to remove all the glands, it is probably useless.

Experience, moreover, has shown that, where glandular infection has taken place, recurrence almost invariably happens: life is not saved. It therefore adds to the immediate risk without probably any compensating advantage in the way of radical cure. Where glands are palpably enlarged and accessible, probably every surgeon would naturally remove them; but I do not think that any material improvement in final results is going to be obtained by exclusive glandular dissections in this region, whereas the primary mortality would be certain to go up. Personally, I should require much more convincing evidence than I have hitherto been able to obtain from the experience of its advocates, before adding to the severity of this already very severe operation by any systematic attempt at glandular extirpation in all cases. The general technique adopted in my operations has been that recommended by Wertheim—viz., to pack the space immediately above the vagina with gauze, and bring the end out through the vaginal orifice; to sew over this the peritoneum, thus shutting off the peritoneal cavity; finally, to close the abdominal wound throughout. In two cases (2, 10), which were exceptionally septic, a drainage tube was brought out of the abdominal wound. The vaginal gauze strip is removed on the fifth day.

(8) SHOCK.

Five patients suffered severely from shock; none died of it. In the remainder the shock is described in the table as *nil*. It is meant that there was no shock to give rise to any anxiety. Of the five cases that suffered severely from shock, in one (6) there was no particular reason for it as far as the operation was concerned. The patient, however, had been a nurse; she knew she was to undergo a very serious operation, and the mental effect probably had a good deal to do with it. Of the remaining four, in three the disease was advanced, the operation exceptionally long and difficult, and in two of them the general condition previous to operation was bad. In Case 23 the operation was also prolonged on account of the patient being very fat and of delay in securing hæmostasis. I believe that as the surgeon becomes more experienced in the proper selection of cases, either before or after opening the abdomen, and more dexterous with practice in the performance of the operation itself, thereby shortening the time the patient is on the table, which is the all-important matter, he will have little to fear from shock. It is common observation how well women stand severe operations on the pelvic organs, and I consider, generally speaking, with an operator of experience the time for any case which is suitable for operation at all should be finished within one and a half hours, and frequently within the hour. If these conditions are complied with, in my opinion there is little to fear from shock. Of course individual operators will differ in their selection of cases, and, if very advanced cases are undertaken, the operation may take a good deal longer, and there may be considerable danger of death from shock. Probably, however, such cases as these are better left alone. At all events, neither Wertheim's nor any other operation is likely to effect a radical cure, and it is doubtful whether a great primary risk should be run in return for the prospect of a short respite. A very important point is the final vaginal toilette before the patient is anæsthetized. This, in my cases, is done in the ward an hour before the operation, and shortens the operation itself by twenty minutes.

(9) PROGRESS OF THE WOUND.

Sepsis is in my experience the most serious complication of Wertheim's operation. It may attack either the abdominal or pelvic wound. In 21 of the 23 cases the abdominal wound was closed without drainage; in only 10 of these, or 47 per cent., was primary union secured. In the remainder, suppuration, sometimes trivial, sometimes pretty severe, occurred, though I have not seen a case of sloughing of the whole abdominal wound, as mentioned by some writers. All of the cases that suppurated eventually did well. The technique I have adopted is briefly as follows:—

(1) A week previous to operation all the external growth is cut and curetted away, and the cavity left is swabbed with 10-per-cent. formalin or iodized phenol.

(2) In the interval before the operation vaginal douches twice daily of 1 in 6,000 biniodide of mercury.

(3) An hour before the operation the patient is put in the lithotomy position and the vagina is thoroughly scrubbed out with soft soap and sterilized water by means of a cotton-wool swab on a holder. The vagina is then irrigated with 1 in 1,000 biniodide of mercury, followed by sterilized water.

(4) At the operation the abdominal incision is protected with sterile Battist mackintosh stitched to its sides.

(5) Just before applying Wertheim's forceps to the vagina, the latter is cleansed with a dry sterile swab on a holder. If this is properly done, on opening the vagina it is usually quite dry.

(6) General aseptic technique, including a fresh set of instruments for closing the abdominal wound.

Sepsis attacking the wound in the pelvis is a more serious complication. It certainly accounted for the only deaths in my series of cases. Case 2 died of septic peritonitis caused by endeavouring to deal with a very advanced stage of the disease, in which it was considered necessary to drain the abdominal wound. Case 17 died suddenly of pulmonary embolism a week after operation. She had had a post-operative temperature and was not doing well. She died suddenly. A post-mortem revealed suppuration in the pelvis and a clot in the pulmonary artery.

(10) IMMEDIATE RESULTS.

In 23 cases[1] there have been 21 recoveries and two deaths—a mortality of 8·6 per cent. My second case died. There followed 15 cases without a death; then a second death followed by seven subsequent recoveries. This mortality is, I believe, lower than the general mortality for this operation, though of course the number of cases is small. The only remark I shall make here is that with increasing experience in the selection of cases, as well as in the actual performance of the operation, which often is a difficult one, there is reason to expect a still further decline in the primary mortality.

GENERAL REMARKS.

Since Wertheim[2] read his paper at Leicester in 1905 his operation has been on trial in this country as the radical treatment for carcinoma of the uterus, alternative to vaginal hysterectomy, which up to that time had been almost exclusively performed for this disease. In attempting to form a comparison between Wertheim's operation and vaginal hysterectomy the obviously important points for consideration are their primary mortality and ultimate results. Some writers have said, never mind if the immediate mortality is higher provided that we can get better ultimate results, and that with this end in view the surgeon is justified in recommending his patient an increased primary risk. This may be a perfectly sound argument, and is a perfectly natural argument in the presence of the very unsatisfactory late results of vaginal hysterectomy amongst most operators. But in practice, if the patient die from the operation, it is not much consolation to the surgeon to know or the relatives to be told that, had she survived it, she might have lived for years, or even been cured outright. It is an argument, at all events, that will not appeal to the public, whatever solaces it may have for the surgeon. We cannot, therefore, pass over the question of the primary mortality, and a great deal has been made of it by the critics of this operation. In the discussion, for instance, on Dr. Berkeley's paper last year at the Medical Society,[1] one very high authority said that the most terrible thing about the operation was its high mortality. Wertheim's original 40 per cent., later his 18 per cent., and later still his 9½ per cent. mortality has been unfavourably contrasted with that following vaginal hysterectomy, which amongst operators of experience is somewhere in the neighbourhood of 5 per cent. With regard to Wertheim's original 40 per cent., it may be said at once that neither it nor anything approaching it is likely to be repeated. It was the mortality of the pioneer of the operation, the man who was groping his way through its initial difficulties and dangers. With an operation of this nature his early mortality was bound to be high. Such has been the history of almost every major operation introduced into surgery; and if in the past we had been deterred by the high primary mortality of operations while they were, so to say, finding their footing, we might still be tapping an ovarian cyst through the abdominal parietes or feeding patients with bleeding fibromyomata on ergot till they developed threatening gangrene of their extremities, as actually happened in the case of a relative of my own. Wertheim's 40 per cent. mortality is already ancient history. It is only of interest as illustrating the difficulties and dangers of establishing an operation of this nature on a sound surgical basis.

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[1] Nineteen operations were for cancer of the cervix, with two deaths—i.e., 10·5 per cent.

[2] *Brit. Med. Journ.*, 1905, ii, p. 689.

I shall next invite your consideration for a few moments to what is the primary mortality of Wertheim's operation at the present time, and compare it with that of vaginal hysterectomy. I shall take, to begin with, the records of some well-known Continental operators. Mackenrodt, in 69 cases, had 14 deaths, a mortality of 20 per cent.; Döderlein, 47 cases, with 7 deaths, mortality 14·8 per cent.; Bumm, 82 cases, with 17 deaths, mortality 22 per cent.; Pollosson, 133 cases, with 17 deaths, mortality 12 per cent.; Wertheim, a first series of 200 cases, with 49 deaths, mortality 24·5 per cent.; a second series of 200 cases, with 20 deaths, mortality 10 per cent. There were 731 cases in all, with 124 deaths, a primary mortality of just upon 17 per cent. No individual English operator has hitherto published a large collection of cases, but if we take Dr. Berkeley's figures, summarized in Table B, which you have before you, we have 216 cases, performed by twenty-two different operators, with 39 deaths, a mortality of just 18 per cent., almost exactly the same as Continental operators have experienced. This at first sight compares very unfavourably with vaginal hysterectomy, the mortality of which is about 5 per cent. It is three and a half times as great. But a closer analysis of the cases shows this comparison to be the veriest fallacy; and for this reason: The high primary mortality of Wertheim's operation occurs exclusively in the advanced cases, which cannot be submitted to vaginal hysterectomy at all. In cases which could have been attacked from the vagina, and in which, therefore, a comparison with vaginal hysterectomy alone is possible, Wertheim's operation gives very little higher primary mortality than the vaginal operation. By way of illustration I shall take my own cases first. They are few in number, but, as I am familiar with their details, I can analyse them quite accurately. There have been 23 cases. Those which could have been dealt with by vaginal hysterectomy were the early case (20); the moderately advanced cases (1, 3, 5, 6, 7, 13, 14, 15, 16, 17, 21, 22, 23), in which I have said, though the local disease was advanced, it was, as far as clinical examination indicated, still confined to the uterus; the case of diffuse adenomyoma (10) and the case of malignant adenomyoma involving both cervix and body (18)—i.e., 16 in all. In these it would have been possible to remove the uterus *per vaginam*. Wertheim's was the operation performed. There was one death (17), or a mortality of 6·2 per cent., which is only 1·2 per cent. higher than the average statistics for vaginal hysterectomy, and considerably lower than many operators have obtained for that operation. For the remaining 7 cases—and this is the point—there was no question of vaginal hysterectomy at all. They were inoperable by that route. It was Wertheim's operation or nothing as far as radical treatment was concerned. Amongst the 7 there was also one death—i.e., a mortality of 14·2 per cent., or more than twice as great.

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[1] *Trans. Med. Soc. Lond.*, 1909, xxxii, p. 102.

I shall next take the cases collected by Dr. Berkeley, which have been submitted to a similar analysis in Table B. There were in all 221 cases of Wertheim's operation, performed by 23 different British surgeons. In 5 of these (3 by W. Newham and 2 by T. Wilson) the stage of the disease was not returned; they are therefore omitted. In the remainder, numbering 216, the stage of the disease was returned and was signified as early, moderately advanced, or advanced. The total mortality was, as I have said, 18 per cent. Now, I do not think I am touching disputable ground if I say that only the early cases, or at most the early and moderately advanced cases, could have been dealt with by vaginal hysterectomy. Vaginal hysterectomy has never, so far as I know, been held to be possible in advanced cancer.[1] Taking the early cases alone, we have 33 cases of Wertheim's operation with two deaths, a primary mortality of just 6 per cent. If we add to them the moderately-advanced cases, we have 52 cases with three deaths, a primary mortality of 5·5 per cent. In other words, as far as the returns of British operators have been hitherto available, where Wertheim's operation is performed for early cases or for early and moderately-advanced cases—the only ones in which a comparison can be made with vaginal hysterectomy, because for these alone vaginal hysterectomy is possible—we have a primary mortality between 5 and 6 per cent., or less than 1 per cent. higher than the average for vaginal hysterectomy. In the advanced cases, on the other hand, numbering no fewer than 164 out of 216 (illustrating, by the way, how greatly Wertheim's procedure increases the percentage operability), we have a primary mortality of 21·9 per cent. For these there was no question of vaginal hysterectomy. It was Wertheim's operation or nothing. There appears to be only one conclusion from these figures—viz., that for cases of cancer in which vaginal hysterectomy is possible Wertheim's operation is practically as safe. The high primary mortality of Wertheim's operation is confined, as I have shown, exclusively to the advanced cases, which cannot be attacked from the vagina at all. The operation, in fact, enlarges the field, increases the percentage operability, which means that it enables later cases to be dealt with, patients whose disease is more advanced and whose powers of resistance are consequently less; the mortality amongst them is bound to be high. As far as the primary

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[1] I mean, by advanced cancer, cancer infiltrating the structures outside the uterus and fixing it.

TABLE B.

Analysis of 216 cases of Wertheim's operation for Carcinoma of the Cervix, performed by 22 British Operators.

OPERATORS' NAME.	Total No.	Early Cases.						Moderately Advanced Cases.						Advanced Cases.					
		Number.	Operative Deaths.	Immediate recovery.	No after record.	No recurrence when last seen or to date.	Death from recurrence or having known recurrence.	Number.	Operative Deaths.	Immediate recovery.	No after record.	No recurrence when last seen or to date.	Death from recurrence or having known recurrence.	Number.	Operative Deaths.	Immediate recovery.	No after record.	No recurrence when last seen or to date.	Death from recurrence or having known recurrence.
Berkeley, C.	16	1	...	1	1	...	...	1	...	1	...	1	...	14	3	11	...	9	2
Bonney, V.	14	2	...	2	...	2	...	...	...	...	...	...	...	12	2	10	1	5	4
Spencer, H.	13	...	...	...	...	...	...	4	1	3	1	...	2	9	3	6	1	3	2
Blacker, G.	10	1	...	1	...	1	...	4	...	4	...	1	3	5	2	3	...	1	2
Andrews, R.	19	1	...	1	...	1	...	...	...	...	...	...	...	18	5	13	...	12	1
Lockyer, C.	30	3	1	2	1	...	1	...	...	...	..	...	...	27	6	21	13	...	8
Tweedy, H.	26	...	...	...	...	...	...	...	...	...	...	...	...	26	5	21	17	...	4
Malcolm, J. D.	5	2	...	2	...	2	...	...	...	...	...	...	...	3	...	3	...	1	2
Drew, D.	7	...	...	...	...	...	...	...	...	...	...	...	...	7	1	6	...	3	3
Williamson, H.	3	1	...	1	...	1	...	...	...	...	...	...	...	2	1	1	...	1	...
Purslow, C.	3	2	...	2	...	2	...	1	...	1	...	...	1	...	...	...	...	...	...
Fairbairn, J.	4	...	...	...	...	...	...	...	...	...	...	...	...	4	3	1	...	...	1
Giles, A.	4	1	...	1	...	1	...	3	...	3	...	3	...	...	...	...	...	...	...
Targett, J. H.	3	...	...	...	...	...	...	...	...	...	...	...	...	3	...	3	...	3	...
Fothergill, W.	2	...	...	...	...	...	...	...	...	...	...	...	...	2	...	2	...	...	2
Ionides, T. H.	1	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	...	1	...
Eden, T. W.	7	7	...	7	2	4	1	...	...	...	...	...	...	...	...	...	...	...	...
Boyd, Mrs.	5	1	1	...	...	...	...	1	...	1	...	1	...	3	1	2	...	1	1
Taylor, F. E.	1	...	...	...	...	...	...	1	...	1	1	...	...	...	...	...	...	...	...
Ryall, C.	10	1	...	1	1	...	...	1	...	1	1	...	...	8	1	7	6	1	...
McCann, F.	18	6	...	6	...	6	...	3	...	3	...	2	1	9	1	8	...	1	7
Wilson, T.	15	4	...	4	...	4	...	...	...	...	...	...	...	11	2	9	...	9	...
Total ...	216	33	2	31	5	24	2	19	1	18	3	8	7	164	36	128	38	51	39

NOTE—W. Newham's three cases have been omitted, and two cases of T. Wilson, because the stage of the disease was not returned.

CONCLUSIONS.

	Number.	Primary Mortality.	No recurrence to date or when last seen.	Death from operation or recurrence or having known recurrence.
1. Early Cases	33	6·06 per cent.	85·8 per cent.	14·2 per cent.
2. Moderately Advanced Cases	19	5·2 per cent.	50 per cent.	50 per cent.
3. Advanced Cases	164	21·9 per cent.	40·5 per cent.	59·5 per cent.

4. Assuming that the Early and Moderately Advanced Cases could all have been dealt with by Vaginal Hysterectomy, the number of cases operable by Wertheim's method is increased from 51 to 216; i.e., is more than 4 times as large.

mortality is concerned, the true position of Wertheim's operation is contained in the following statement:—

(1) If a patient apply with early cancer—and I mean by this if there is no palpable evidence of its spread beyond the uterus; if, in fact, the case is suitable for vaginal hysterectomy—the surgeon has a choice of Wertheim's operation as an alternative with a primary mortality practically as low.

(2) If a patient apply with more advanced cancer, the surgeon has no choice but Wertheim's operation. It is that or nothing as far as radical treatment is concerned. In these cases the operative mortality is high—about 20 per cent. He can put it before his patient; he can offer her the chance of cure, however remote, or at least of a prolongation of life and of a less distressing death, at a considerable primary risk. If she elects to take her chance, the operation is sometimes well worth the doing. For instance, in Cases 9 and 12 of my series the disease was so advanced that I felt sure at the end of the operation that a thorough extirpation had not been accomplished. One was operated on in February, and the other in May, 1909. Both are alive at the present time, and one of them still enjoys very fair health. Though both have undoubted recurrence, neither of them suffers any pain to speak of.

If, therefore, except for advanced cases, which, as I have stated, must be put in a class by themselves, the primary mortality of Wertheim's operation is as low as that of vaginal hysterectomy, it is to the ultimate results that we must look to determine whether it can claim a superiority. For information upon this point we must go to the Continent: the operation has not been performed long enough in this country to give the requisite evidence. For this reason I have given no after-results in my own cases: a reference to the dates on which most of the operations were performed will show that at present they would be valueless. The following Continental statistics, however, are to the point: Wertheim, in 151 cases which survived the operation, had four deaths from intercurrent disease, 59 recurrences, and 88 free from recurrence after five years—i.e., 59 per cent. of those who survived the operation well after five years. Mackenrodt in 144 cases had 74 per cent. well after periods varying from one and half to six and half years. Scheib, reviewing the cases in the clinique at Prague, found surviving after five years 28·5 per cent.; after six years, 27·2 per cent. Pollosson in 1909 found 35 per cent. well of those operated upon in 1905 and 1906. It must be noted—and it is a very important point—that these are the late results of all operable cases, both early and advanced. The late results of selected cases, which alone can fairly be compared with those of vaginal hysterectomy, would certainly be better than these. But, even taking these results as they stand, I am aware of none from vaginal hysterectomy that can compare with them. Spencer and Lewers give 24 and 16 per cent. of recoveries respectively — a much better result than most vaginal hysterectomists have achieved, but quite meagre in comparison with the Continental statistics I have given, although from the very nature of their operation (vaginal hysterectomy) they represent the ultimate outlook in none but a priori favourable cases. The need of distinguishing between the late results, no less than the primary mortality, of the two distinct classes of cases that can be dealt with by Wertheim's operation is manifest on reading the discussion on Dr. Berkeley's paper. Some speakers drew attention to the number of recurrences which were conspicuous in the returns. On casually looking at the returns the number of recurrences does appear large. But a closer analysis of them, which is supplied in Table B, demonstrates, as of course might have been expected, that the advanced cases account for the majority of recurrences as of the high primary mortality. Of course the results are too recent to be of value, and are only given to show in which class of case most of the recurrences have taken place.

To sum up, for all cases of carcinoma of the uterus, whether of the cervix or body, Wertheim's operation is to be preferred to vaginal hysterectomy, for the following reasons:—

(1) It is in the first instance exploratory in all cases, and enables the surgeon to determine by sight and direct touch whether the case is suitable at all for radical treatment, information which he cannot possibly obtain with anything like the same degree of accuracy before operating by the vaginal route.

(2) In cases in which the disease has not spread beyond the uterus, and in which alone a comparison with vaginal hysterectomy is possible, the primary mortality is, for all practical purposes, as low as that of the latter operation.

(3) It is a more scientific operation, all the details of which are carried out directly under the eye of the operator, and is liable, therefore, to fewer operative risks than vaginal hysterectomy, which is more or less a shot in the dark.

(4) It is on all fours with the more extensive operations for cancer which have been practised of late years in other regions, and which have given far better ultimate results than the old niggling and partial operations. The principle governing these is that cancer in the first instance

is a local disease, and spreads centrifugally from its original focus; that it is impossible to say by sight and touch in any given case how far it has already spread; and that, consequently, as wide a removal of tissue in its neighbourhood as can be effected with due regard to the patient's immediate safety, and the anatomical possibilities of the region in which the disease is situated, will certainly be more likely to include the whole disease, and as the result of this to give a larger percentage of cures.

(5) There is evidence to show that it does give a far larger percentage of cures and a much longer lease of life than have been obtainable by vaginal hysterectomy.

(6) It may enable cancerous glands to be removed, if present. Though glandular infection, as a rule, is late, it may exceptionally occur early and be capable of removal, as happened in one of Dr. Russell Andrews' cases.

(7) It enlarges the fields of operability and enables more advanced cases to be dealt with. This, as far as radical cure is concerned, will, I think, be shown to be the least of its advantages, though it almost certainly saves some lives in these cases, and certainly prolongs life in many others. But in advanced cancer, with the pelvic contents extensively involved, the primary mortality is high; nor do I think we are to expect a large percentage of cures. But it is for these cases the only operation available, and may be recommended where conditions are otherwise suitable. It is in the early cases that the good results are to be expected.

In my list of cases it will be seen that the operation has been performed for some conditions other than cancer of the cervix. In Cases 8 and 9 the disease was in the body; in Case 10 the condition was one of diffuse adenomyoma with pyometra; in Case 18 malignant adenomyoma involving both cervix and body, accompanied with pyometra. I have performed it in such conditions because I consider it a superior operation in every particular to vaginal hysterectomy, and suitable for almost all conditions requiring total extirpation of the uterus.

DISCUSSION.

The PRESIDENT (Dr. Macnaughton-Jones) said that the time was very limited for an adequate discussion of two such important communications, which, taken together, practically covered the greater part of the field of cancer of the uterus, involving as they did its pathology, diagnosis, and prognosis, in the bearing of all three on the operative treatment. There were present those who between them had performed over a hundred Wertheim's operations, and who could therefore speak from a considerable personal experience of its technique and advantages.

Dr. HERBERT SPENCER asked, with reference to Dr. Leitch's paper, whether the duration (one year, nine months) of cancer of the body was obtained by investigating cases which had been operated on and recovered (in which case the figure was fallacious) or from cases not operated on; in this case he would like to know how many histories were investigated. His own experience would lead him to give a much longer course for cancer of the body and to regard very few cases indeed as inoperable when seen, so that he thought it would be difficult to obtain a large number of cases of cancer of the body which ran a natural course. Mr. Childe's mortality for Wertheim's operation for cancer of the cervix (nineteen cases with two deaths) was 10·5 per cent., and the author was to be congratulated upon it, as it was about the same as Wertheim obtained in his second 200 cases; but it was not right to include the four cases of malignant disease of the body in considering an operation for cancer of the cervix, and he hoped Mr. Childe would bring his two cases of "malignant adenomyoma" before the Section, as very few cases of the kind had been observed. With a great deal that Mr. Childe had stated he found himself in agreement; but Mr. Childe had overstated the mortality from vaginal hysterectomy, the best results from which—de Ott's 277 cases with five deaths (1·8 per cent.), Küstner's 1·8 per cent., and Olshausen's 100 cases with one death (1 per cent.)—had never been equalled or approached by the abdominal operation. Mr. Childe was quite wrong in his supposition that his (Dr. Spencer's) cases treated years ago by operation by the vagina with the cautery were all early cases. One of the cases in which the uterus was fixed was declared to be inoperable after examination under anæsthesia by two of the most distinguished gynæcologists in this country, both of whom stated as their opinion that the patient could not live for four months. After vaginal hysterectomy with the cautery she remained quite well for six years; then recurrence took place in the glands, of which she died eight and a quarter years after the operation. Of the first twenty-five cases of cancer of the cervix he had operated on at University College Hospital (six by vaginal hysterectomy, nineteen by high amputation) all recovered, and at least six remained well for over five years, three of these, complicated with advanced pregnancy, for eleven years. These results, and de Ott's, Küstner's, and Olshausen's results, were obtained not in early cases alone but for all cases (early and advanced), and they showed that there was practically no mortality from vaginal operations in *early* cases. Almost any case which could be removed by the abdomen could be removed by the vagina, and Staude by the vagina had a much greater operability percentage than Wertheim by the abdomen. The great superiority of Wertheim's operation for the moderate and advanced cases lay in the fact that it enabled the operator to remove the uterus and the adjacent tissues in a more scientific and surgical manner. Mr. Childe had not given his results as regards recurrence, and quite rightly stated that they were useless for the purpose; but he had not hesitated to use Dr. Berkeley's figures dealing with this branch of the subject, which were even more valueless than his own, some of the cases "free from recurrence" having been operated on within a month or two months of the reading of Dr. Berkeley's paper. The large number of Mr. Childe's cases in which suppuration of the wound occurred he (Dr. Spencer) thought was due to the use of metal retractors, and he preferred retraction by the hand of an assistant, a method he had seen Wertheim employ.

Dr. CUTHBERT LOCKYER expressed his hearty appreciation of Dr. Leitch's and Mr. Childe's papers. He had been deeply interested for many years in the extended operation for uterine cancer, and since the year 1905 had always employed the technique adopted and initiated by Wertheim, by whom he was taught his method of operating, at St. Mary's Hospital for Women and Children. The speaker regretted that he had had no opportunity of reading Dr. Leitch's valuable paper, as it was obvious that it required and merited careful study before the author's statements could be seriously discussed. If Dr. Lockyer had rightly understood the author, his conclusions were based upon the findings of post-mortem examinations made by other pathologists, and mainly on cases where death had occurred without operation. The disappointing fact about such material was that in advanced uterine cancer the adjacent tissues were generally so matted together and altered by inflammation that accurate dissection became a matter of great difficulty. The speaker had endeavoured to carry out the same observations in the post-mortem room, and in one instance undertook a journey of thirty miles into the country to investigate after death the condition of the pelvis in a very interesting but inoperable case which had been in the Samaritan Hospital; but, in spite of the fact that the patient had only been dead a few hours, the state of the pelvic viscera was such that no satisfactory details as to the mode of spreading, or the involvement of lymphatic glands by the cancerous process, could be made. The speaker noticed that Dr. Leitch had not mentioned a gland lying upon the uterine artery, in the juxta-cervical position; this had been twice observed by Dr. Lockyer in the forty-three cases in which Wertheim's technique had been followed. It was not a large gland, and it would probably be impossible to find it in advanced cases where the parametrium was extensively infiltrated by direct continuation of cancerous growth from the cervix uteri. The condition of hydro-ureter Dr. Lockyer was familiar with, but in the few ureteric sections he had made there was no invasion of the wall of the duct such as Dr. Leitch had discovered and previously made public. The statement that the pelvic field of operation (the inter-iliac area) was analogous to the axilla may be theoretically correct, but there was no analogy between the practical difficulties of efficiently clearing out the one and the other. To denude the pelvic structures of their cellular tissues to the full extent, which Dr. Leitch seemed to advocate, would, in the speaker's opinion, lead to necrosis of the ureter, and by unduly prolonging the operation expose the patient to the risk of additional shock. Dr. Lockyer was disposed to agree with Mr. Childe in his remarks on this important question—i.e., palpable glands should be sought for and removed, but anything like a systematic dissection of the lymphatic cellular tissues was, in the present state of our experience, contra-indicated. He agreed with Mr. Childe that Wertheim's technique increased the percentage operability, but differed from him in his belief that it offered fewer risks than vaginal hysterectomy. Dr. Lockyer, prior to 1905, had performed vaginal hysterectomy seventeen times for cancer of the uterus without a death ; but in his forty-three cases of Wertheim's hysterectomy he had a primary mortality of nine. Mr. Childe stated that the more extended operation gave a larger percentage of cures, and he had the authority of Continental operators to support his assertion, but it was a pity that he had seriously analysed the English cases to find additional evidence in support of this statement, as they were not ripe for such conclusions. In the 216 cases recorded by Dr. Berkeley and Mr. Bonney in January, 1909, one of the thirty cases contributed to that series by Dr. Lockyer was in bed convalescing from operation at the time the paper was read. The only conclusion as yet which could be drawn from English experience was as to primary mortality, and it was certainly satisfactory to find that in the English series of 216 the primary mortality was about the same as in the Continental series of 731. Dr. Lockyer noted that Mr. Childe did not employ the cautery in the preliminary technique he adopted a week before operation. Dr. Spencer had conclusively proved the value of the cautery knife in his cervical amputations for cancer, and in the speaker's opinion this part of Wertheim's technique should never be omitted. With regard to packing the cellular space with gauze before closing the peritoneum, the speaker had long ago given it up ; gauze was of no value as a means of stopping hæmorrhage, and, even if it were, the wound should be rendered dry by perfect ligation of vessels before closure. As a means of drainage, gauze was useless, as the open space into the vagina drained itself of any serous discharges. In Dr. Lockyer's opinion, gauze favoured sepsis, and he thought that possibly its use accounted for the ascending suppuration referred to by Mr. Childe as occurring in 47 per cent. of his cases. If Mr. Childe gave up using gauze and secured perfect hæmostasis—in other words, a dry wound—he would find that the abdominal parietes would heal without suppuration.

Dr. RUSSELL ANDREWS said that it was difficult to believe that the average duration of life in cases of carcinoma of the body of the uterus was only 1·83 years after the occurrence of the first symptoms, as in his experience hysterectomy was often possible after bleeding had been present for a year or two. He agreed with Mr. Childe that the primary mortality of Wertheim's operation need not be much greater, if at all greater, in early cases than that of vaginal hysterectomy. The high mortality in the early experience of most operators was due to the fact that they operated on unsuitable cases where the disease was too far advanced. He had only lost one patient out of his last forty. He agreed that a systematic removal of glands which were not enlarged could not be complete and added unnecessary risk by taking up extra time. As regards ultimate results, it was too early to speak. He had had several very discouraging cases of early recurrence.

Dr. BLACKER said that the most important question with regard to the treatment of cancer of the uterus by Wertheim's operation was whether this operation procured a greater freedom from recurrence than any other. His own experience so far had not been encouraging. Of twenty-five cases in which he had performed the operation (the first in December, 1905), three patients had died from the operation, a mortality of 12 per cent., and nine patients were dead or had recurrence of the growth. He still thought, however, that this operation should he performed in all cases in which it was possible, since Wertheim's own results were better than those obtained by any other operator. He practised Sampson's method of exposing the ureters by stripping the posterior layers of the broad ligament off the side wall of the pelvis and picking up the ureter between the finger and thumb. He considered this an easier and more certain method than that of cutting down upon the ureter through the peritoneum.

Dr. FREDERICK MCCANN said that there were many important points for discussion in the papers read that evening, but owing to the lateness of the hour he would refer more especially to certain details in the technique of the Wertheim operation. Dr. McCann thought that it was best to begin the operation by removing the iliac glands, and in that way gradually working downwards in the pelvis, to endeavour to remove the lymphatic tracts, thus imitating the operation for breast cancer. As the present practice was to remove the glands after the excision of the uterus, appendages, etc., and then only if enlarged, it seemed to him absurd to speak of a "complete operation." One part of the Wertheim technique which did not appeal to the surgical mind was stitching the peritoneum over a piece of gauze and leaving a dead space between the peritoneum and the remnants of the cellular tissue. It was much better to stitch the peritoneum covering the bladder to the cut vaginal wall anteriorly and the rectal peritoneum to the posterior vaginal wall, and to unite the peritoneum laterally as done by Bumm and others. Mr. Childe had asked for an expression of opinion on the methods of preventing infection of the abdominal wound. Dr. McCann was careful to protect the abdominal walls by special "protectors" stitched in position, and to avoid any contamination by swabs or sponges which had come in contact with the vagina should any part of the abdominal wall have become exposed. Further, the use of a light retractor, of which Gosset's was a good pattern, and the final cleansing of the abdominal wound with warm saline solution were both helpful. Following Wertheim, Mr. Childe left gauze in the vagina for five days. Dr. McCann thought that was an error, for it was too great a strain on the antiseptic properties of any piece of gauze to leave it five days in the vagina in such cases. Infection undoubtedly occured, and although it might not be severe, yet a mild type of infection would be noted. It was better when definite hæmostasis had been secured not to leave gauze in the vagina, but if gauze were used it should be removed within forty-eight hours. As to the relative merits of abdominal versus vaginal hysterectomy, Dr. McCann repeated the opinion which he had expressed on former occasions, that cases of cancer of the cervix which would formerly have been looked upon as suitable for vaginal hysterectomy would, if treated by abdominal hysterectomy (Wertheim), yield better results, and for advanced cases of uterine cancer the treatment by any method of operating was by no means hopeful.

The PRESIDENT, in calling on the authors to reply, said that he regarded the exhaustive analysis which they had heard from Dr. Leitch of the relation of the lymphatics and glands to malignant disease of the uterus as one of the most valuable contributions which had been made to the subject. He much regretted that there had been so little time to discuss it and Mr. Childe's paper. Both communications invited careful perusal when they appeared in the *Proceedings*. One thing struck him (the President) with regard to all statistics bearing on the results of operations for uterine cancer: If they excluded the primary mortality following on the operation itself, the proportion of deaths after the second year rapidly increased until the fifth and sixth years were reached. After this, though life was occasionally prolonged or a cure could be truthfully said to be effected, the results were very disappointing in the comparatively few survivals. He would not like to go so far as Mr. Childe in saying that after the menopause bleeding always meant cancer. That bogey was constantly in the minds of women, and to generate a widespread belief that in a woman after fifty any hæmorrhage must of necessity mean that she was suffering from cancer, would often create an unjustifiable degree of worry, which in itself might prove a predisposing cause to its occurrence.

Dr. LEITCH, in reply, said that in calculating the duration of life in cancer of the body he selected, from a larger number, 30 cases where no operation had been performed and where the clinical notes were complete as to the date of onset of the first symptoms. The duration, 1·83 years, was contrary to his preconceived notions. Some of them lived as long as six years, and one had a natural tendency to remember the longest duration and forget the more numerous shorter lives, but the average was as stated. He wished, however, to place beside this estimated duration of life in cancer of the body the fact that secondary deposits were found as frequently as in cancer of the cervix. In reply to Dr. Cuthbert Lockyer, he had already mentioned that in about one quarter of the post-mortem cases of cervical cancer there was matting of the pelvic structures, preventing accurate description of the intra-pelvic spread. He relied on his own observations for the description he had given of the latter. He had also referred to the "juxta-cervical gland," following Poirier by calling it a "knot." Histologically, it was a collection of lymphoid cells, but it was not a well-defined lymphatic gland. It was the junction of several lymphatic channels, and he had seen it affected in cancer of the cervix. In hydro-ureter he had sometimes failed to find invasion of the ureter wall, but usually it was involved. To the criticism that an operative attempt to encompass the danger zones, which from the point of view of pathology he had put before the Section, would lead to increased risk to the patient, he could not as a pathologist reply, but as a member of the public he would say that the difficulty might be met by increased surgical dexterity.

Mr. CHILDE, in reply, thanked the members of the Section for their friendly criticism of his paper. It was with the view of eliciting the experienced judgment of those engaged in Wertheim's operation that he had introduced the subject, and in the hope that discussion might be the means of improving the operative results, both immediate and remote, in uterine cancer. He would like, in the first place, to remove a misconception which had arisen in the minds of two of the speakers—Dr. Herbert Spencer and Dr. Lockyer. He alluded to the interpretation of the conclusions drawn from Table B as to the remote results in Dr. Berkeley's cases. He had expressly stated in his remarks that these results were valueless as evidence of non-recurrence. He had only adduced them in reply to a criticism on Dr. Berkeley's paper that recurrences were conspicuously numerous. An analysis of Dr. Berkeley's cases showed that these recurrences had taken place in the advanced cases, and he thought that, if the criticism was fair, the reply to it drawn from Table B was fair also. In stating, as Dr. Spencer had done, that vaginal hysterectomy was applicable to advanced cancer, he thought a good deal hung on the interpretation put upon the term "advanced." He had considered cases "advanced" only when there was evidence of spread of the disease beyond the uterus, fixing that viscus and rendering it impossible to draw it down towards the vaginal outlet. Such cases he had always considered unsuitable for removal by the vaginal route, and he had been in the habit of rejecting them as inoperable. But many of them could undoubtedly be removed successfully, as far as the operation was concerned, by Wertheim's method. He thought he had fairly stated the primary mortality of vaginal hysterectomy as about 5 per cent. Dr. Spencer had shown that a few individual operators had had a considerably lower mortality. That was no doubt a fact, but he thought it hardly a correct inference on those grounds to put the mortality as low as between 1 per cent. and 2 per cent., or "practically nil." If the primary mortality of a large number of experienced vaginal hysterectomists was computed, it would be found somewhere in the neighbourhood of 5 per cent. He did not agree with Dr. Lockyer that gauze packing did not stop hæmorrhage. In his experience there was nothing else as good to arrest oozing from multiple points, where ligation was not only difficult but occupied much time, and the saving of time was the most important determining factor in the immediate success of Wertheim's operation. While the main objects of his paper had been to show that the primary risk need be no deterrent in abdominal hysterectomy for cancer, he quite agreed that the ultimate judgment as to the value of the operation must rest on the late results; with regard to these, he thought speakers were inclined to be too pessimistic. It should be remembered that Wertheim's operation was a new operation, and a very difficult operation. Its distinguished pioneer had occupied many years in perfecting its details, and his experience had run into hundreds of cases. It was not to be supposed that others would at once obtain the same results as Wertheim. But the outstanding fact was that he had been able to achieve results which no vaginal hysterectomist had approached. His was the example we had to follow, and there was no reason why others, with equal perseverance and equal experience, should not obtain as good a record as Wertheim could show from an operation which the instinct of every surgeon must tell him was, as a surgical procedure, superior in every detail to vaginal hysterectomy for uterine cancer. The President had disagreed with him on the point of hæmorrhage after the menopause always meaning cancer; the exceptions in his own experience proved the rule. It was a good enough working hypothesis in attempting to educate women to the urgency of seeking early advice for hæmorrhage after the menopause. Worry had been assigned as a cause of cancer, with as little real foundation, in his opinion, as most of its other causes. He thought the nebulous risk of inducing cancer by worry might be cheerfully run in the attempt to bring the unfortunate victims of this disease into the hands of the surgeon at the only time when it was possible to cure them.

Fibromyoma Uteri causing Acute Intestinal Obstruction.

By J. Barris, F.R.C.S.

This case is thought to be worth bringing before the notice of the Section, not so much from its pathological aspect, but on account of its unusual and interesting clinical history. The specimen is that of a fibromyoma uteri, which gave rise to acute intestinal obstruction; the site of the obstruction being at the level of the small intestine.

The patient was a woman, aged 56, the mother of two children, the younger being 19 years old. The menopause had not yet occurred, but the periods were regular and not profuse. Ten years ago a fibroid polypus was removed by the vagina. She continued in her usual health until October 9, when, at 4 p.m., she was seized with vomiting. This at first consisted only of food. It lasted throughout that night and became clear and watery in character. On following day the vomiting had continued; no flatus and no fæces had been passed; and the patient was admitted to hospital.

On examination the general aspect of the patient, the pulse and temperature, did not suggest an acute peritonitis, and the abdomen was distended but lax. A hard mass could be felt rising from the pelvis to midway between pubes and umbilicus. *Per vaginam*, the cervix was found high up and close against the symphysis; there was a hard, fixed resistance in Douglas's pouch. This was found to be part of the abdominal swelling on bimanual examination. The diagnosis of acute intestinal obstruction due to an impacted fibroid uterus was made.

The abdomen was opened in the middle line, and, on cutting through the peritoneum, many distended coils of intestine were visible. The tumour was pulled through the abdominal wound. There then came into view some intestine which had been impressed between the tumour and the bony pelvis. This portion of the gut was small intestine, and for a distance of about 2 ft. it was dark-plum coloured, showed hæmorrhage beneath the peritoneum and into the mesentery, and was markedly distended. The peritoneum, however, had not lost its natural glossy appearance. The gut below this level was not distended, and was of a pale colour. A subtotal hysterectomy was performed, and as the gut had not lost its glossy colour, the injected portion was not resected, but was returned into the abdomen. The patient made an uneventful recovery.

The tumour itself will be seen to spring from the lower portion of the uterus, and, under the microscope, has the characters of an ordinary fibromyoma.

I have to thank Mr. Waring for his kind permission to publish the case.

The PRESIDENT remarked that acute intestinal obstruction from uterine myoma was a rare occurrence, and he had never met with a case.

A Case of Supposed Hermaphroditism.

By DOUGLAS DREW, F.R.C.S.

IN the *Obstetrical Society's Transactions*, 1896, is an instructive paper by Dr. Blacker and Mr. Lawrence on a case of "Unilateral Hermaphroditism occurring in a Fœtus of eight and half Months," together with an analysis of the recorded cases.[1]

The case that I am bringing before the Section occurred in a living child; it is therefore open to the criticism that absolute proof that it is a genuine case of true hermaphroditism is wanting, as I am only able to present microscopic sections of the genital organ of the right side. The specimen was removed from the sac of a right inguinal hernia in the course of the operation for radical cure; the broad ligament came off from the side of the sac. The child presents external genitalia of the male; there is a somewhat stumpy penis, with a urethra in its normal position (there is no hypospadias); the penis is bound down to the median raphe, and the scrotum is bifid; the right side of the scrotum is small, but the left is well developed, and contains an organ which appears to be a testis; the body, epididymis, and vas can be made out.

In the absence of microscopic sections of both the genital glands, I fear I shall have difficulty in satisfying some critics that the case is one of true lateral hermaphroditism.

Mr. Lawrence, the Curator of the Museum of University College Hospital, has very kindly given me a description of the specimen and of the microscopic appearances of the sections of the ovary and uterus. The specimen consists of the right uterine appendages, together with a portion of the uterus and an attached plate of fibrous tissue probably representing part of the right broad ligament.

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[1] *Trans. Obstet. Soc. Lond.* (1896), 1897, xxxviii, p. 265.

The Fallopian tube, which measures 1·5 in. in length, has the usual convoluted appearance, and terminates in a well-developed fimbriated extremity. Attached to it is a mesosalpinx measuring 4 mm. at its widest part and having a free border passing to the distal extremity of the ovary.

The ovary has the normal relation to the Fallopian tube, and a distinct ovarian ligament passes up from its proximal end to the junction of the tube with the uterus. It measures 11 mm. in length by 4 mm. in width, and has the flattened, elongated, ovoidal form of the normal ovary, with slight indentations at intervals in its periphery.

The portion of uterus present probably represents the right uterine cornu. It consists of a fusiform mass of firm muscular substance, continuous with the Fallopian tube, and measuring 17 mm. in length by 6 mm. in thickness. It is oval in section, and is traversed by a minute canal, which is just visible to the naked eye. The greater part of its surface is covered with normal peritoneum, but along its lower border a flat plate of fibrous tissue is attached to it, and probably represents part of the right broad ligament.

In microscopic section the ovary shows two zones: a central zone composed of a slightly cellular, loose, fibrous tissue containing numerous arteries and veins; and a peripheral zone composed of a richly cellular connective tissue, the cells of which are spindle-shaped and arranged, with the intervening white fibres, in delicate, sinuous bundles. The superficial part of this zone is somewhat clearer and less cellular than the deeper portions, and the surface of the organ is marked by numerous indentations. Scattered sparsely through the peripheral zone are slender columns of spheroidal or polyhedral cells with scanty protoplasm. The columns are mostly two or three cells thick, but in a few spots there are distinct cell-masses with a thickness of seven or eight cells. In one or two of the cell-masses a cell of larger size and with abundant protoplasm is found lying among the smaller cells, but no definite primordial follicles are present. An imperfect layer of low cubicle epithelium can be recognized in places, chiefly in the depressions on the surface of the organ.

The uterus is composed mainly of wavy bundles of smooth muscle-fibres, among which are numerous large and thick-walled arteries. The lining of the canal previously mentioned shows longitudinal folds, and is covered with a single layer of columnar epithelium, beneath which there is a thick submucous layer composed of closely-packed, oat-shaped cells. No uterine glands are present.

The PRESIDENT said that they had the question of hermaphroditism before them at the last meeting of the Section. As to its anomalous embryological cause, they had not made much advance since Sir James Simpson wrote his paper in the sixties. In this was a drawing of a case of spurious hermaphroditism, somewhat similar to the one now shown by Mr. Drew. They could not positively say that this child was an instance of true lateral hermaphroditism, inasmuch as they had no absolute proof that there was a testicle at the left side. The tissue of the tumour removed was pronounced by Mr. Lawrence to have ovarian elements, but no follicles were found.

Sarcoma of the Uterus in a Patient aged 25.

By DOUGLAS DREW, F.R.C.S.

CASES of sarcoma of the uterus are always viewed with a certain amount of scepticism, and I have no doubt that this case will not be exempt from criticism. Histologically, I think there can be no doubt as to its nature.

The patient was admitted to the Hospital for Women on April 10, 1910, in an extremely anæmic state. The history stated that during the last three years she had suffered with severe menorrhagia, lasting three weeks at each period, which completely incapacitated her. She was operated on in the London Hospital about three years ago, but did not obtain more than temporary relief. Owing to the patient's unfavourable condition, amputation rather than total hysterectomy was performed on April 22. It was not until Dr. Stevens, who kindly assisted me at the operation and who undertook the microscopic examination of the specimen, came to investigate it that any suspicion was aroused that it was not a fibroid tumour.

Dr. Maxwell has been very kind in furnishing me with a description and a diagram of the condition, together with microscopic slides of the material that was removed when the girl was in the London Hospital in June, 1907. From this it appears that the sarcoma may have originated in a degenerating fibroid; on the other hand, it is possible that the original tumour was sarcomatous from the beginning, and that the cyst that was evacuated may have developed in a sarcoma, which is not an unusual occurrence.

Description of the specimen: The uterus is of globular shape, with a smooth exterior, and about the size of the fist. On section, the posterior wall is much thinned by the growth, while the anterior wall is hypertrophied, and both are infiltrated by the growth. Situated towards the posterior aspect of the growth are several cystic spaces, which contained clear fluid. The substance of the tumour was firm and of a pink colour.

Microscopic Report by Dr. Stevens.—"All the red material bordering upon a breaking-down cavity is sarcomatous, and the smooth, glassy material invading the uterine wall is also sarcomatous. It is a spindle-celled growth, and contains wide, thin-walled spaces; it shows practically no necrotic areas."

DISCUSSION.

Dr. RUSSELL ANDREWS said that the patient was under his care at the London Hospital in June, 1907, when she was aged 22. For some months the monthly periods had lasted fourteen to sixteen days, and she had become very anæmic. The uterus was regularly enlarged and seemed to contain a fibroid. He dilated the cervix and found bulging into the cavity of the uterus a swelling which he thought to be a large submucous fibroid. When its capsule was incised over half a pint of colourless watery fluid escaped, followed by a considerable quantity of soft necrotic tissue. Microscopically this tissue (sections of which were shown) seemed to be degenerated œdematous fibrous tissue. After the patient left the hospital he lost sight of her. He agreed that Mr. Douglas Drew's sections showed sarcoma.

Dr. AMAND ROUTH alluded to a specimen of supposed myxo-sarcoma of the uterus which he had shown at the Obstetrical Society in 1899.[1] At intervals of a few months on eight occasions a large tumour developed in the wall of the uterus, underwent myxomatous and septic changes and sloughed away, the uterine tumour varying from the size of a six weeks' pregnancy to that of six months. Eventually a vagino-abdominal panhysterectomy was performed, and the patient recovered. The Pathological Committee reported that it was a myxomatous degeneration, and could find no evidence of a sarcomatous change.

Hæmatosalpinx with Torsion of the Tubes.

By A. LOUISE McILROY, M.D.

THE chief interest of this case consists in the fact that it is an example of hæmorrhage into the Fallopian tube apart from pregnancy or any previous pathological condition of the uterine appendages. The patient, a cook aged 46, a widow for the past four years, with a family of four children, the last birth occurring twenty years ago, was sent to me by her doctor on account of severe left iliac pain of sudden onset. She was admitted to the Victoria Infirmary on July 17, 1910.

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[1] *Trans. Obstet. Soc. Lond.* (1899), 1900, xli, p. 367.

Clinical history: Five days before admission she was seized with sudden pain in the left iliac region, which she thought was due to a chill. This pain was accompanied by shivering and sweating, and the patient was compelled to go to bed. There was no history of a fall or any sudden exertion. Vomiting was not present, and the bowels were somewhat constipated. There was no uterine hæmorrhage. Menstruation had been irregular, the last period occurring four months previously after an interval of twelve months' amenorrhœa. The patient did not complain of any previous pelvic symptoms, but for the last two years had a slight prolapse of the uterus.

On examination the patient appears healthy, the tongue slightly furred. The heart and lungs are normal. The abdomen is tender in the left iliac region, with some rigidity on palpation of the hypogastrium. On examination under an anæsthetic the vaginal outlet is found to be lax and the cervix hypertrophied. A mass, the size of an orange, is found in the posterior cul-de-sac and lying over to the left side. This mass is incorporated with the body of the uterus, which cannot be differentiated from it. The diagnosis lies between an impacted fibroid of the uterus and a left sactosalpinx.

On July 22 I performed laparatomy and found the enlarged retroverted uterus to be closely adherent to a mass filling Douglas's pouch. This mass, which was adherent to the sigmoid also, was raised up after separation of the adhesions, and was found to consist of the left Fallopian tube dilated to form a hæmatosalpinx, with a complete torsion at its uterine end. This torsion, which consisted of two complete spirals in the reverse direction to the hands of a watch, was unwound and the pedicle ligatured and the tumour removed. The ovary, which was somewhat atrophied, was found to be lying entirely separate from the tube and was left *in situ*. The right appendages were normal to all appearances. The posterior surface of the uterus was covered with recent adhesions. The fundus was fixed to the anterior parietal peritoneum, and the laparatomy wound was closed in two layers. The patient made an uninterrupted recovery, and was dismissed well on August 7.

The pathological specimen consists of the left Fallopian tube distended with blood-clot. The surface is dark red in colour, being mottled by areas of ecchymosis. There is no evidence of necrosis. The contents are semi-solid in consistence. The walls of the tube show hæmorrhagic infiltration. About an inch from the uterine end the tube becomes distended, forming a sac about the size of an orange. The surface of this portion is somewhat irregular in outline. The abdominal ostium is closed; the remains of the fimbriæ are seen spread out as ridges on the interior of the tube. On the external surface is seen a small nodule which represents the hydatid of Morgagni.

Histological examination of sections taken from the uterine end of the tube which formed the pedicle of the tumour shows marked infiltration of the walls by blood corpuscles, these appearing normal in outline, thus proving the short duration of the interstitial apoplexy. In the inner layer the tubal villi have disappeared, the mucosa consisting of irregular groups of epithelial cells in a mass of blood corpuscles. In the middle layer the muscular bundles are broken up by the extravasated blood. The peritoneal covering is thinned out, the nuclei of the cells being elongated. The nuclei of the stroma cells are poor in chromatin. Numerous blood-vessels engorged with blood are seen scattered through the wall. Doubtless many of these vessels are of new formation. There is no evidence of sclerosis in the vessels, nor are there any cells which would indicate the presence of a pregnancy. Sections taken from the dilated portion of the wall show the same condition.

Remarks.—The interest of the condition lies in the absence of pregnancy in the tube, this being the usual cause of hæmatosalpinx. There is no evidence of any previous salpingitis. The hæmorrhage seems to have been caused by the torsion of the uterine end of the tube bringing about a sudden congestion with escape of blood into its lumen. The problem is, What part has the uterus played as a factor in the condition? There must have been some sudden exertion, which in all probability caused an accentuation of a previously existing backward uterine displacement, which in its turn caused pressure upon, and torsion in, the tube lying close behind it. That there was considerable rise of blood pressure in the tube is evidenced by the infiltration of its walls. It is more probable that the extravasation of blood in the tube has been secondary to the torsion than that torsion has occurred in a pendulous cystic dilatation of the outer end of the tube.

The PRESIDENT thanked Miss McIlroy for her communication, which added another instance to the fact they were familiar with. that torsion of the tube caused hæmatosalpinx or hæmatocele.

Obstetrical and Gynæcological Section.

January 5, 1911.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

Subtotal Hysterectomy for Fibromyoma Uteri: Forty Additional Histories.

By Alban Doran, F.R.C.S.

In November, 1905,[1] I read, at a meeting of the Obstetrical Society of London, a communication bearing the title "Subtotal Hysterectomy: After-histories of Sixty Cases." I had already discovered that long series of "successful cases" of hysterectomy for fibroid tumours, when "success" simply means recovery from operation, teach little or are actually misleading, for "recovery" is by no means synonymous with cure. The tumour is gone, but certain discomforts may for a while embarrass the patient, and the surgeon should be aware of the nature of these possible discomforts. On that account I prepared tables of sixty cases in my own practice where the patient had not reached the menopause, and where an after-history of at least two years was obtainable—tables drawn up after Dr. Crewdson Thomas's method. When one or both ovaries are saved a two years' history is absolutely needed, for, as I then noted, and as the present record of further experience will show, a kind of period may be observed for many months after a subtotal hysterectomy where both ovaries have been removed. Therefore I then rejected, as I now reject, many interesting cases where the after-histories were too short or otherwise imperfect. The main object of my memoir was to determine how far patients receive lasting benefit from subtotal hysterectomy, how far they are liable to certain discomforts, how far the preservation of more or less ovarian tissue may save them from these discomforts, and, lastly, how far it may be necessary to preserve some of the endometrium, or, in surgical language, how far it is advisable to amputate the fibroid uterus above the level of the os internum.

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[1] *Trans. Obstet. Soc. Lond.* (1905), 1906, xlvii, p. 363.

In this communication I analyse forty similar cases from my own operative practice as before. The operations in the new series were performed between February, 1903, and February, 1908. Thus I have collected 100 cases, which simplifies, among other things, the calculation of percentages, whilst the value of the two series is in other respects greatly enhanced by their combination.

I earnestly hope that operators who prefer panhysterectomy in the treatment of uterine fibroid will see their way to publish similar analyses of their operative practice. I do not deny that the complete operation has its advantages. I will not discuss either the immediate or distant benefits or risks associated with the preservation of the cervix, for the aim of this communication is not the consideration of the purely surgical merits of a certain operation, but the making known of its remote effects on the patient. I exclude all other considerations, many of high importance, such as the value of the cervix as a support to the pelvic floor.[1] On the other hand, I must be excused for avoiding all purely physiological questions, and therefore I will not discuss the important monographs of Scott Carmichael and Marshall [3], Blair Bell, and Hick [1], Bond [2], and the British and foreign workers who have recently experimented on transplantation.[2] We must study with care the reports issued by these distinguished authorities; and, for my part, I follow their example when I publish the result of my experience. With all respect to them I must direct attention to the truth that women are not rabbits. When we remember how many fallacies have arisen through theorizing on the homologies of bones, how can we rely on theories about the homologies of the ovary and endometrium in woman and the lower mammals? We must not take for granted any assumption that lutein is the same in *lepus* as in *homo* and that the endometrium immediately above the cervix bears the same relation to the ovary in the rabbit as in our own species. At the same time, we know that the able, painstaking, and experienced workers to whom I have just referred will do their best to throw more light on these dark questions.

Dr. Arthur Giles, in an important statistical treatise on his operative work [4], finds that "after hysterectomy menstruation usually ceases; but if a small portion of the body of the uterus be left, as may happen in a high supravaginal amputation, menstruation may continue regularly for a longer or shorter period with no other apparent difference than a diminution in quantity." His experience agrees with my own, but his tables are not drawn up quite after the plan which Crewdson Thomas and myself have adopted, as many after-histories are too short for the present purpose, and cases of panhysterectomy and amputations for fibrosis as well as fibromyomatous tumour are included. Still, although a precise scrutiny of the level of amputation would have been desirable, we must admit that Dr. Giles has taken great pains in his analyses of menopause symptoms after subtotal hysterectomy. Mrs. Scharlieb has published two instructive series of tables of operations for fibromyomata of the uterus [9], but not quite on the same plan as my own, and many after-histories are short, but in the discussion on the second paper I learnt that her cases were followed up as far as possible, so that we hope to hear more from so competent an authority.

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[1] In none of my hundred cases did prolapse of the vagina develop; Giles finds that "the fate of the cervical stump after supravaginal hysterectomy need cause no apprehension."

[2] "The Possibilities of Ovarian Grafting in the Human Subject," *Journ. of Obstet. and Gyn. of Brit. Emp.*, 1907, xi, p. 215.

Lastly, in respect to statistical tables designed to throw more light on the questions under consideration, the Section would, I am sure, be much interested if two strong advocates of panhysterectomy well known and honoured by us all would furnish us with analytical tables such as I have prepared. I refer, I need hardly add, to our former President, Dr. Herbert Spencer, and to my friend and colleague, Mr. J. D. Malcolm, who once, four years ago, favoured us at the Obstetrical Society with an important memoir on the surgical management of the cervix in hysterectomy for fibroids [7].

In regard to the recently expressed opinions of others in respect to the saving of the ovarian tissue and the preservation of a piece of endometrium by amputation above the os internum, I find that some writers are in accord with views expressed in this communication.

Kelly and Cullen [6] declare, on statistical evidence which they publish, that the surgeon should, wherever possible, save every normal ovary. In their series of fifty-six cases where one or both ovaries were saved the after-histories are shorter than two years in at least the last two cases, a fact which might draw down criticism from those who make light of conservative methods. In order to exclude absolutely any abnormally early appearance of the menopause, these writers only include in their table cases in which the patients were not over 41 years of age. I have included in my series all patients who still menstruated, as the sudden arrest of the catamenial function is to be avoided, if possible, at any age; and if a relatively aged woman in whom that function persists is subjected to operation, it is both interesting and important to ascertain the after-history.

Dr. Howard Kelly, however, writing independently, advocates preservation of a piece of endometrium, as well as one or both ovaries [5]. "I have in selected cases proceeded upon the plan of keeping a portion of the uterine body with the mucosa above the cervix in women who were still menstruating, in this way succeeding in preserving regular menstruation in many cases of non-malignant disease for the most part treated to-day by a cervical amputation of the uterus or even by panhysterectomy. The two classes of patients best fitted for this plan of treatment are, on the one hand, those with fibroid uteri not suitable for myomectomy, nor demanding panhysterectomy; and, on the other, the large group of subinvoluted hæmorrhagic uteri. It is a *sine qua non* in each case that one or both ovaries must be sound enough to be retained. It is sufficient to retain a little pocket of the mucosa within the uterus big enough to lodge the end of the little finger, while more should be kept if possible; with the mucosa the corresponding portion of the muscular investiture is retained. The tissues so conserved are then sutured face to face, as not seriously to interfere with the blood supply. An amputation of this sort is easily done by simply cutting through the uterine wall well above the cervix, starting, in other words, to make a high amputation in and not below the uterine body."

Thus, like Abel, Zweifel, and myself, Howard Kelly deliberately saves some of the endometrium when possible in order to prevent, as far as practicable, a sudden cessation of the menopause. He is yet more conservative than other operators, for he "grades the amputations done *in utero* from one which is barely made supra-cervical to one which is done at the very fundus itself, and is principally a myomectomy." He practises a horizontal and a vertical amputation, describing the steps of each operation. He publishes nine results, but no doubt a longer series will some day be issued.

These tables certainly show that there can be no doubt that the preservation of ovarian tissue is advisable, provided that the ovaries are healthy and that their sacrifice is not demanded for certain surgical reasons. The preservation of a piece of endometrium seems almost equally advisable. The saved fragment may, it will be seen, carry on menstrual functions when both the ovaries are sacrificed. The advantages of sparing a piece of the uterine mucosa came out clearly in Group II, where one ovary was spared. The point is obscured in Group III, where both ovaries were saved, owing to certain non-essential complications present in no less than three cases.

Subtotal Hysterectomy for Fibromyoma Uteri. Forty Additional Histories.

No.	Date of operation	Age of patient	Character of fibroid	Treatment of ovaries and line of amputation	Date of last report	Condition according to last report
61	February 19, 1903	37 (married)	11½ lb.; broad ligament opened up; hæmatoma of omentum, severe menorrhagia	Both removed; above os internum	May, 1909	Strong and well, stouter; periods never seen since operation, flushings daily over face still
62	March 3, 1903	49 (married, still menstruating)	2 lb. 6 oz., occupying whole posterior wall of uterus and cervix; metrorrhagia ten years, great anæmia	One (left) saved; below os internum	June, 1909	Good health; show three or four times after operation, occasional flushings and vertigo still
63	April 28, 1903	43 (single)	Bulky fibroid, extending above umbilicus and down into pelvis; menorrhagia	One (right) saved; below os internum	February, 1907	Fair health, obesity; distinct show twice since operation, occasional flushings and vertigo still
64	October 15, 1903	44 (married)	Fibroid, size of tangerine orange, enucleated from anterior wall of supravaginal cervix; severe metrorrhagia, double aortic disease (*see* text)	Both removed; above os internum	August, 1910	Fair health, obesity; periods continued regular for two years, no flushings or other neuroses
65	November 12, 1903	38 (single)	3 lb., hard, extending into pelvis; broad ligament invaded; menorrhagia, pressure symptoms	Both saved; below os internum	May, 1909	Good health, gained flesh; no periods since operation, no flushings
66	December 12, 1903	40 (married)	15 lb., cystic perithelioma; rapid increase, tapped once; no menorrhagia	One (left) saved; right ovary fibromatous; above os internum	May, 1908	Good health, obesity; no sign of recurrence, periods regular three years, no flushings (*see* author and Lockyer, "Two cases of Uterine Fibroid showing Peritheliomatous Changes," *Journal of Obstetrics and Gynæcology of the British Empire*, 1908, xiv, p. 320)
67	January 7, 1904	27 (single)	2 lb. oval fibroid, pressing on bladder; no menorrhagia; temporary insanity after influenza four years before operation	Both saved (*see* text); above os internum	June, 1909	Good health; periods regular for nearly two years, last period four and three-quarter years after operation; periodical epistaxis for about a year, when periods were disappearing; occasional pain, left breast

No.	Date of operation	Age of patient	Character of fibroid	Treatment of ovaries and line of amputation	Date of last report	Condition according to last report
68	February 10, 1904	41 (married)	2 lb., bulky, rapid growth; no menorrhagia	Both ovaries saved; above os internum	March, 1908	Good health, obesity; periods continued for over one year, no neuroses
69	April 21, 1904	48 (married)	3½ lb. fibroid of cervix, portio vaginalis invaded; no menorrhagia, periods still regular	Both ovaries removed (double hydrosalpinx); below os internum	March, 1910	Good health; one show shortly after convalescence, no flushings
70	June 3, 1904	40 (married)	8 lb. 4 oz., rapid growth; no menorrhagia	Both ovaries saved; above os internum	August, 1908	Good health; no periods since operation, no flushings
71	June 14, 1904	34 (married)	Bulky fibroid in anterior wall, undergoing myxomatous degeneration; severe metrorrhagia; thickened endometrium removed by curette one month before operation	Both ovaries saved; above os internum	May, 1909	Good health, obesity; periods still regular, coloured show four-weekly; occasional flushings third year
72	June 21, 1904	49 (married)	1 lb. 9 oz. multiple interstitial myomata; menorrhagia one year	One ovary saved; above os internum	June, 1909	Good health; periods regular for seven months after operation, then within six months irregular flushings and complete menopause
73	October 4, 1904	43 (married)	4 lb. hard fibroid, pressing on bladder; cervix and broad ligament involved	Both ovaries removed; below os internum	July, 1909	Good health; no periods since operation, flushings once or twice weekly for a year and occasionally still; dyspeptic
74	November 3, 1904	41 (single)	6 lb.; large mass, extending into posterior wall of cervix; menorrhagia (exophthalmos tachycardia)	One ovary saved; above os internum	May, 1909	Very good health; no periods since operation
75	January 24, 1905	50 (married)	2 lb. 12 oz. hour-glass fibroid, dragging on bladder; no menorrhagia	Both ovaries saved; above os internum	May, 1909	Sickly; no periods since the operation; flushings, perspirations, and debility, continuing till date of last report
76	February 14, 1905	38 (single)	Large fibroid reaching above umbilicus, rapid growth; severe menorrhagia	One ovary saved; above os internum	May, 1909	No periods since operation, no flushings; dyspepsia, occasional tympanitic distension of abdomen

77	June 1, 1905	42 (single)	Fibroid in anterior wall of uterus, another in left broad ligament; severe pelvic pain; menorrhagia	Both ovaries removed; above os internum	September, 1910	Slight show four times since operation; severe flushings and headaches for several months over three years after operation, then insanity, following mental shock and influenza
78	June 17, 1905	36 (married)	2½ lb. "cystic perithelioma," rapid growth; emaciation, menorrhagia	Both ovaries removed; above os internum	February, 1908	Good health, no sign of recurrence; no periods since operation; flushings one year; *see* author and Lockyer, *loc. cit.*, No. 66
79	July 6, 1905	43 (single, sister to Nos. 6 and 12)	8 lb. 2 oz., rapid growth, no menorrhagia	One ovary saved; above os internum	July, 1909	Good health; periods regularly for one year after operation, last show about six months before date of last report
80	October 24, 1905	35 (single)	2¾ lb.; menorrhagia (adenomatous polypus discovered in uterine cavity)	One (left) ovary saved; above os internum	November, 1910	Good health; periods regular but very scanty, no flushings
81	December 12, 1905	41 (single)	7 lb. 2 oz. invading broad ligament; painful pressure symptoms, slight menorrhagia	Both removed (corpus luteum hæmatoma in left); above os internum	May, 1909	Good health; no periods since operation, occasional flushings
82	January 23, 1906	47 (married)	7½ lb.; very firm pressure symptoms, menorrhagia (*see* text)	Both saved; above os internum	July, 1908	Fair health, plethoric (*see* text); parametritic abscess and sinus, no show of blood from vagina after operation; blood escaped from sinus (*see* text) regularly for three months, then irregularly with flushings; last show two years and four months after operation, then flushings ceased
83	February 1, 1906	46 (married)	2½ lb., three lobes, one submucous; menorrhagia	Both ovaries saved; below os internum	October, 1908	Fair health; no periods since operation, no flushings
84	March 21, 1906	38 (married)	Small three lobed tumour; broad ligament opened up; pressure symptoms, no menorrhagia	Both ovaries removed; below os internum	June, 1909	Good health; no periods since operation, severe flushings and perspirations for a few months after operation
85	May 15, 1906	40 (single)	1 lb. 1 oz., multiple fibroids, some submucous; severe menorrhagia	Both ovaries saved; above os internum	May, 1909	Good health; periods still present though scanty; flushings and faintness after each period till two years and five months after operation; obesity

No.	Date of operation	Age of patient	Character of fibroid	Treatment of ovaries and line of amputation	Date of last report	Condition according to last report
86	May 17, 1906	38 (married)	1 lb. 2 oz., spherical fibroid; menorrhagia	Both ovaries saved; above os internum	June, 1909	Good health; periods still present and regular, occasional pains at periods, no flushings
87	June 26, 1906	45 (single)	2 lb. 1 oz., increasing quickly; abdominal pains, no menorrhagia	Both ovaries saved; above os internum	May, 1909	Good health, except dyspeptic pains; no periods since operation, no flushings
88	June 28, 1906	46 (single)	2 lb. 5 oz., pedunculated myoma and small interstitial myoma in anterior wall; cyst left mesometrium below ovary, dysuria, no menorrhagia; hæmoptysis at 31	Right ovary saved; above os internum	May, 1909	Good health, gained weight, no evidence of pulmonary symptoms, no periods since operation; recently occasional flushings and perspirations
89	July 10, 1906	41 (single)	1¼ lb., multilobular; left broad ligament opened up; pressure symptoms, menorrhagia	One ovary saved, the other was cystic and contained large hæmatoma; above os internum	June, 1909	Quite well; periods regular though scanty, no flushings
90	July 17, 1906	41 (married)	3¼ lb. cervix involved, rapid growth; abdominal pains, dysmenorrhœa, no menorrhagia	Both removed (cystic); below os internum	June, 1909	Good health, obesity; no periods since operation
91	October 29, 1906	47 (married)	Large multilobular fibroid; no menorrhagia	Both ovaries saved; above os internum	June, 1909	Good health; periodical molimen (*see* text) for a year and a half, with show on three occasions; then "depressed spirits" for a month or two; no flushings
92	November 20, 1906	52 (single)	Mass of interstitial fibroids; pressure symptoms and menorrhagia	Both ovaries saved; above os internum	March, 1910	Good health; no periods after operation; no neuroses
93	July 16, 1907	50 (married)	Highly lobulated fibroid, left mesosalpinx opened up; adhesions to mesentery, chronic obstruction; periods still present, becoming irregular	Both ovaries removed; above os internum	January, 1910	No periods since operation; no menopause symptoms (*see* author, "Chronic Intestinal Obstruction due to Adhesions of a Uterine Fibroid to the Mesentery," *Lancet*, ii, 1907, p. 1309)

94	July 23, 1907	42 (single)	2 lb. 6 oz. uterus infested with small fibroids; menorrhagia, recently amenorrhœa; great anæmia	Both ovaries saved; above os internum	August, 1909	Good health, gained colour and flesh; no periods since operation; moderate flushings three to five months after operation; occasional tenderness in region of right ovary
95	July 28, 1907	41 (married)	3 lb. 2 oz., rapid growth; menorrhagia	One ovary saved; above os internum	July, 1909	Good health; no periods since operation; no flushings
96	September 22, 1907	38 (married)	Polypus (adenoma) removed January, 1906; hæmorrhages, August, 1906, till operation; interstitial growth as big as a walnut, also a submucous adenoma	Both ovaries removed; a little below os internum	September, 1910	Good health, obesity; no periods since operation; no menopause symptoms
97	November 5, 1907	38 (single)	Large sessile subserous tumour, 4 in. diameter, and smaller but similar tumour, both on posterior aspect of uterus; pressure symptoms, no menorrhagia	Both ovaries saved; 1 in. above os internum	December, 1909	Good health; periods still regular, four-weekly, except on two recent occasions, when interval was only two weeks
98	January 21, 1908	35 (married)	1¾ lb., deeply lobulated, one lobe blocking Douglas's pouch; no menorrhagia	Right ovary saved; above os internum	January, 1910	Good health; slight, regular periods for a few months after the operation; flushings and headache
99	January 28, 1908	38 (married)	Fibroid, size of turkey's egg on fundus, retroversion; right hæmatosalpinx, free adhesions; no menorrhagia; *see* "Hæmatosalpinx with Acute Symptoms following Retroversion of Fibroid Uterus," *Proceedings of the Royal Society of Medicine* (Obstetrical Section), 1908, i, p. 171	Both ovaries removed; above os internum	January, 1910	Good health, gained flesh; no periods since operation, but severe hot flushes regularly for a few months, now about every two months and less severe
100	February 6, 1908	28 (single)	1 lb. 8 oz. firm fibroid posterior wall, growth rapid; menorrhagia	Left ovary saved; ½ in. above os internum	February, 1910	No periods after operation; no unpleasant symptoms

GROUP I.—BOTH OVARIES REMOVED.

Twelve cases come under this heading—namely, Nos. 61, 64, 69, 73, 77, 78, 81, 84, 90, 93, 96 and 99.

Age: Five between 35 and 40, Nos. 61, 78, 84, 96 and 99: 40 to 45, five, Nos, 64, 73, 77, 81 and 90; 45 to 50, one, No. 69; one fifty years old, No. 93.

Married or single: Ten married, Nos. 61, 64, 69, 73, 78, 84, 90, 93, 96, and 99; two single, Nos. 77 and 81.

Results.

(1) Menopause neither immediate nor complete: One case, No. 64.

(2) Menopause complete, no symptoms: Three cases, Nos. 69, 93 and 96.

(3) Menopause complete, distinct but mild symptoms: Four cases, Nos. 61, 73, 78, 81.

(4) Menopause complete, severe symptoms: Four cases, Nos. 77, 84, 90 and 99.

Analysis of the Cases where the Menopause was neither Immediate nor Complete.

No. 64: Aged 44, married, four pregnancies in last twelve years. Hæmorrhages, severe menorrhagia. Size of tumour, small; it involved the cervix. Line of amputation, above os internum. Ovaries normal.

No. 64 may be ranked among those interesting cases where, although both ovaries were removed, it is certain that some of the endometrium was saved, and where the periods continued for over two years, whilst no flushings or other distressing symptoms preceded or followed their cessation. Last August, close upon seven years after the operation, the patient wrote to inform me that she had grown stout, and had never felt so well since her youth—a "history," however, quite common after the menopause. This patient had been under the care of Mr. T. F. McDonnell, of Hailsham, for severe uterine hæmorrhages, beginning after her last confinement eleven years previously. Recently they had been very severe; one attack two months before the operation lasted five weeks and nearly proved fatal. There was double aortic disease, distinct but not pronounced, and small external hæmorrhoids. I enucleated a fibromyoma of the size of a tangerine orange, which mostly grew in the anterior wall of the supravaginal portion of the cervix. The body of the uterus was very bulky, hæmostasis was difficult, and on that account I did not attempt to finish the operation as a pure myomectomy, whilst the appendages had to be sacrificed in order to secure the ovarian vessels far above the engorged veins on the uterus. The artery of each round ligament required separate ligature. Then the uterus became pale and diminished in bulk. I thought that some endometrium might well be saved, bearing in mind some previous cases, so I amputated above the os internum. The anterior wall of the cervix made a good flap, after careful trimming, over a stump which was not at all bulky.

Relation to cases in the old tables: This case (No. 64) resembles No. 36 in the old tables, where menorrhagia was marked. The periods reappeared three months after the operation and continued regular for seven months; by the end of two years there was only occasional show; there were no flushings. In No. 64 the periods were regular. In No. 12 the periods, though irregular, persisted for over two years, but there were flushings, which subsided when the periods ceased. In No. 29 not only were both ovaries removed but the line of amputation passed below the os internum. A slight mucous discharge persisted, and it was distinctly coloured every four weeks. Possibly, as I suggested when I reported the case, there were endometritic elements in the cervical mucosa.

We see clearly, however, that in 10 per cent. in this group and in 4 per cent. of the whole series, menstruation continued for some time after the removal of both ovaries, though the influence of the other factor under consideration is not evident. They seem to prove that the endometrium can maintain the catamenial functions when the ovaries have been extirpated.

Analysis of the Three Cases where the Menopause was Complete without Symptoms.

Age: The youngest, No. 96, was 38 years old; the eldest, No. 93, was 50; No. 69 was 48.

Married or single: All three married.

Hæmorrhage: Only in No. 96, where a submucous growth had developed.

Size of tumour: 3½ lb. in No. 69; moderate sized, mesosalpinx invaded, mesenteric adhesion in No. 93; small in No. 96.

Line of amputation: Above os internum in one case, No. 93; below in two, Nos. 69 and 96.

Ovaries: Associated with tubal disease in one, No. 69; normal in two, Nos. 93 and 96.

In No. 93, reported in the *Lancet*,[1] I operated on account of symptoms of chronic obstruction due to an adhesion of one lobe of the tumour to the mesentery. This adhesion formed an arch under which coils of small intestine became incarcerated. Although the patient was fifty years of age the periods were still present, with more free show than when she was in the prime of life; such is not rarely the case in fibroid disease. For surgical reasons it seemed best to sacrifice the ovaries, but the uterus was amputated a little above the os internum; the stump was not fleshy and was easily covered over by a small flap. The menopause was complete and there were no flushings or any other symptoms. This satisfactory after-history is not always to be reported when both ovaries are removed from a relatively elderly woman who has not reached the menopause.

Relation to cases in the old tables: Six came under this group, Nos. 1, 6, 31, 37, 41 and 47. In five the line of amputation passed below the os internum; adding Nos. 69 and 96 the total will be seven; in No. 41 this factor was uncertain. Adding No. 93, the total number of cases where the menopause was complete without symptoms, after removal of both ovaries, will be nine—that is, 22·5 per cent. in this group and 9 per cent. in the whole series.

Analysis of the Four Cases where the Menopause was Complete with distinct but mild Symptoms.

Age: Two under 40, Nos. 78 (36) and 61 (37); two under 45, Nos. 73 (43) and 81 (44).

Married or single: Three married, Nos. 61, 73 and 78; one single, No. 81.

Hæmorrhages: Severe in one case, No. 61; moderate in two cases, Nos. 78 and 81; absent in one, No. 73.

Size of the tumour: 11½ lb. in No. 61; 7 lb. 2 oz. in No. 81; 3½ lb. in No. 73; 2½ lb. "cystic perithelioma" in No. 78.

Line of amputation: Above os internum in three, Nos. 61, 78, and 81; below in one, No. 73.

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[1] *Lancet*, 1907, ii, p. 1309.

Condition of ovaries removed: Suspicious in appearance in one, No. 78; normal, but drawn very high up in two, Nos. 61 and 73. Lastly, in No. 81, "there was a true corpus luteum hæmatoma appearing as a blood-cyst of the left ovary. The opposite (right) ovary has ovarian tissue extending into the ovarian ligament to within ¼ in. of the uterine cornu. The ovarian stroma has here produced a corpus luteum of most perfect type, containing a large amount of compact lutein tissue" (C. Lockyer.)

Menopause symptoms in these cases: Limited in all four cases to occasional flushings. The most noteworthy case in this group is No. 61. The patient was relatively young, 37 years of age, and had been married fourteen years; she had borne six children, the youngest being three years old. A large, bleeding uterine fibroid developed; there were strong omental adhesions, and a hæmatoma, appearing as a solid body of the size of a chestnut, lay in the omentum immediately above the line of adhesion; it represented an old hæmorrhage, no doubt due to the omentum being knocked backwards against the fibroid. The uterine appendages were very high up, and, as the tumour was extremely vascular, I found it necessary to tie the ovarian vessels on both sides; the vermiform appendix lay on the capsule on the right side, the broad ligaments were widely opened up, and the base of the tumour was enucleated from their folds. I was able to amputate the uterus immediately above the os internum, leaving a piece of healthy endometrium, whilst the muscular coat was cut thin. It may be noted that, although a large vascular tumour, causing severe menorrhagia, was removed, the operation being lengthy on account of adhesions, enlarged vessels and enucleation, and the ovaries being sacrificed, nevertheless the sudden artificial menopause was not associated with severe symptoms in this case where endometrium was designedly saved.

Relation to cases in the old tables: Nine came under this group—namely, Nos. 2, 4, 5, 13, 16, 23, 28, 33 and 38. In all the nine mild flushings were noted, No. 23 had tinnitus, and No. 33 vertigo as well. In four cases the line of amputation passed below the os internum. Adding No. 73 in the present series, the percentage will be 5·0. In five cases the line passed above the os internum. Adding Nos. 61, 78 and 81, the percentage will be 8·0. The gross percentage of cases in this group where the menopause was complete, with distinct but mild symptoms, is 32·5, or 13 per cent. of the whole series.

Analysis of the Four Cases where the Menopause was Complete with severe Symptoms.

Age: Two between 35 and 40, Nos. 84 and 99; two between 40 and 45, Nos. 77 and 90.

Married or single: Three married, Nos. 84, 90 and 99; one single, No. 77.

Hæmorrhages: None in three, Nos. 84, 90 and 99; distinct menorrhagia in one, No. 77.

Size of tumour: In none was the tumour large; the operation was undertaken on account of pressure symptoms, &c.; in No. 77 there was also a fibroid in the left broad ligament, and in No. 90 the cervix was involved.

Line of amputation: Above os internum in two, Nos. 77 and 99; below in two, Nos. 84 and 90.

Condition of ovaries removed: In the worst case, No. 77, the appendages were unavoidably damaged during the operation; in No. 90 both ovaries were cystic; in Nos. 84 and 99 I found it advisable, for surgical reasons, to remove the ovaries.

Menopause symptoms in these cases: No. 77 was the worst in the whole series, as the patient became insane; but it is by no means clear that this mental disease was due to the operation. The patient was a cook, aged 42, subject to severe pelvic pains for two years, which kept her from her work. When under Dr. Cuthbert Lockyer's observation for five months she suffered from menorrhagia. Protopsis and tachycardia had been noted by a former medical attendant, but, although the eyes were very prominent, the pulse on admission was 84. There was no thyroid enlargement, as in No. 74. The uterus was enlarged by a fibroid in its anterior wall, and pushed to the right by a solid oval body as big as a turkey's egg. I removed the uterus and ovaries in June, 1905, enucleating the fibroid in the left broad ligament. The stump was fleshy, and I trimmed it freely. This patient was a rheumatic subject. A little show was observed four times after the operation, but it is doubtful if it were menstrual. She returned to her duties as cook, and enjoyed very good health until November, 1908, over three years after the operation, when her health broke down. She suffered from constant flushings, worse by day; the last show was then observed. Tinnitus and deafness on the right side developed after November, and disease of the middle ear was detected; the flushings continued through

1909, but grew less severe. The patient complained of vertical headache and a "pumping noise." In the winter 1909-10 she improved greatly, but she rather suddenly became insane in the spring of 1910. Summoned home on account of her mother's illness, she arrived too late, finding her parent dead. This caused severe shock, and she then had an attack of influenza. On June 1, 1910, the medical superintendent of the Berkshire Asylum reported that she was "very lost and rambling, and has periods of excitement." She was afterwards removed to Hanwell. Her sister had been twice under restraint. In the remaining cases, Nos. 84, 90 and 99, the symptoms were nothing worse than severe flushings, generally with perspiration.

Relation to cases in the old tables: Ten cases came under this important group—namely, Nos. 3, 10, 14, 25, 26, 30, 35, 42, 44 and 54. In eight the symptoms were, as in the present group, simply severe flushings, but Nos. 25 and 26 were graver cases. The second, No. 26, was already intemperate before the operation; for three years after it the patient was troubled with flushings, which then ceased, and at the same time she resumed her intemperate habits, and died of carbolic acid poisoning (suicidal ?) at the end of six months. No. 25 resembled No. 77 in one respect, but the patient had already been under restraint before the operation, although I was not informed of the fact until afterwards. About five months after the operation she was troubled with persecutory delusions, which occasionally recurred for a year or two. This patient, aged 41, did not suffer from flushings. It will be seen that in Case 67 in the third group of this series I was informed that the patient had been temporarily insane before I undertook the operation. In six cases in the old tables amputation was made below the os internum; adding Nos. 84 and 90, the total will be eight. In the remaining four there was uncertainty about the level. The gross percentage of cases in this group where the menopause was complete with severe symptoms is 35, or 14 per cent. in the whole series. It will be seen that the percentage is much lower in the second and third groups where the patient was not deprived of all ovarian tissue.

GROUP II.—ONE OVARY SAVED.

Thirteen cases come under this heading—namely, Nos. 62, 63, 66, 72, 74, 76, 79, 80, 88, 89, 95, 98 and 100.

Age: One 28 years old, No. 100; three between 35 and 40; Nos.

76, 80 and 98; six between 40 and 45, Nos. 63, 66, 74, 79, 89 and 95; three between 45 and 50, Nos. 62, 72 and 88.

Married or single: Five married, Nos. 62, 66, 72, 95 and 98; eight single, Nos. 63, 74, 76, 79, 80, 88, 89 and 100.

Results.

(1) Menopause neither immediate nor complete: Eight cases, Nos. 62, 63, 66, 72, 79, 80, 89 and 98.

(2) Menopause complete, no symptoms: Four cases, Nos. 74, 76, 95 and 100.

(3) Menopause complete, distinct but mild: One case, No. 88.

Analysis of the Eight Cases where the Menopause was neither Immediate nor Complete.

Age: Two between 35 and 40, Nos. 80 and 98; four between 40 and 45, Nos. 63, 66, 79 and 89; two between 45 and 50, Nos. 62 and 72.

Married or single: Four married, Nos. 62, 66, 72 and 98; four single, Nos. 63, 79, 80 and 89.

Hæmorrhages: Severe and very chronic, No. 62; moderate menorrhagia associated with adenomatous polypus, No. 80; menorrhagia, Nos. 63, 72 and 89; none in Nos. 66, 79 and 98.

Size of tumour: Very large in No. 66 (15 lb. peritheliomatous elements); large in Nos. 63, 79 and 98; moderate in Nos. 62 and 80; small in Nos. 72 and 89.

Line of amputation: Above os internum in six, Nos. 66, 72, 79, 80, 89 and 98; below in two, Nos. 62 and 63.

Condition of ovary removed: In one case, No. 89, the left ovary was cystic, and the cyst contained a hæmatolith, a perfectly flat, triangular body, with a base ¾ in. long. There had been much menorrhagia in this case, but neither pain nor ovarian tenderness. In the seven remaining cases, Nos. 62, 63, 66, 72, 79, 80 and 98, one ovary was sacrificed either to facilitate hæmostasis or because the corresponding broad ligament was opened up and damaged.

Menopause symptoms in these cases: In No. 63, where the line of amputation was below the os internum, there were distinct flushings for several years. It is not quite certain that this case should come under this group, but I include it with the remaining seven because the patient declared that the show seen six months and two years and two months after the operation was in both instances associated with molimen, and the flushings were in a certain measure periodical. In No. 62, where the line of amputation was also below the os internum, distinct show was seen three or four times after the operation, and for as long as six years at least there were occasional flushings with vertigo. The patient, at the date of operation, was still menstruating regularly, although 49 years old. In all the remaining six cases some endometrium was left behind. In Nos. 66, 80 and 89 the period continued regular for several years. In No. 66, where the operation (described elsewhere) was very severe, the period continued for three years, and then ceased; I saw the patient two years after the menopause, there had been no neuroses of any kind. In No. 80 the periods were quite regular, although scanty, four years after the operation; there had been no flushings. In No. 89 the condition was precisely similar three years after the operation. In Nos. 72 and 98 the periods continued for a few months, and there were flushings, rather severe, and in No. 98 associated with headache. In No. 79 the periods remained quite regular for one year, and then the menopause came on slowly without any disagreeable symptoms.

Relation to cases in the old tables: As in the new tables, eight cases came under this group—Nos. 19, 34, 39, 43, 51, 52, 57 and 60. In all there was persistence of menstruation for a distinct period, as in Nos. 66, 80 and 89. No. 51 was remarkable, as the periods were regular for sixteen months, then the show never reappeared, but a regular periodical molimen continued for at least a year and a half. In seven cases in the old tables the line of amputation was above the os internum, making, with the six in the new tables, a total of 13 per cent. In the remaining case there was some uncertainty about the level of amputation. The gross percentage of cases under this group is 16. However, another group collected from the old tables has been suppressed as not strictly necessary, I mean instances where the catamenia appeared after operation, but were irregular—five in all.[1] Such cases are here included under "menopause, neither immediate nor complete." In all five the line of amputation was above the os internum, making a total of 18 per cent. The gross percentage of cases where one ovary was saved, and the menopause was neither immediate nor complete, the periods continuing either regular or irregular, amounts to

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[1] Nos. 17, 20, 24, 27 and 58. No. 20 was an instance where the periods continued for four years and perhaps longer, but there was amenorrhœa for six months in the second year. In No. 24 alone were flushings noted.

53·8 per cent. in this group, and 21 in the entire series. I may add that in one of the five (No. 58), added to the group as at present arranged, the same phenomenon was noted such as I observed in No. 51. The periods were regular for some time after operation,[1] then the show of blood ceased; but, for at least two years after its cessation, the menstrual molimen recurred with perfect regularity. In No. 91, belonging to Group III, there was a similar, but shorter, persistence of molimen.

Analysis of the four Cases where the Menopause was complete without Symptoms.

Age: One between 25 and 30, No. 100; one between 35 and 40, No. 76; and two between 40 and 45, Nos. 74 and 95 (both 41).

Married or single: One married, No. 95; three single, Nos. 74, 76 and 100.

Hæmorrhages: Severe in one case, No. 76; slight (distinct menorrhagia) in three cases, Nos. 74, 95 and 100.

Size of tumour: Very bulky, not weighed in one case, No. 76; 6 lb. in No. 74, 3 lb. 2 oz. in No. 95, and 1 lb. 2 oz. in No. 100.

Line of amputation: Above os internum in all four cases.

Menopause symptoms: Flushings and headaches did not trouble any of the four cases. In No. 76 there was, four years after the operation, occasional tympanitic distension of the abdomen, probably due to dyspepsia.

No. 74 deserves some notice. The patient was a single woman, aged 41. She had been in weak health all her life, and latterly very subject to tonsillitis, for which she placed herself under the care of Dr. Tucker, of Barnsbury. She admitted to him that she had frequent attacks of tremors with profuse perspiration ever since she was aged 20, after he had noted that there was marked exophthalmos with distinct enlargement of the thyroid body; the pulse was also unaccountably rapid. For nearly two years this patient was kept under appropriate treatment for the exophthalmos: at the same time abdominal swelling was observed. It developed into a well-defined tumour which extended for 2 in. above the umbilicus. The late Mr. Nunn advised operation. When the patient was admitted into the Samaritan Hospital in October, 1904, I found the exophthalmos marked, without nystagmus or defective sight. There were muscular tremors, and the pulse, even when the patient was kept quiet in bed, ranged from 132 to 140. Dr. C. Hubert Roberts examined her with me. Graefe and Stellwag's signs were absent, the thyroid body showed slight enlargement of the lateral lobes. The temperature never rose above 99° F. before operation. The patient's appetite was good, the tongue rather glossy along the edges. I amputated the uterus above the cervix; the whole mass, which weighed 6 lb., consisted chiefly of a large interstitial fibroid; the left appendages were drawn up very high on the surface of the tumour and rotated to the right of the middle line. I was determined to save one ovary if possible, owing to the patient's general condition, and was therefore well satisfied to find that the right ovary lay low down, well outside the mass. In amputating the cervix I enucleated a large fibroid growth which had developed in the posterior wall of the uterus below the bigger fibroid and invaded the wall of the cervix. After the enucleation I passed the knife above the level of the os internum. Five years later the patient wrote to me saying that she had enjoyed good health ever since the operation, the periods had never returned, but no neuroses of any kind had developed. Dr. Tucker had recently detected renewed enlargement of the thyroid body, and administered fresh thyroid tablets for two months, the enlargement steadily subsiding. The question of the relation of exophthalmos to the functions of the generative organs has recently been discussed by Pinard [8].

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[1] Not over six months; in No. 51 they continued for sixteen months.

Condition of ovary removed: Normal in all four cases, sacrificed for surgical reasons.

Analysis of the Case where the Menopause was complete with distinct but mild Symptoms.

No. 88: Age, between 45 and 50; single. Hæmorrhages: None, no menorrhagia. Size of tumour: 2 lb. 5 oz., with a cyst in the left mesometrium (below ovary). Line of amputation: Above os internum. Condition of ovary removed: Normal, but closely connected with the cyst in the mesometrium.

Menopause symptoms: In No. 88 flushings and perspirations appeared in the third year, although there had not been a trace of show since the operation. They seem to be associated with physical fatigue. On the other hand, the patient gained a stone in weight within three years, yet she had spat blood fifteen years before the operation, and there were pulmonary complications during the first week after the removal of the uterus. It is not certain that the symptoms above noted were really due to suppression of the menstrual function.

Severe menopause symptoms: Let it be remembered that in this sub-series where one ovary was saved and the amputation carried above the os internum, in eleven (all but two out of the thirteen) there was no instance of severe symptoms clearly attributable to the menopause.

Relation to cases in the old tables: Once more I must explain that I have rearranged the groups of the "one ovary saved" series. In the older communication I included under one group all cases where the menopause was complete and immediate after operation independently of menopause symptoms. The total was thirteen[1] against five in the present group, No. 88 being added to Nos. 74, 76, 95 and 100. In eight of the old cases the line of amputation was above the os internum, as it was in all five of the new cases, making a total of 13 per cent. In five of the old cases the line was below. The total percentage of cases where the menopause was immediate and complete is 46·1 in this group and 18 in the entire series. Lastly, whilst in the group "both ovaries removed" in no less than 40 per cent. of the series or 35 per cent. of that group the menopause was complete with severe symptoms, in this "one ovary saved" series the percentage is 0, unless we consider that Nos. 22, 32, 45 and 48, where sharp flushings occurred, should be included under severe cases.

GROUP III.—BOTH OVARIES SAVED.

Fifteen cases come under this heading—namely, Nos. 65, 67, 68, 70, 71, 75, 82, 83, 85, 86, 87, 91, 92, 94 and 97.

Age: One between 25 and 30, No. 67; one between 30 and 35, No. 71; three between 35 and 40, Nos. 65, 86 and 97; four between 40 and 45, Nos. 68, 70, 85 and 94; four between 45 and 50, Nos. 82, 83, 87 and 91; and two over 50, Nos. 75 and 92.

Married or single: Eight married, Nos. 68, 70, 71, 75, 82, 83, 86 and 91; and seven single, Nos. 65, 67, 85, 87, 92, 94 and 97.

Results.

(1) Catamenia regular after operation: Five cases, Nos. 67, 71, 85, 86 and 97.

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[1] Nos. 7, 8, 9, 11, 15, 18, 22, 32, 45, 48, 55, 56 and 59. In No. 11 flushings had been present before the operation.

(2) Catamenia soon suppressed: Two cases, Nos. 68 and 91.

(3) Menopause complete and immediate after operation: Eight cases, Nos. 65, 70, 75, 82, 83, 87, 92 and 94.

Analysis of the Five Cases where the Catamenia were regular after Operation.

Age: One between 25 and 30, No. 67; one between 30 and 35, No. 71; two between 35 and 40, Nos. 86 and 97; and one between 40 and 45, No. 85.

Married or single: Two married, Nos. 71 and 86; and three single, Nos. 67, 85 and 97.

Hæmorrhages: Severe in Nos. 71 and 85; moderate in No. 86; none in Nos. 67 and 97.

Size of tumour: Bulky, reaching nearly to umbilicus, Nos. 71 and 97; weight, 2 lb., No. 67; 1 lb. 2 oz., No. 96; 1 lb. 1 oz. (bleeding), No. 85.

Line of amputation: Above os internum in all five cases.

Catamenial history after operation: In no less than four cases the periods were still regular according to the last report, Nos. 71, 85, 86 and 97; that is to say, they continued for at least five years in No. 71, for at least three years in Nos. 85 and 86, and for at least two years and one month in No. 97. In one case they continued regular for well over one year, and then became irregular, but had ceased before the date of the last report; this case was No. 67.[1] The last period was observed about four years and nine months after the operation; it was unusually free. For several months in the third year there was bleeding from the left nostril about every three weeks, but the patient informed her physician (Dr. D'Esterre) that she felt queer at monthly times though there was no show, and the epistaxis, although regular, was not synchronous with the molimen. This fact makes me distrustful about taking periodical bleedings beyond the area of the genital tract for "vicarious menstruation." I shall return to the subject when I relate the after-history of No. 82. In the fifth year this patient, No. 57, suffered from occasional but irregular attacks of pain in the left breast.

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[1] As this patient had suffered from temporary mental disturbance after influenza four years before the operation, I took care not only to save both ovaries but also to preserve some of the endometrium.

Relation to cases in the old tables: In three cases, Nos. 21, 49 and 53, both ovaries were saved and the catamenia remained regular, more or less, for over two years. In Nos. 21 and 49 amputation was above the level of the os internum, in No. 53 there was some doubt about the level. In the two cases where there was no doubt about it, the periods were still quite regular three years after the hysterectomy, whilst in the third, No. 53, the periods were regular at the end of the same space of time, but there had been amenorrhœa for three months in the second year after operation.[1] Thus in eight out of the twenty-one cases in this group, or over 38 per cent., making 8 per cent. in the whole series, both ovaries were saved and the periods continued for over two years after the operation.

Analysis of the Two Cases where the Catamenia were soon Suppressed.

Age: One between 40 and 45, No. 68; one between 45 and 50, No. 91.

Married or single: Both married.

Hæmorrhages: Moderate in No. 91; none in No. 68.

Size of tumour: Large lobulated tumour, No. 91; 2 lb. tumour, No. 68.

Line of amputation: Above os internum in both cases.

Catamenia after operation: In No. 68 the periods continued regular for over a year and then ceased altogether; the patient, lean at the time of the operation, grew stout. There were no neuroses of any kind, and the patient's health was quite satisfactory four years after the operation. In Case No. 91 the patient was highly neurotic, and on that account I was anxious about preserving both ovaries and a piece of endometrium, and I was able to so do, although one of the lobes burrowed into the right broad ligament. The patient, although aged 47, had been perfectly regular until the operation, but from experience I find that a sudden artificial menopause induced near the climacteric may be as severe as when induced in a young subject. Her medical attendant (Dr. Styan, of Ramsgate) carefully watched the case after operation.

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[1] As in No. 20 in the "one ovary saved" series. In respect to these cases, I noted in my first communication how they showed the importance of a fairly long after-history. At a year and a half both patients seemed to have reached the menopause. I have rejected several similar cases where the history could not be clearly traced for at least two years after operation.

The phenomenon observed in Nos. 51 and 58 in Group II was present in this instance. Irregular show was observed at least three times, whilst fullness of the breasts, always associated with the periods before the removal of the uterus, occurred very regularly every month for a year and a half. Then they ceased, and the patient suffered for a time from depression of spirits, with irritability of temper; there were no flushings. The neurosis soon passed away, and the patient, within three years after the operation, felt free from all physical or mental discomfort. Had the operation been less conservative, I suspect that the neuroses would have been more marked.

Relation to cases in the old tables: As in the present tables, there were two cases in which the catamenia were soon suppressed. In No. 40 the periods occurred three times after the operation and ceased abruptly; in No. 46 they continued for eighteen months and also ceased abruptly, and, as in No. 68, the sudden menopause was not associated with any neurosis. In No. 40 the line of amputation passed above the os internum; in No. 46 there was some uncertainty about the level of amputation. Thus in 4 per cent. of the entire hundred cases, or over 19 per cent. in this group, both ovaries were saved and the catamenia were soon suppressed; neuroses were observed in only one out of the four, and in that instance the patient was already neurotic.

Analysis of the Eight Cases where the Menopause was Complete and Immediate.

Age: One between 35 and 40, No. 65; two between 40 and 45, Nos. 70 and 94; three between 45 and 50, Nos. 82, 83 and 87; and two still menstruating, though aged, in one case 50, No. 75; and in the other 52, No. 92.

Married or single: Four married, Nos. 70, 75, 82 and 83; and four single, Nos. 65, 87, 92 and 94.

Hæmorrhages: Severe in No. 94; menorrhagia, hardly severe in four cases, Nos. 65, 82, 83 and 92; none in three, Nos. 70, 75 and 87.

Size of tumour: 8 lb. 4 oz. in No. 70; 7½ lb., No. 82; 3 lb., opening up broad ligaments, No. 65; 2 lb. 12 oz., dragging on bladder, No. 75; 2 lb. 10 oz., No. 87; 2½ lb., No. 83; 2 lb. 6 oz., No. 94; small, No. 92.

Line of amputation: Above os internum in five, Nos. 70, 82, 87, 92 and 94. Below in three, Nos. 65, 75 and 83.

History after operation: In five cases, Nos. 65, 70, 83, 87 and 92, there was no show of any kind, and none of the patients suffered from flushings or other neuroses.

Nos. 75, 82 and 94 deserve some consideration:—

No. 75: Age 50; married twenty-nine years; first husband lived twenty years; patient re-married shortly after his death; second husband still living. Never pregnant. Hæmorrhages: No metrorrhagia. Notwithstanding the patient's age, the periods were absolutely regular; there was free show for about three days, associated not with pain, but with a feeling of languor. Size of tumour: A large lobular mass weighing, with uterus, 2 lb. 12 oz.; the anterior lobe sprang from the anterior wall, the posterior from the posterior wall of the uterus. The posterior lobe had fallen back into Douglas's pouch, which it filled, the cervix was raised high up behind the pubes, and the anterior lobe tipped upwards, so as to drag on the bladder. Its upper limits lay within 3 in. of the umbilicus. Line of amputation: Below os internum. Catamenial history after operation: In May, 1909, the patient, in reply to my inquiries, wrote: "I have not seen any monthly period since my operation. I suffer very much with flushings and perspirations of the face and lower parts at times." She also had bleeding hæmorrhoids and neglected pyorrhœa alveolaris. Before the operation this patient was a stout woman, with suborbital pigmentation and marked pyorrhœa, which, as above stated, she neglected after convalescence. She had never suffered from any serious illness. The pulse and temperature were normal. She had been under Mr. Butler-Smythe's observation for a few weeks owing to extreme dysuria caused by the falling back of the tumour, as above mentioned. The operation was not very easy owing to the shape of the mass; there was a heavy drag on the broad ligaments, which were clamped and divided; then the tumour was drawn out of the wound, but I was obliged to amputate low down, below the os internum. The after-history is mainly of interest in respect to the conditions noted above, as reported before the operation. The patient's menopause was belated; perfectly regular up to the date of operation, it never reappeared afterwards. The pyorrhœa could not fail to have a bad influence on the patient's health, but one fact remains—namely, the appearance of characteristic flushings such as are observed both in healthy and in sickly women in the natural, and also in an artificial, menopause. In my reference to Kelly and Cullen's opinion on the question under consideration, I explained how I thought it advisable to record instances of late artificial menopauses, of which No. 75 was an eminent and instructive example. Many natural menopauses in healthy women are more severe, and a subject with pyorrhœa cannot be called healthy. Would the flushings have been milder or absent altogether had the amputation been made above the os internum? Would they have been more severe had the ovaries been removed? I feel fairly sure that a positive reply may be given to the latter question. The former, however, cannot be solved; we cannot calculate how far age and the disease existent at the time would prove disturbing factors, however beneficial the saving of the endometrium might be. Kelly, as I have explained, has recently introduced a method of operation designed to preserve endometrium. I hold it very important that late artificial menopauses, as well as earlier ones, should be carefully reported if we desire a thorough knowledge of the value of this conservative step, one of the main questions to which I have turned attention in this communication, and on that account I have dwelt on No. 75 at some length.

No. 82: In this case a troublesome complication occurred which raises once more the question of vicarious menstruation. Age, 47; married thirty years, two children and one abortion; the second child was aged 27, and the abortion occurred a year earlier. The patient was robust, plethoric, and subject during recent years to attacks of vomiting. The urine occasionally contained a trace of albumin. She declared that she had never been laid up with any illness, but alcoholism was evident. Hæmorrhages: The catamenia occurred every three weeks, lasting for about nine days; the show was very free and clots were expelled after attacks of pain compared by the patient to those of parturition. Size of tumour: It was multilobular, one lobe reaching to the left hypochondrium. I operated in January, 1906. When the big lobulated mass was drawn out of the abdominal wound I met with some difficulty in reaching the front of the cervix owing to the presence of two subserous fibroids projecting from the lower segment of the uterus. I made a serous flap by dividing the peritoneum over the fibroids horizontally and turning it down. The uterine arteries were then secured, but in reaching the right artery and vein the parametric connective tissue, hyperplastic and hyperæmic, was unavoidably subjected to much handling. I amputated the uterus a little *above* the cervix. The stump, which was not very fleshy, was covered over with the serous flap, No. 3 van Horn catgut being employed. The right parametrium suppurated and the abscess was opened by an incision in the right groin on March 9. History after operation: No show of blood passing from the vagina was ever observed after the operation. The sinus gave great trouble, and in the last weeks of July, August, and September, 1906, about the time that the period was due, the patient noticed that blood issued freely for several days from its orifice. Flushings with sensations of fullness in the head came on. On March 18 and April 17, 1908, blood began to escape from the sinus and to discharge steadily for three days, ceasing abruptly at the end of that space of time. In the week ending May 23, 1908, the same phenomenon occurred when the patient was under my observation. The flushings then ceased, and I understand that the periodical discharge of blood has not recurred. The patient's general health remained good, although there was some œdema of the right leg, but the plethora and stoutness were more marked than before the operation, when I last saw her in the summer of 1908.

I think it right to record this case where the after-history was somewhat unsatisfactory. I do not think that the infection must have certainly been derived from the piece of endometrium left in the stump. The tissues in the right parametrium were opened up and the catgut used for the flap probably carried infection into the parametric tissue. I had some trouble with catgut in other cases at the same time, and complications ceased when I discarded that which I had in store and made use of the same material from another stock.

The more we learn through experience how to ward off infection, the fewer will such cases become even when we spare some endometrium. Not many years ago the tolerably regular hæmorrhages from the sinus would have been reckoned by most gynæcologists as "vicarious menstruation." But in relating No. 67, I noticed how Dr. D'Esterre reported that the molimen was regular for some time without show, whilst there were regular attacks of epistaxis, but the molimen and the epistaxis were not synchronous. In No. 82 there was no regular molimen, but the hæmorrhages recurred monthly for three months in the first and in the third year, being associated on the latter occasion with flushings. It is much more likely that the hæmorrhages in No. 82 were directly related to the catamenial function, but I am sure that no physiologist would feel certain about their precise signification.

No. 94: A single woman, aged 42. Hæmorrhages: Chronic menorrhagia causing great anæmia, more recently no regular period, but irregular passage of clots. Size of tumour: A bilobed, hard mass occupying the right iliac fossa; the outer was a subserous spherical fibroid about 2 in. in long diameter, the inner being the body of the uterus infested with small interstitial fibroids; there were also three small growths of the submucous type, all fibromyomatous. No adenomatous polypus existed. The whole weighed 2 lb. 6 oz. Line of amputation: Above os internum. Catamenial history after operation: I examined the patient two years and one month after the removal of the tumour. There had been no period since the operation. In the course of the first year she had lumbar pains like the molimen, which occurred at the beginning of the normal periods before her illness, but this molimen was never regular for several months, as in No. 91. At the same time there were occasional flushings. Whilst these phenomena were present the anæmia caused by the hæmorrhages from the fibroid steadily disappeared, the patient's complexion became rosy, and she gained flesh without growing corpulent. The right ovary was slightly enlarged and tender to the touch. The flushings were probably associated with the changes in the circulation due to the removal of the sole cause of the anæmia. The same symptom, but far more severe, was present for a time after operation on one of the worst cases of bleeding fibroid I have ever had under my charge, but, as it must be numbered 102 in my series of hysterectomies for fibroid with authenticated after-histories, it could not be included in these tables. The patient was greatly reduced by periodical bleedings which had gone on for nine years. Both ovaries were removed, being diseased; the uterus was amputated above the level of the os internum. For a few months flushings with "attacks of a very cold sensation" gave trouble, but completely disappeared, and the patient, aged 44, was in good health two years after the operation. This case is not under the same group as No. 94, but in both the sudden stopping of prolonged hæmorrhages has to be taken into account.

Relation to cases in the old tables: Only one case, No. 50, came under the present group. There was a little show of blood for five weeks about fourteen months after the operation, but this show could hardly be reckoned catamenial. Nearly one year later it had not recurred. The patient was then under the impression that she occasionally felt a slight molimen. She was only 30 years of age when the uterus was removed. This case is really akin to Nos. 65, 70, 83, 87 and 92; the "molimen," however, should, I suspect, be better ranked as a neurosis. Thus, in 9 per cent. of the 100 cases, or in over 42 per cent. of this group, both ovaries were saved and the menopause was complete and immediate. Altogether the results were favourable, though in three, Nos. 75, 82 and 94, troublesome symptoms developed, but certain complications had to be taken into account.

SUMMARY OF THE COMPLETE SERIES OF 100.

Thus: In forty cases, or 40 per cent., *both* ovaries were *removed,* and the menopause was neither immediate nor complete in four; in three out of the four the amputation was above the os internum.

In thirty-nine, or 39 per cent., *one* ovary was *saved*, and the menopause was neither immediate nor complete in twenty-one; in eighteen out of the twenty-one the amputation was above the os internum.

In twenty-one cases, or 21 per cent., *both* ovaries were *saved.* The menopause was neither immediate nor complete in eight; in seven, possibly all, of the eight the amputation was above the os internum.

REFERENCES.

[1] BLAIR BELL, and HICK. "Physiology of the Female Genital Organs," *Brit. Med. Journ.*, 1909, i, p. 517, *et seq.*

[2] BOND. "Uterine and Ovarian Physiology and Pathology in Rabbits," *Brit. Med. Journ.*, 1906, i, p. 121.

[3] CARMICHAEL (SCOTT) and MARSHALL. "The Correlation of the Ovarian and Uterine Functions," *Proc. Roy. Soc.*, B, 1907, lxxix, p. 387. "On the Occurrence of Compensatory Hypertrophy in the Ovary," *Journ. Physiol.*, 1908, xxxvi, p. 431, and (S. Carmichael), "Conservative Surgery of the Uterus and Ovaries," *Edin. Med. Journ.*, 1909, N.S., ii, p. 242.

[4] GILES. "A Study of the After-results of Abdominal Operations on the Pelvic Organs: based on a series of 1,000 consecutive cases," *Journ. of Obstet. and Gyn. of Brit. Emp.*, 1910, xvii, p. 370.

[5] KELLY. "Amputation of the Uterus in the Corpus to preserve the Menstrual Function," *Amer. Journ. Obstet.*, New York, 1909, lix, p. 570.

[6] KELLY and CULLEN. "Myomata of the Uterus," Philad. and Lond., 1909, p. 603.

[7] MALCOLM. "On the Advisability of Removing the Cervix in performing Hysterectomy for Fibromyomatous Uterine Tumours," *Trans. Obstet. Soc.* (1907), 1908, xlix, p. 148. Salva Mercadé, "La période post opératoire, soins et accidents," 1910, pp. 450-2, shows that the chances of cancerous degeneration of the stump have been greatly over-estimated owing to certain sources of fallacy.

[8] PINARD. "De l'expression symptomatique appelée goître exophthalmique dans ses rapports avec la fonction de reproduction chez la femme," *Ann. de Gyn. et d'Obstét.*, Paris, 1909, 2me sér., vi., p. 257.

[9] SCHARLIEB. See "An Analysis of a Second Hundred Cases of Operation for Fibromyomata Uteri, with special reference to their Degenerations and Local Complications," *Proc. Roy. Soc. Med.*, 1909, ii (Obstet. and Gyn. Sect.), p. 45.

DISCUSSION.

Dr. GRIFFITH agreed with most of Mr. Doran's conclusions, preferring supravaginal amputation, but he wished to ask him how, in the course of the operation, he was able to determine the position of the internal os uteri and so leave a piece of the mucous membrane of the body. His own experience was that with the deformity produced by multiple fibroids it was not always easy to determine this point by the examination of the parts even after removal.

Dr. HERBERT SPENCER expressed his appreciation of the immense amount of labour Mr. Doran had undertaken in preparing the paper, which, while it had the value of all carefully recorded work, had led the author to draw from it only two conclusions—viz., the ill-effects of removal of both ovaries, and the advantage of leaving a portion of the mucous membrane of the body of the uterus in performing supravaginal hysterectomy. With regard to the removal of both ovaries, it appeared to Dr. Spencer that the author's advice accorded little with his practice, for out of the first sixty cases he had removed both ovaries in twenty-eight, and out of the last forty in twelve cases, altogether in no less than 40 per cent. Of the forty patients from whom both ovaries were removed no fewer than three became insane. With regard to Mr. Doran's request for similar tables dealing with cases of total hysterectomy, Dr. Spencer regretted he was not yet in a position to give them, but on one of the two points in the paper he was able to furnish him with particulars. Out of a consecutive series of sixty total hysterectomies for myoma, Dr. Spencer had saved both ovaries in thirty-five cases (Mr. Doran in six cases), one ovary in nineteen (Mr. Doran in twenty-six), and had removed both ovaries in only six cases (Mr. Doran in twenty-eight cases). In his own six cases the reasons for removal of the ovaries were—in one suspected cancer, in one tubo-ovarian cyst, in one pyosalpinx and ovarian abscess, in two an ovarian cyst on one side and cystic ovary on the other, in one bilateral hydrosalpinx. In short, in not a single case of myoma were both ovaries removed except for serious disease, whereas in Mr. Doran's twenty-eight cases in no fewer than nineteen it was stated that the ovaries were normal. He asked Mr. Doran to inform them, in view of the disadvantages which followed the removal of both ovaries, and of his statement that "when healthy at least one ovary should be saved," why he had removed both ovaries in no less than 46 per cent. of the series of sixty cases. With regard to the advisability of leaving a small portion of the mucous membrane of the body in performing subtotal hysterectomy, the question had for Dr. Spencer only an academic interest, as he had given up the operation in favour of total hysterectomy for the last ten years. He was of opinion that hysterectomy was far too frequently performed for myoma; that a patient, after myomectomy, in which *the uterus* was left behind, had better health than a patient after hysterectomy, by whatever method it was performed: but the important fact had to be borne in mind that the conservative operation was much more dangerous than the radical, and he thought it must increase the risk of "subtotal" hysterectomy to leave a portion of the body behind. He asked Mr. Doran if Case 82 was the only one in which he had trouble in connexion with the stump. He thought Mr. Doran's cases did not support his contention as to the advantages of leaving endometrium behind, for Cases 75, 76, 77, 82 and 93, in which this was done, were actually the only ones in the present series where the health was other than "good" or "fair" after the operation. With regard to leaving endometrium behind when both ovaries were removed, Mr. Doran's cases, and the experience of all who had done the old oöphorectomy for fibroids, showed that the periods usually ceased whether part or the whole of the uterus was left behind; and when they did not he could corroborate Dr. Griffith's statement that neither the patient nor the operator was satisfied until cessation of the bleeding occurred.

Dr. LEWERS said some remarks had been made by the previous speaker (Dr. H. Spencer) to the effect that serious consequences, such as sloughing of the stump, &c., were apt to follow the operation known as "subtotal hysterectomy," where the body of the uterus only was amputated and the cervix retained. Dr. Lewers performed this operation as a general rule, unless there was some special indication for panhysterectomy. He had performed the operation nearly 300 times in the last nine and three-quarter years, and had not met with any unsatisfactory after-results connected with the cervical stump. In a small number of cases the patients had continued occasionally to see a very slight "show" of blood periodically, presumably due to a small portion of the endometrium having been left in the stump. His impression was that these patients suffered less from the disturbances connected with the menopause than did others in whom no such slight losses occurred.

Mr. MALCOLM said that for some years he frequently removed both ovaries in performing hysterectomy. He regretted that he had not found time to obtain systematic details of the after-histories, but many cases had very little trouble from the menopause and a considerable number had no trouble at all. One reason for adopting this course was that in the days when the serrenœud was used, and both ovaries were almost always removed, the results were often in every way good. The field of operations was then very limited, but in the cases where the part of the uterus below the tumour was narrow and sufficiently long to be fixed in the abdominal wound without tension, there was no operation safer or more satisfactory in its immediate and later consequences. When there was much dragging on the stump the results were not so good and there was often much after-trouble, but the facts did not seem to show that the removal or leaving of the ovaries was the most important point influencing the after-histories. Since the last paper published by Mr. Doran on this subject, the speaker, out of deference to the views of the Fellows of the Obstetrical Society at that time, had always left one or both ovaries when this was possible. He had not noticed any marked difference in the results, and in one case, in which he believed at the time of operating that he removed a simple fibromyoma of the uterus and left two healthy ovaries, the patient returned after about a year with an inoperable malignant growth which had every appearance of having developed in the right ovary.

The PRESIDENT (Dr. H. Macnaughton-Jones) said that the two points arising out of Mr. Alban Doran's paper were, first, the removal of one or both ovaries, and, secondly, the preservation of a portion of the endometrium. They were not discussing the relative advantages of myohysterectomy and panhysterectomy. He had not himself had the serious consequences which were said by some to follow the former operation. As to removal of the ovaries, he thought the practice was almost universal of leaving an ovary if it were healthy. The decision to remove the ovaries depended mainly on this. In the classical work of Kelly and Cullen on myomata, out of some 1,700 cases in which myohysterectomy had been performed, there were only some seven instances of serious mental sequelæ or disorders of mentalization following from the operation. In connexion with this fact, Kelly's practice of saving the ovaries had to be remembered. With regard to the second point of preservation of the endometrium, out of the forty cases cited in the present paper of Mr. Doran, there were nine in which the division was made below, and thirty-one above, the os internum. Looking at the results of the whole series, seeing that but very few suffered from any serious post-operative symptoms—for they could not include such symptoms as flushings and other physiological disturbances—it did not appear from his tables that it really made much difference whether a small portion of endometrium were left or not. There could be no doubt that the difficulty in following out the after-histories of cases in which hysterectomy was performed was a weak point in deciding the permanent results of the operation. This could only be possibly done in a proportion of those operated upon, and the time and labour involved in following out the final results in a hundred cases, as Mr. Doran had done, were great. The Section was much indebted to him for this valuable contribution to the subject.

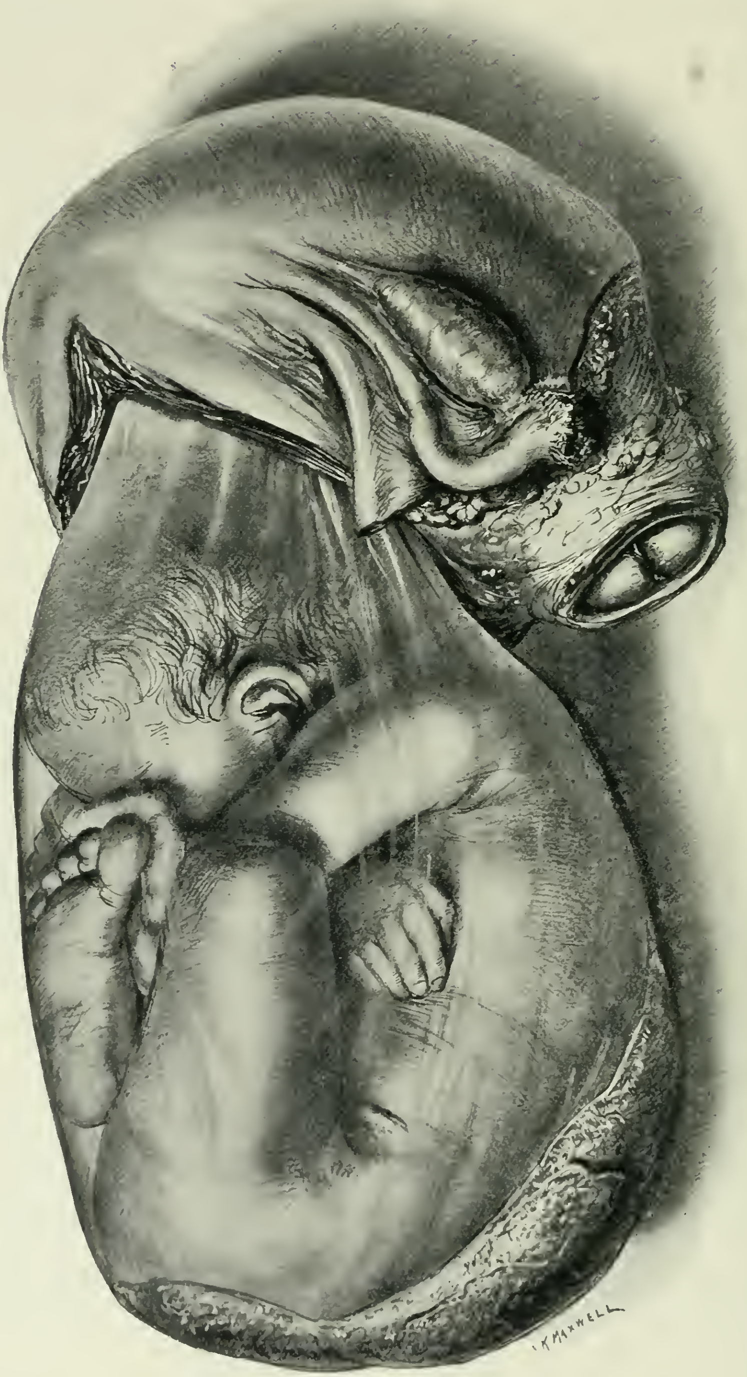
Mr. ALBAN DORAN believed that the level of the os internum was not difficult to estimate, except when the fibromyoma had developed low down in the uterus. He found that many patients, so far from objecting to the continuance of menstruation after hysterectomy, felt more satisfied if it were not suppressed. In regard to the forty cases where both ovaries were removed, he had noted in the text, although not in the tables, of his former communication the condition of those organs in each case. Many were normal, but in this sub-group, as his colleagues were aware, a large number of severe cases were included, where the first duty of the operator was to secure hæmostasis by early ligature of the ovarian vessels. Death from hæmorrhage was a graver matter than a year or two of troublesome neurotic symptoms. With increased experience, however, he found that the ovaries could often be saved without increasing the risk of hæmorrhage. He admitted that the uterine stump became infected in several of his cases, but with improved technique, as Dr. Lewers's and his own recent experience had shown, the dangers of infection might be obviated. He (Mr. Doran) had observed vaginal discharge in some of his cases, but it was never severe. Dr. Herbert Spencer referred to the after-history of No. 75, but the patient, as also No. 82, was a very unhealthy subject. He was interested in Mr. Malcolm's experience, according to which that surgeon's extraperitoneal hysterectomies, where both ovaries were removed, had fared as well as his more recent subtotal or total intraperitoneal hysterectomies, where one ovary at least was usually saved. In conclusion, Mr. Doran was gratified to learn that the President was, like himself, strongly in favour of the publication of series with reliable after-histories.

Gravid Uterus ruptured through the Scar of a Cæsarean Section.

By S. J. Cameron, M.B.

The patient from whom this specimen was removed had been delivered on two occasions by one of the obstetric physicians (Dr. Kerr) at the Glasgow Maternity Hospital. As the pelvis was contracted owing to rickets, Cæsarean section was performed each time, but the Fallopian tubes were not divided or ligated. Impregnation again occurred, and when the patient had advanced to the eighth month of pregnancy she was suddenly seized with acute abdominal pain. A few hours later, as the pain still continued, her medical attendant visited her and immediately ordered her removal to the maternity hospital. On admission the surface of the body was cold and clammy. It was obvious that the patient was dying, and she succumbed ten minutes later. Meanwhile I had been summoned to attend the case, but I did not arrive until a quarter of an hour after the patient had died. The mucous membrane of the lips was blanched, and on abdominal palpation the fœtus was easily felt with its back directed towards the abdominal wall on the right side. A firm mass, which was considered to be the uterus, could be palpated at the pelvic brim on the left side. On vaginal examination the cervix was found to be undilated and no blood had escaped into the vagina. At the post-mortem examination it was discovered that the abdominal cavity was filled with blood. The Cæsarean scar in the anterior wall of the uterus had given way, so that the child in the unruptured membranes was lying almost free in the abdominal cavity. The lower pole of the membranes still remained adherent to the uterus, which was lying well contracted within the pelvic cavity. The child was in the second cranial position.

It would be out of place to enter into the technique of the operation of Cæsarean section at the present moment, but in connexion with the operation of Cæsarean section without sterilization I have brought here this evening another specimen in which rupture of the uterus through a Cæsarean scar was actually in progress when I operated on the patient. This specimen I removed about a fortnight ago from a patient who had not been sterilized after Cæsarean section. She conceived, and three weeks before full time labour commenced. As I had warned our resident medical officer of the possibility of the uterus rupturing, he sent for me



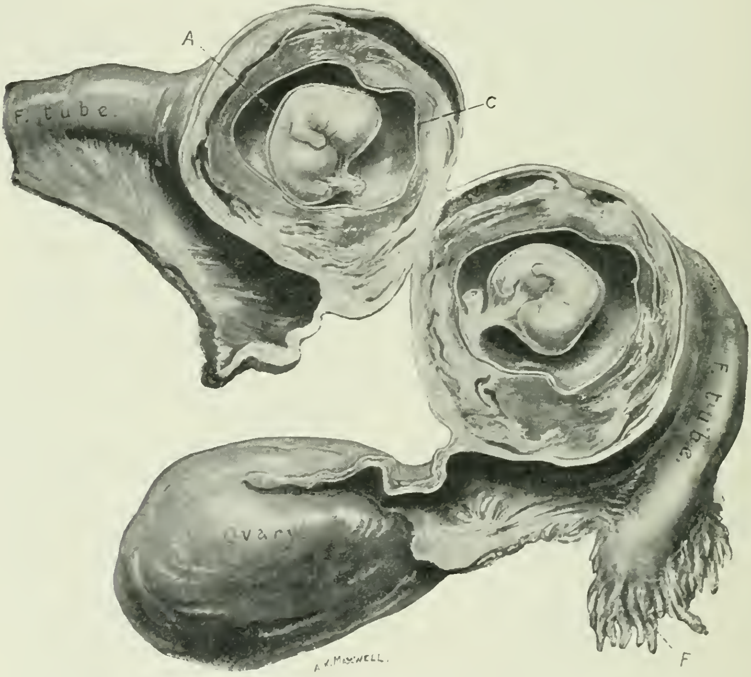
Specimen of rupture of the gravid uterus through a Cæsarean scar in the anterior wall of the uterus.

at the onset of labour. On opening of the abdomen a portion of the anterior surface of the uterus was found to be adherent to the anterior abdominal wall in the middle line. When the adhesions had been divided it was evident that the scar was giving way. The margins of the old scar, below the peritoneal surface, were widely separated by the protrusion of the membranes, which in their passage forwards caused the peritoneum covering the uterus to bulge into the abdominal cavity. At the lower part of the protruding mass, which measured about 2 in. in length, the peritoneum had given way, so that a little blood-stained fluid was trickling through the rent. This blood may have been produced by the separation of fibro-muscular tissue at the lower angle of the weak portion of the scar, or it may have come from the vessels at the placental site. A transverse incision having been made on the fundus of the uterus, the child was extracted through this opening. A supravaginal hysterectomy was then performed. The patient is making a satisfactory recovery. On examining the specimen it will be observed that for some distance over the old scar peritoneum alone separates the interior of the uterus from the peritoneal cavity. A small opening can also be seen at the lower part of this window of peritoneum where rupture had actually occurred. If the finger is introduced into the uterine cavity through the fundal incision, the fibro-muscular margins of the old scar are found to be far apart so that the finger comes at once into contact with the peritoneum. Obviously the operative measures which were adopted in this case averted disaster, as there can be little doubt that in a short time the uterine contents would have been precipitated into the abdominal cavity.

Twin Tubal Pregnancy.

By S. J. Cameron, M.B.

It is seldom that two ova develop in a single Fallopian tube, and for this reason I have brought this specimen here this evening. The specimen was removed from a young woman who nearly succumbed to intraperitoneal hæmorrhage as a result of rupture of the gravid tube. On opening the abdomen an artery in the tube wall was observed spouting into the abdominal cavity, so that a large quantity of fluid blood was contained therein. After the tube had been excised it was evident that rupture had not been aided by intratubal hæmorrhage, but that the rupture had been brought about entirely by the action of the villi on the tissues of the tubal wall. There was no irregular tearing of the tubal tissues such as one sees when intratubal hæmorrhage causes rupture. A small perforation was present, and through this aperture the tips of several villi projected into the peritoneal cavity. After the tube had been allowed to harden in a solution of formalin it was cut across, and then the specimen was discovered to be one of twin extra-uterine pregnancy. As the placenta and membranes were not invaded by hæmorrhage the structures can be easily recognized.



Specimen of twin tubal pregnancy. **A**, amniotic sac; **C**, chorion; **F**, fimbriated extremity of tube.

The specimen, as you will observe, consists of a gravid Fallopian tube which has been severed transversely in the region of the ampulla. Fortunately the knife in dividing the tissues passed between the two small fœtuses, which were probably about seven weeks advanced in development according to the menstrual history. They are, however, considerably smaller than intra-uterine fœtuses of the same age. When the tube was divided the amniotic sac and umbilical cord of the fœtus, which was situated towards the inner portion of the tube, were cut across. Each fœtus has a distinct amniotic sac and their umbilical cords only join as they enter the placental tissue. A considerable space exists between the amniotic sacs and the smooth inner surface of the chorion. On microscopical examination the tissues adjacent to the syncytial cells were found to have undergone pronounced necrosis.

Osteomalacic Pelvis.

By S. J. Cameron, M.B.

Owing to the rarity of osteomalacia in this country, one seldom has an opportunity of obtaining a specimen of an osteomalacic pelvis. The specimen which I have brought here to-night was obtained in the summer of 1910 from a patient who was admitted in a moribund condition to the Glasgow Maternity and Women's Hospital. Her obstetrical history is fairly characteristic of this disease. At the time of her admission she was in her sixth pregnancy. Her first and second labours lasted seven hours, while her third labour only lasted fifteen minutes. Forceps were applied during the first and second labours, but interference was unnecessary with the third. She evidently first came to hospital in the year 1905 (fourth pregnancy), and during labour the head lay in the transverse diameter at the brim, with the anterior parietal presenting. The promontory exercised considerable pressure on the left temporal bone. At the same time it was noticed, on vaginal examination, that a double promontory existed, and that there was antero-posterior contraction at the brim.

The patient having been placed in the Walcher position, forceps were applied, and, after a stiff pull, delivery was effected. At this time some of the pelvic measurements were estimated as follows: Intercristal, 10 in.; interspinous, 9¾ in.; external conjugate, 7 in.; diagonal conjugate, 4 in.; diagonal conjugate to false promontory, 3¾ in.

In May, 1907, the patient, who was now in her fifth pregnancy, returned to hospital in poor physical condition. She was pale and emaciated. Her joints were so painful that the legs were maintained in a position of flexion, while she was also unable to separate her thighs owing to the severity of the pain. The abdomen was very prominent, and again the double promontory was felt on vaginal examination.

The head lay transversely, with the occiput directed towards the right side, and the diagonal conjugate is given as $3\frac{1}{2}$ in. +. The anterior parietal bone was presenting. After the patient had been placed in the Walcher position the head was brought through the pelvic brim and cavity with surprising ease, and it is interesting to note that on this occasion no depression could be discovered on the fœtal head. Ten days after delivery the intercristal and interspinous measurements were found to be 10 in. and $9\frac{1}{4}$ in. respectively. A note in the journal states that the patient was bedridden owing to a chronic rheumatic condition of the thigh muscles from which she had suffered since her last confinement.

Her third admission to hospital was in June, 1910, when she came into hospital in a dying condition. Attempts had been made by her physician to deliver her with forceps, but without any success. On abdominal examination the child seemed to stand on its head on the mother's vertebral column. There was no attempt at engagement, and the child was evidently dead. A destructive operation was commenced for the removal of the child, but the mother died just as delivery was accomplished.

Owing to the approximation of the rami, the hand could scarcely be introduced into the vagina, so that the manipulation of the craniotomy instruments was extremely difficult. In fact, had it not been for the sudden separation of the symphysis, it would have been necessary to remove the child in fragments. So small was the available space that Cæsarean section would have been resorted to for the extraction of the dead child if the mother had not been dying.

The pelvis is a well-marked example of an osteomalacic pelvis, so that we need not go into a general description of it. As the pelvic measurements have been given in inches, the following measurements obtained from the specimen may be briefly mentioned in inches also: (1) Intercristal, $8\frac{4}{5}$ in.; (2) interspinous, 10 in.; (3) true conjugate, 3 in.; (4) from the inner surface of the upper margin of the ischio-pubic ramus over centre of acetabulum to the disk between last lumbar and first sacral vertebræ, (*a*) on right side, $1\frac{2}{5}$ in.; (*b*) on left side, 2 in.; (5) a line drawn from the disk between the last lumbar and first sacral vertebræ to the tip of the sacrum measures 4 in.; (6) anteriorly, the distance between the inner borders of the ischial tuberosities is $1\frac{1}{4}$ in., while posteriorly the inner borders are $3\frac{4}{5}$ in. apart.

It has been suggested that this case was one where osteomalacia had affected a woman who was the subject of rickets. Of this, however, I am not certain. Probably in 1905 the changes produced in the bones by osteomalacia were just commencing, so that difficulty was experienced in delivering the case. In 1907 delivery proved extremely easy, as the bones were then in the stage of softening. Solidification had occurred by the time the patient came under observation in 1910. Except for a slight excess in the connective tissue of the ovaries, no abnormality could be detected in these glands on microscopical examination.

DISCUSSION.

Dr. GRIFFITH asked Dr. Cameron to state what was the reason for which Cæsarean section was done and what was the method of suturing the uterus. In cases on which he himself had operated, and in which Cæsarean section was repeated, either by himself or his colleagues, the line of the previous incision was not recognizable and the uterine wall was as firm and thick as at the first operation. The method he adopted was to insert three or four silkworm gut sutures as the principal ones, using thread for the intermediate sutures. He also pointed out the inaccuracy of the terms "malacosteon" and "osteomalacia"—impossible "classical" words introduced into obstetric literature.

Dr. AMAND ROUTH asked Dr. Cameron why he had feared that the uterus which he had shown was likely to rupture. Was it merely because the patient had previously had Cæsarean section performed? Relatively, such ruptures through the scar of a previous Cæsarean section were very rare. It was true that about 200 cases had been reported, but according to Singer, of Paris, only twenty-seven of these were reported so fully that it was certain a faulty scar was the determining cause of the rupture. He assured Dr. Cameron that if one of his objects in showing the specimens was to encourage sterilization of patients after Cæsarean section, the results of repeated Cæsarean section and the relative infrequency of rupture proved that his views were wrong. In the 1,282 cases of Cæsarean section recently collected by him there had been 108 cases of repeated Cæsarean section with seven deaths—a mortality of 6·4 per cent., which compared very favourably with the mortality of any large number of primary Cæsarean sections. He was not at all sure that the rupture was not adjacent to the scar rather than actually in it.

Dr. HERBERT SPENCER inquired as to the sutures and mode of healing in the Cæsarean sections preceding the rupture. He suspected the sutures would turn out to be made of catgut, which (although it was employed by some high authorities) he considered was an unsuitable material, from its liability to become loose or untied as a result of the contractions and relaxations of the involuting uterus. His experience of four cases of repeated Cæsarean section was that the site of the previous incision was to all appearance as strong as the rest of the uterus. He considered that silk was the best material for the sutures. If catgut were employed or the wound became infected, such disasters as Dr. Cameron had recorded might occur, and means should be taken to prevent them ; but to sterilize an aseptic case of simple contracted pelvis was, in his judgment, quite unjustifiable.

Dr. LEWERS said he preferred silkworm gut sutures for the uterine wound in Cæsarean section. He had done a few second operations on the same patient, and had found the scar on the uterus from the previous operation easily recognizable—in fact, the ends of the silkworm gut sutures could be felt. Such scars had seemed quite sound. There was often omentum adherent to the scar.

Dr. CAMERON stated, in reply to Dr. Griffith's question as to the reason for Cæsarean section having been undertaken in these cases, that antero-posterior contraction at the brim was present owing to rickets. In reply to Dr. Herbert Spencer's question as to the nature of the suture material used, Dr. Cameron stated that catgut was employed. Dr. Cameron, however, believed that catgut was probably a more satisfactory suture material than silk, provided the former was carefully prepared and chromicized so as to resist absorption for three weeks. In reply to Dr. Amand Routh, who inquired why Dr. Cameron had been so careful to warn the house surgeon regarding the possibility of rupture in the second case, Dr. Cameron said that any surgeon who had experienced an accident such as occurred in the first case could never dismiss from his mind the possibility of rupture resulting when a patient who had undergone Cæsarean section again became pregnant.

Interstitial Gestation Sac of between Three and Four Months' Development removed Unruptured with the Body of the Uterus by Abdominal Hysterectomy.

By ARTHUR H. N. LEWERS, M.D.

THE patient from whom the specimen shown was removed was a married woman, aged 29. She was admitted into the London Hospital —on the surgical side in the first instance—on July 17, 1910, and was transferred to Victor Ward under Dr. Lewers's care on the following day. She had been married three years and had had one child eleven months previously. Up to a fortnight before her admission she had been suckling. Soon after the confinement there had been six weeks of amenorrhœa, but after that the catamenia returned, and had occurred regularly every twenty-eight days, till three months prior to her admission to the hospital. Since then she had "seen nothing." The last period, therefore, was in the middle of April, 1910.

Ten days before admission the patient detected a lump low down in the abdomen on the left side. Three days before admission, during the night, she had sudden attacks of acute pain in the region of the lump. After admission there were again acute attacks of pain in the same region. There was no history of any vaginal hæmorrhage since the periods had stopped.

As the patient was seriously ill, Dr. Lewers was sent for to see her about 1 a.m. on July 18. Her pulse then was 120, her temperature 97·5° F., and she was suffering great pain. A mass appeared to occupy the left iliac region, but was not easily examined owing to great superficial tenderness, with rigidity of the abdominal muscles. On vaginal examination no blood was seen. The abdominal swelling seemed to be part of the uterus, which was considerably enlarged. The diagnosis of ectopic pregnancy, possibly interstitial, was provisionally arrived at, and an exploratory laparotomy was decided on. On opening the abdomen the condition was seen to be one of interstitial pregnancy on the left side; it was leaking, with a slight escape of blood, but had not actually ruptured. The only satisfactory way of dealing with the condition seemed to be that of performing a supravaginal hysterectomy, which was accordingly done without any special difficulty. The uterine appendages were not removed. A good deal of shock followed the operation during the first twenty-four hours, but after that the patient made an uneventful recovery.

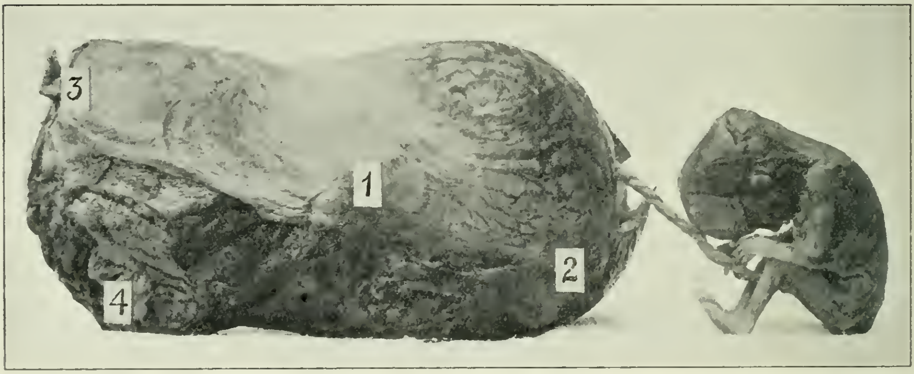
DESCRIPTION OF THE SPECIMEN.

The specimen removed consists of the uterus and an adjacent pregnancy sac, and has the appearance of a bilobed tumour. The two lobes are unequal in size, and separated from each other by a superficial sulcus. The smaller of the two lobes is the unimpregnated enlarged body of the uterus, lying below and to the right; the larger, lying above and to the left, is the ectopic sac. From the cavity of the uterus, at the level of amputation, projects a reddish decidual mass. The ectopic sac is completely globular, about 4 in. in diameter. Over the whole of its peritoneal surface large vessels are seen ramifying, especially from the left tubal stem and the contiguous left border of the uterus. At the lower and inner pole of the gestation sac, and a little external to the above-mentioned sulcus, is seen the thick, hypertrophied left round ligament inserted directly into the sac, and in marked contrast to the small right round ligament; a distance of 4 in. separates the two round ligaments.

At the upper and outer pole of the gestation sac is seen the stump of the left tube, with ovarian vessels ligatured close to the sac, so preserving the ovary.

Anteriorly no breach of surface is seen on the convex, glistening peritoneal aspect of the sac. But posteriorly there are one or two faintly eroded areas where the peritoneum has lost its gloss, and serum exudes through these areas on pressure. This has evidently been the source of hæmorrhage met with during the removal of the specimen.

Anatomical relations show that the case is one of interstitial pregnancy on the left side of the uterus, since the left round ligament is inserted directly on to the sac on its anterior surface, and the normal, unaltered tube lies well to the outer side of the gestation sac. On opening the sac subsequently, after the hardening, the highest point of the sac is seen to be very attenuated in thickness. A "window," cut here in the wall of the sac, passes through a thin slice of placental tissue, and exposes a normal three months' fœtus connected by the umbilical cord to the placenta, which occupies the bulk of the inner surface of the sac. Sections taken through the sac wall show the presence of muscle and the placenta.



Interstitial pregnancy, with unruptured sac at left cornu. The gestation sac has been opened at its thinned-out apex, traversing placental tissue. A well-developed fœtus is seen outside the artificial opening. 1, left round ligament; 2, stump of left appendages; 3, right round ligament; 4, projecting decidua at level of amputation.

Dr. Lewers is indebted to Dr. R. D. Maxwell, Obstetric Registrar at the London Hospital, for the trouble he has taken in preparing the specimen and superintending the preparation of the accompanying photographs.

The PRESIDENT remarked that the interstitial gestation might be one of the variety described by Taylor, in which the primary conception occurred in the tube, followed by a secondary invasion of the uterus, through hæmorrhage or otherwise. The round ligament in Dr. Lewers's case was certainly on the anterior wall of the gestation sac.

Suppurating Ovarian Tumour, perforated into Small Intestine and Omentum, successfully removed with affected Bowel, Omentum, and Appendix.

By HERBERT R. SPENCER, M.D.

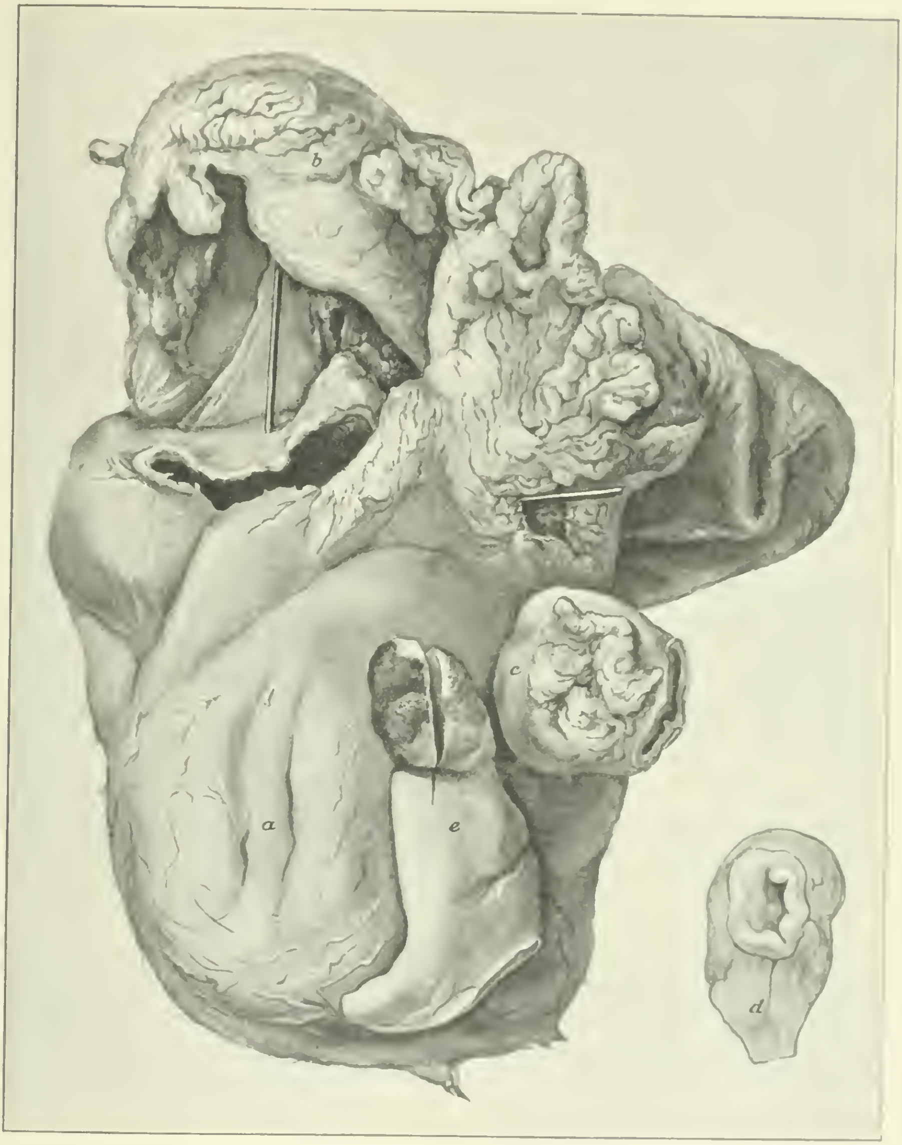
THE specimen exhibited consisted of a suppurating ovarian tumour with œdematous omentum which was adherent over an area showing a perforation through which the purulent contents had leaked, causing sloughing of the under surface of the covering omentum. An inch from this perforation was a second smaller perforation which had communicated with an adherent coil of small intestine, the mesentery of which was 1·5 cm. thick.

The cyst was as large as a football, but had shrunk much owing to its preservation in spirit. It was multilocular, but mainly consisted of one large cyst, and was covered with adhesions; a secondary cyst was examined under the microscope and was found to be lined with a single layer of short columnar epithelium. The vermiform appendix had its wall and mesentery thickened by inflammation and showed a blood-stained inflammatory mass adherent to its tip. The excised knuckle of small intestine was 2·5 cm. long; its wall and mesentery were much thickened, and on its peritoneal surface was a deep, sloughy ulcer which had perforated into the intestine by a small hole covered with lymph on the mucous surface (*see* fig.).

The patient from whom the specimen was removed (L. L., a widow, aged 42) was admitted to University College Hospital on February 15, 1909, complaining of a lump in the abdomen which had been present for ten years, pains in the chest for three weeks, and flooding nine days before admission. Menstruation began at the age of 17, was regular every four weeks, and lasted for three days. Five years ago it became profuse, but for the last four years she had seen nothing till the flooding nine days ago. She had had an attack of pleurisy fifteen years ago, and had had "fever" at King's College Hospital at the age of 11. Married ten years ago, she had never been pregnant.

On admission the patient was extremely ill, having the hebetudinous appearance of one suffering from typhoid fever; the complexion was sallow, the temperature was 99·6° F., the pulse 96, small. There was a smooth, fluctuating abdominal tumour rising out of the pelvis to a height of 1½ in. above the umbilicus, movable laterally, and not tender. Some large veins were visible in the abdominal wall on the right side; there was no free fluid in the abdomen. There was no swelling in the pelvis. The cervix was small, atrophied, and situated high up in the middle of the pelvis. The body of the uterus could not be felt, but the cervix moved with the abdominal tumour. The breath-sounds were weak; a few scattered rhonchi, but no crepitations, could be heard.

The case was diagnosed as a suppurating ovarian tumour; but, as the patient looked as if she had typhoid fever, Widal's reaction was tried, with a negative result. On February 17 the temperature reached 102·6° F., and was as high as 102° F. nearly every day for the next week. As no signs of pneumonia could be detected, and it was clear that the condition of the patient was not improving, it was decided to remove the tumour. On February 27 a long incision was made slightly to the left of the middle line from the pubes to 2 in. above the umbilicus, as it was desirable, if possible, to remove the suppurating tumour intact. A cystic tumour covered with adherent œdematous omentum was exposed, and without much difficulty was raised out of the abdomen. An attempt was made to separate the omentum from the tumour; but, on doing this, a large quantity of putrid, dark pus escaped from the cyst, which had been perforated by the suppuration, causing a sloughy condition in the omentum which covered the perforation and adhered around it. After the stinking pus, in quantity several pints, had escaped, the cyst was washed out and packed with gauze. The adherent omentum was then tied off and divided a short distance from the tumour. Several coils of small intestine were now seen adhering to the cyst; these were separated without difficulty; but it was found that one coil, which was more firmly adherent than the others, had communicated through a small hole with the cyst. The vermiform appendix was also found to be thickened and adherent to the cyst, but its lumen was not distended and did not communicate with the cyst. The perforated coil of gut was temporarily wrapped in gauze, while the ovarian cyst was removed after ligating its pedicle with silk. Then the vermiform appendix was removed, and, finally, as it was found that the hole in the intestine could not be closed



Suppurating ovarian tumour, perforated into small intestine and omentum. *a*, suppurating ovarian cyst with two perforations; the bigger has been enlarged at operation. *b*, œdematous omentum, raised up from perforation by glass rod; to the right of the rod the sloughing surface of the covering omentum is seen. *c*, gut showing small perforation at bottom of ulcerated surface, with raised edges; above it (on the cyst) is seen the rough surface to which the gut adhered; a rod is passed into the perforation in the cyst. *d*, cut surface of additional piece of gut, removed to facilitate anastomosis, showing œdema of wall and mesentery. *e*, vermiform appendix, with thickened mesentery and inflammatory mass at end.

by sutures, the knuckle of gut with its œdematous mesentery was cut away and an end-to-end anastomosis performed with silk sutures. To do this conveniently it was necessary to excise a further portion of the intestine, owing to the œdema of the mesentery. A drainage-tube was inserted through the lower angle of the wound, the upper part of the wound was closed with through-and-through stitches of silk-worm gut and buried fascial stitches of silk. The operation lasted one hundred and five minutes. After the operation the temperature, which had previously risen almost every day to 102° F. and on one day to 102·6° F., fell to normal and remained normal for three days, then gradually rose to 100° F. on the seventh day, fell to 99° F. on the thirteenth and fourteenth days, and remained practically normal after the twenty-first day. The wound healed by first intention, except at the site of the drainage tube, where it did not completely close until April 14. The patient left the hospital quite well on April 23, and was seen in good health several months later.

Perforation of *large* intestine by suppurating tumours was not very rare, and could usually be successfully treated by suturing or gauze packing. Perforation of small intestine by suppurating tumours he believed to be an extremely rare occurrence, and, owing to the extensive ulceration of the peritoneal surface, the stiffening and softening of the inflamed gut and the risks of intestinal obstruction, the above-mentioned methods of treatment would be found unsuitable. The immediate improvement and simple recovery in this extremely grave case led him to hope that his colleagues who met with a similar case would try the same treatment, which completely removed the infected tissues—tumour, omentum, appendix, and gut.

The PRESIDENT congratulated Dr. Herbert Spencer on the very successful issue of a brilliantly executed piece of difficult surgery. Such a case showed that the modern gynæcologist could not limit the sphere of his operations to the pelvic organs, for frequently the complications present demanded the technical skill in manipulation of the expert abdominal operator.

Obstetrical and Gynæcological Section.

February 2, 1911.

Dr. H. MACNAUGHTON-JONES, President of the Section, in the Chair.

The Indications for, and Technique of, Cæsarean Section and its Alternatives, in Women with Contracted Pelves, who have been long in Labour and exposed to Septic Infection.

An Address Introductory to a Discussion on the Subject.

By AMAND ROUTH, M.D.

I AM glad to acquiesce in the request of the Council to open this discussion, for after my visit to the Congress at St. Petersburg I realize fully the difficulty of the question. Obstetricians in every nation of Europe are seeking to find an operation which will save the lives of infected cases. The ideal operation would be one which is free from the fœtal mortality of craniotomy, from the maternal mortality of classical Cæsarean section, from the morbidity of pelviotomy and extraperitoneal Cæsarean section, and from the sterilization which accompanies all forms of hysterectomy.

Let me first draw attention to Table I (*see* p. 162), which shows the enormous difference between the mortality of the "clean" and the "suspected" cases.

It will be seen that the cases in Groups A and B had a death-rate of only 2·9 per cent., whereas in Groups C and D the mortality was 17·3 per cent. In Group D the death-rate was as high as 34·3 per cent. (22 deaths in 64 cases).

Realizing the risk of classical Cæsarean section in "suspect" and septic cases, obstetricians have been continually inventing alternative operations. For a time Porro's operation (1876) held the field, though it was not originally employed especially for septic cases, but as a routine operation. Its success in reducing the death-rate from 88 to 55 per cent. was due first to the fact that there were faults in the operation of Cæsarean section as then performed, and secondly to the fact that the large majority of cases of labour with contracted pelvis at that date had previously had attempts of all sorts made to deliver them so as to avoid the enormous mortality of Cæsarean section, and as a result they were in most cases already septic.

TABLE I.—SHOWING THE MORTALITY FOR CÆSAREAN OPERATIONS FOR CONTRACTED PELVIS WHERE DETAILS ARE GIVEN AS TO POSSIBLE INFECTIVITY (1891 TO 1910).

Condition			Cases		Maternal deaths		Percentage mortality	
"Clean"	A	"Not in labour"	245	469	9	14	3·6	2·9
	B*	In labour, membranes intact ...	224		5		2·2	
"Suspect"	C	In labour, membranes ruptured ...	166	230	18	40	10·8	17·3
	D	Frequent examinations or attempts at delivery	64		22		34·3	

* B, or "onset of labour," or "in labour but in hospital some days."

To prevent craniotomy having to be done the Sigaultian operation of symphysiotomy, first performed in 1777, was revived in 1866, but its post-operative morbidity has prevented it being largely adopted in this country. At a later date pubiotomy was strongly recommended, even in septic cases, and to avoid the obvious risk of opening up the cellular tissue and dividing the bone, subcutaneous methods were adopted.

It soon became evident to most operators that pubiotomy or symphysiotomy, however suitable for moderate degrees of pelvic contraction when the patient was aseptic, became dangerous to life, or was accompanied by enormous morbidity during convalescence, if the patients were septic. To avoid, therefore, the alternatives of craniotomy or Cæsarean hysterectomy, foreign obstetricians have endeavoured, during the last three or four years, to extract the child without the peritoneal cavity being involved in the operation. In this so-called extraperitoneal Cæsarean section some operators endeavour to strip off the peritoneum from the lower uterine segment without opening the peritoneal cavity—

true extraperitoneal Cæsarean section. Others, realizing that that is frequently impossible, or may involve injury to the peritoneum, prefer to open the peritoneal cavity and then to incise the visceral peritoneum where it is most loosely applied, usually at the point of overlapping of the bladder and uterus, and to strip off some of the peritoneum and stitch it temporarily to the parietal peritoneum at the margins of the abdominal wound. This is usually called the transperitoneal Cæsarean section.

Sellheim, believing that all the extra- and transperitoneal cervical Cæsarean sections are dangerous in infected cases, has more recently advocated the formation of a utero-abdominal fistula in cases of undoubted infection, details of which I need not describe here. This operation may help to save lives in septic cases where primary union of the uterine wound fails, and in a case of my own (1,045)[1] in 1909, I have no doubt that a utero-abdominal fistula, which accidentally formed, was an important factor in saving the patient's life. In that case many attempts had been previously made to deliver by forceps, but by such experienced accoucheurs that I hoped infection was absent. When the child was extracted by Cæsarean section I found the head abraded, and in a few days septic sloughs and abscesses appeared both on the child's head and over the abdominal wound, and a utero-abdominal fistula developed. Three months afterwards appendicitis supervened, and when the appendix was removed by Mr. Stanley Boyd the fistula was closed.[2]

All extraperitoneal operations are difficult, and there is no finality in the technique, which has to be varied according to the difficulties experienced. Though these operations were originally intended for septic cases the opinion is steadily gaining ground, even on the Continent, that in such cases it is a dangerous proceeding. It is obvious that it must be so, and the caution of the British obstetrician in refusing to recognize the operation as at present performed is abundantly justified. The disadvantages of the operation in septic cases may be enumerated as follows:—

(1) The uterus, with its large placental site, its decidual surface, and its incised wall, is retained as a channel for infection.

(2) The peritoneum is bruised, and when stripped off from its subserous attachments necessarily loses to a large extent its blood supply, and is liable to become less resistant to septic processes.

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[1] References to cases are to a Report on Cæsarean Section published in the *Journ. of Obstet. and Gynæc. Brit. Empire* (January), 1911, xix, pp. 1-233.

[2] This patient emigrated to Canada, a hundred miles away from a town, and writes that she is again pregnant.

(3) The under surface of the peritoneum, thus to some extent devitalized and injured, is during the operation constantly in contact with any septic germs present *in utero*. As a result, a large percentage of the fatal cases die of peritonitis.

(4) A large area of connective tissue is opened up and exposed to infection.

(5) The bladder, if stripped off, may remain displaced, and as a result of adjacent cellulitis may become inflamed and adherent in an unsatisfactory position.

(6) The resulting cicatrix, according to Bumm, is unfavourable, for it distorts the neck of the uterus forwards and causes retroflexion of the body, making a repeated operation difficult.

At the recent International Congress of Obstetricians at St. Petersburg there were three *rapporteurs* besides myself: Bumm, of Berlin, Brandt, of Christiania, and Pestalozza, of Rome. All agreed that the present methods of dealing with "suspect cases" and "infected cases" were unsatisfactory, and I will briefly quote their opinion, together with the opinions of those who joined in the discussion. It will be observed how doubtful or even hopeless, and also how divergent, many of their opinions are.

Bumm says: "For feverish women with septic genital canal there is not yet found an absolutely secure method of Cæsarean operation." I refer to his treatment later.

Brandt, of Christiania, alludes to the absence of any operation suited to these septic cases, and refers to Frank's extraperitoneal Cæsarean section, and its numerous variations, as attempts in that direction, but goes on to say that no one would perform the extraperitoneal variety instead of classical Cæsarean section in infected cases, because the mortality is very high; and that, even where the case is "suspect," the number of those in favour of extraperitoneal Cæsarean section is becoming less and less. He spoke favourably of supravaginal hysterectomy, with extraperitoneal treatment of the stump in these septic cases, and in this opinion he is upheld by Schauta. Brandt disapproves of symphysiotomy, but has successfully performed hebosteotomy, not only in "suspect cases" but in cases definitely infected. In these latter cases he thinks a chance of survival should, in otherwise suitable cases, be given to the child by hebosteotomy. As regards craniotomy, he says that in *primiparæ*, when infection is undoubtedly present, craniotomy is the best and surest treatment, for by it one life already compromised is lost; whereas, by all other methods, two lives would probably be sacrificed.

Pestalozza, of Rome, representing Italy, considers that extraperitoneal Cæsarean section is safer than classical Cæsarean section in "suspect cases." In definitely infected cases he agrees that neither classical nor extraperitoneal Cæsarean section is admissible, but, wherever possible, embryotomy should be performed. If the conjugata vera is as small as 5 cm. (2 in.), the method selected should either be "l'opération de Porro," the uterus being opened after eventration, or Sellheim's utero-abdominal fistula. He urges all to seek new methods of dealing with these septic cases.

In the discussion which followed, Opitz, of Düsseldorf, admitted the risk of extraperitoneal section in septic cases, but thinks some of its dangers can be lessened by vaginal drainage.

Sellheim, of Tübingen, considers that in definitely infected cases all operative deliveries including Cæsarean section and its modifications, and all operative enlargement of the pelvis, are dangerous to the health and life of the mother, and advises embryotomy. He considers that his utero-abdominal fistula operation should only be used in infected cases when a child is keenly desired, and in cases where the mother is willing to undergo the extra risk of the operation and there seems no chance of a future pregnancy.

Kholmogoroff, of Moscow, considers extraperitoneal Cæsarean section suitable for "suspect" cases, but Cova, of Rome, considers hebosteotomy less dangerous.

The following are a few of the opinions regarding treatment of my British and American colleagues:—

Hastings Tweedy read a paper at the Royal Society of Medicine[1] last year, and he therein stated that he considered perforation of a living child unjustifiable, and that in a few years such an operation would be considered criminal. He considers that, if the child is alive, the possibility of sepsis should not prevent the operation of extraperitoneal Cæsarean section, and he advocates an operation very similar to Bumm's and Sellheim's transperitoneal modification of Frank's original operation.

Murdoch Cameron is now as strongly against craniotomy being performed as he was in 1891, when he said: "You have no right to sacrifice a child, however unequal its life may be in some cases to that of the mother."

Eden, in the last (1908) edition of his "Manual of Midwifery," says: "It is a wise precaution to remove the uterus whenever there is reason to believe that the uterine cavity has become infected—e.g., when there is offensive liquor amnii" (p. 520). Amongst the indications for craniotomy with a living child he does not include septic cases.

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[1] *Proceedings*, 1910, iii, p. 116.

Galabin and Blacker, in "Practice of Midwifery" (1910), state that "the extraperitoneal Cæsarean section may be performed in cases where the condition of the uterine contents is doubtful or where they are certainly septic." They consider panhysterectomy is only called for "if the cervix is much damaged or infected by sepsis." Craniotomy is not stated to be indicated for septic cases.

Munro Kerr ("Operative Midwifery," 1908, p. 404) says: "In recent years I have made it a rule never to perform Cæsarean section in cases which have been interfered with prior to their coming under my care unless the pelvic deformity is so extreme as to render craniotomy impossible or more dangerous to the mother. I am compelled, therefore, not infrequently to perforate a living child. This I do with extreme regret, but I am convinced I save more mothers, and probably indirectly more children, by so doing." Later on he speaks of the advisability of performing supravaginal amputation of the uterus after Cæsarean section in infected cases. If panhysterectomy is done for septic cases he prefers to leave a small opening into the vagina and insert a gauze drain from above.

Jardine ("Clinical Obstetrics," 1910) says that "where careless examinations have been made before the patient comes under observation, extirpation of the uterus should be performed if Cæsarean section is absolutely necessary, but if a craniotomy can be done it should be performed even if the child be alive."

Whitridge Williams ("Obstetrics," 1910, p. 445) says if the mother is infected, craniotomy is the operation of choice, and he goes on to say that, if the patient has been subjected to repeated examinations during labour by those whose technique is questionable, even though no signs of infection are apparent, Cæsarean section, if done at all, should be followed by panhysterectomy.

I wish at this point to discuss the subject of infection from the bacteriological point of view, for much advantage will be gained if treatment of these septic cases can be safely varied according to the type of the germs found to be present. These germs may roughly be divided into pathogenic and putrefactive. Both of these types can *infect* the patient, at all events in one sense of the word, though the putrefactive micro-organisms can only live and multiply in dead tissue, and are never found in the blood or living tissues of the body except when the patient is on the point of death.

Pathogenic germs such as streptococci not only cause local inflammatory disturbances producing in the process dangerous absorbable toxins, but multiply freely in the blood. They may, however, at first act solely as saprophytes. If they infect the uterine wound in Cæsarean section, they will prevent primary union, and may infect the peritoneum either at the time of the operation or in a few hours by infection of the sutures, or by gaping of the uterine wound.

It is probable that putrefactive germs, if present in large numbers, will do the same as regards preventing primary healing of the wound; for Arnold Lea states that cultures of anaerobic putrefactive organisms injected hypodermically into animals produce localized suppuration often followed by extensive gangrene.

It must also be remembered that some pathogenic germs—such as streptococci—may act as pure saprophytes. It is clear, however, that the living tissues can successfully deal with a small dose of putrefactive organisms, for both the peritoneum and the abdominal wound in ordinary laparotomies must become contaminated with the germs constantly present in the atmosphere, and yet the evidence of infection is usually entirely absent.

Arnold Lea, Foulerton, and Bonney prove that the most serious cases of puerperal infection are due to the presence of streptococci,[1] or streptococci and *Bacillus coli* combined, and that pneumococci and *Staphylococcus pyogenes aureus* are capable of causing fatal infection as well as gonococci. *Staphylococcus pyogenes albus* and *Bacillus coli* are less virulent germs.

Foulerton and Bonney examined the uterine contents bacteriologically in 54 cases of puerperal fever, and found that out of 14 cases which ended in death 10 were cases of streptococcic infection; in 26 fever cases which ended in recovery 18 had streptococci in the uterus, whilst none of the 14 cases of "slight" fever examined by them had a streptococcic infection. *Micrococci pneumoniæ* caused three out of the four other deaths in their series. They failed to find anaerobic germs, which would include ordinary saprophytic organisms, in 53 out of these 54 cases of puerperal fever. On the other hand, Krönig found them in 32 out of 179 cases (18 per cent.), and Whitridge Williams in 8 out of 151 cases (15 per cent.)

Several foreign authorities who were present at the recent International Congress drew attention to the different significance of infection by putrefactive as compared with pathogenic germs. Thus, whilst discussing Cæsarean section in septic cases, Bumm says: "The measure of the danger depends upon the kind of infection. The decomposition of the liquor amnii by ascending putrefactive germs is less dangerous than the presence of streptococci, the ample appearance of which in the liquor amnii leaves no doubt as to the existence of a virulent infection." Bumm says that the presence of putrefactive germs prevents the primary union of the muscle wound of the uterus and causes fatal peritonitis. He says: "If the amnion has been ruptured for some time and there is even only a slight elevation of temperature classic Cæsarean section is fatal," and adds: "In performing supra-symphysary Cæsarean section, if one succeeds in finishing the obstetric act extraperitoneally, one has the possibility, by ample draining of the cellular tissue wounds, of avoiding the danger of a general infection in all those cases where one has to deal with a simple putrefactive decomposition of the liquor amnii. If, however, the uterus at the time of the operation already contains streptococci a fatal result may follow by progressive phlegmon of the pelvis, septic thrombo-phlebitis, or by the migration of the germs through the thin layer of peritoneum covering the utero-vesical excavation."

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[1] *Trans. Obstet. Soc. Lond.* (1905), 1906, xlvii, p. 11.

In reply to a letter from me to Professor Bumm, asking for further information, Dr. Edward Martin replied on November 9 last:—

> On behalf of Professor Bumm I answer the question of your letter of October 30. In every case of contracted pelvis *without* fever, we have to perform the delivery by suprapubic section. When the waters are passing away we make a bacteriological examination by microscope. In all cases of bacteria of any kind we object to the section, and make the perforation of the child. In all suspicious cases, when the patient has been examined outside of the Klinik at several times, or *when she has elevated temperature, when germs are not found* we make it a rule to perform suprapubic section only, but in such cases we make use of drainage. *When bacteria are found in fever cases*, we perforate the child.

Veit says the danger of Cæsarean section consists in the infection by virulent germs or in the penetration of germs of putrefaction from the genital canal into the abdominal cavity with the blood or liquor amnii. With virulent germs he considers all varieties of Cæsarean section inadmissible. The penetration of germs of putrefaction into the abdominal cavity, he considers, can be prevented by various methods of extraperitoneal Cæsarean section, as proposed by Döderlein and Latzko, and his own modification of Sellheim's operation, and also to a large extent by eventration of the uterus before incision, with careful packing round the uterus, and he prefers in such cases Fritsch's transverse fundal incision.

Singer, of Paris, writing in 1909 ("Cicatrices Césariennes Abdominales Classiques"), says that "the utero-vaginal and cervical secretions should be examined microscopically, and if the result of examination is positive conservative Cæsarean section should not be performed."

Gonococcal infection is of a somewhat different nature from the other pathogenic bacteria, inasmuch as gonococci rarely, if ever, multiply in the blood, at all events in the acute stages, and they cause mischief by infecting the mucous lining and glands of the genital tract and uterine wound. Such infection would probably require a more radical treatment than any but the most virulent streptococcic infection.

Hecking reports a case of conservative Cæsarean section performed in a woman who had gonorrhœa. Suppuration occurred in the abdominal wound and both gonococci and streptococci were found to be present. Cases 441 and (?) 496 in the collected tables are cases in point.[1] Léopold is quoted by Singer as saying that the presence of gonorrhœa constitutes a contra-indication to classical Cæsarean section, and with this we are probably all in agreement.

I cannot find any observations or suggestions made by British obstetricians upon the bacteriological examination of cases of contracted pelvis prior to operation. Numerous investigations have, of course, been made regarding the presence or absence of germs *in utero* during the normal puerperium and very different results have been obtained; the bacteriology of the infected puerperal uterus after labour has also been investigated with equally varying results. The bacteriology of the vagina during pregnancy has also been reported on, but I can find very few references to the bacteriology of the vagina during labour, and none to the bacteriology of the amniotic cavity during labour after the membranes have ruptured.

We must, I think, all agree that when a parturient woman is said to have been exposed to infection her danger must largely depend upon the variety and virulence of the infecting organism, as well as upon the length of time which has elapsed since the infection and the presence of abrasions as absorbent areas. It will not often happen in "suspect cases" that infection of the liquor amnii has occurred many hours previously, consequently germs are rarely, if ever, found in the blood.

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[1] *Journ. of Obstet. and Gynæc. Brit. Empire*, 1911, xix, pp. 1-233.

If the membranes are ruptured and the amniotic cavity has been infected the micro-organisms will rapidly multiply there, for the liquor amnii is a suitable culture medium. In a recently infected case the germs would be few. There may be both putrefactive and pathogenic germs present, or several varieties of the latter, with or without any of the former. With our present imperfect knowledge of the interaction between different kinds of bacteria and their power of increasing or reducing each other's activity and virulence, it is evident that in the presence of both virulent and putrefactive organisms one would have to treat the case from the point of view of the most virulent germs. Thus for practical purposes we need only discuss this bacteriological part of the question from the standpoint of whether the patient's liquor amnii is sterile or infected ; and, if infected, whether the germs are pathogenic or only putrefactive. It is also fairly certain that it is not always necessary to obtain swabs from the cavity of the amnion, for if a case has been frequently examined or forceps have been used and potentially virulent germs are found in the cervix or upper vagina, it is manifest that they must have been carried up into the uterus and that the liquor amnii is infected. Apart from the introduction of an examining finger or instrument into the uterus, it is probable that the cervical plug of mucus is usually able, owing to the presence of phagocytes, as suggested by Arnold Lea, to prevent vaginal streptococci, &c., from infecting the amniotic cavity.

It is evident that if the technique of these Cæsarean operations is to be varied according to the presence, absence, or variety of the infecting germ, we must have a report presented to us within an hour or so of the "suspect case" being admitted to hospital. Probably the greatest difficulty in the bacteriological investigation is to be able to recognize the dividing line between saprophytic and pathogenic germs, for, as Arnold Lea says, the same organism may, under different conditions, entirely change its character. Thus the *Streptococcus pyogenes* may exist as a pure saprophyte in the genital tract, whilst some anaerobic putrefactive bacteria may become highly infective. It is also apparently true that if the uterus of a puerperal woman be invaded by streptococci acting at first solely as pure saprophytes and temporarily only causing a toxæmia (sapræmia), conditions may arise in which the streptococci may become infective and true septicæmia result. If this be so, even the examination of cultures, which of course cannot be waited for as the patients are in labour, would not aid us in our choice of technique, but opinions vary greatly on this point. If streptococci were found we must assume they are virulent, and act accordingly. Subject to these limitations, however, I believe that reliable reports from the microscopic examination of these swabs can be usually given us, and that the following remarks on the subject by Mr. Leathem can be relied upon.

Mr. A. N. Leathem, Bacteriologist at Charing Cross Hospital, states "that films could be made and stained from uterine swabbings, and, if the organisms were present in considerable numbers, a reliable report as to the presence of streptococci, staphylococci, or pneumococci could be given within half an hour. If bacilli were present it would be more difficult to decide as to the presence of *Bacillus coli*, although in some cases it would probably be possible to say that the bacilli present in the films were all saprophytic."

I also put the case to Sir Almroth Wright, who writes:—

> "It seems to me that in such a case as you refer to it would be well worth while making a bacteriological examination, for it might be possible, in cases where there was a considerable amount of infection, to detect and identify the microbes by mere microscopic examination. In the majority of cases it would, of course, be impossible to detect or identify the microbes without awaiting the results of cultures."

The presence of gonococci in the acute stages is able to be accurately determined. Mr. Leathem says: "If a swabbing is taken from a purulent discharge and contains a considerable number of gonococci, then from a film made from this swabbing one could give an immediate, reliable report that gonococci were present. If the swabbing is from a purulent discharge containing one or two kinds of organisms but no gonococci, then in most cases a report could be given that gonococci were not present with almost as great certainty, and could be given at once. But in old cases where the discharge is mucoid, rather than purulent, and is crowded with saprophytic organisms, the detection of gonococci, either in films or cultures, is extremely difficult."

The next point was to find out by a series of tests whether a report thus obtained from the examination of films was confirmed by a further examination of cultures obtained from these same swabbings—whether, in fact, the report from the film examination was sufficiently reliable to be acted upon. For this purpose Mr. Leathem,[1] at Charing Cross Hospital, has examined and reported upon swabbings obtained by my resident obstetrical officers, Mr. Salmon and Mr. Cock, first by examination of stained films and subsequently by examination of the cultures from the same swabs. Dr. Banister and Dr. Maxwell have also kindly obtained similar information for me. As my object was mainly to discover whether the film results were confirmed by the culture tests, I have taken swabbings from the cervix and uterine body from all sorts of cases. As a result of such investigations as I have been enabled to make, I believe that these rapidly obtained film results, when reported on by those who are experts, are sufficiently reliable in many cases largely to justify us in adopting a particular type of operation, according to the variety of micro-organisms found present. Unfortunately we cannot, apparently, always get such results, for both Dr. Banister and Dr. Maxwell tell me that in a few cases they had films prepared and failed to find streptococci, and yet cultures showed their presence. This, if not very exceptional, would prove a seriously weak point, for as a rule it is not difficult to find streptococci in the films. Presumably the germs were few and stained badly. It is probable that in the future greater reliability will be able to be placed upon these film reports—perhaps by more perfect methods of differential staining.

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[1] See Appendix at end of paper.

INDICATIONS FOR TREATMENT.

Perhaps the best way to consider the treatment would be to assume certain clinical groups of cases and to discuss their appropriate surgical indications—How best to save the woman's life. The cases we are considering are "suspect cases" and may roughly be divided into five classes :—

(1) *Cases which have been exposed to infection but where the membranes are not ruptured.* If germs had been carried into the vagina during previous examinations they could almost certainly be destroyed or rendered harmless by copious vaginal douches of 1 in 1,000 sublimate or biniodide of mercury solutions.

(2) *Second Type of Case. In labour some hours, membranes ruptured, vaginal examination made*, no feverishness, no offensive discharge.

This case is necessarily "suspect," but if a bacteriological report was forthcoming that the liquor amnii was sterile a classical Cæsarean section could be safely done, though some would prefer to eventrate the uterus before incision, as the woman has been previously examined. If germs were found it would be dealt with as Type 3, 4, or 5, according to variety of germs.

(3) *Third Type of Case.—In labour some hours, membranes ruptured, examination made or attempts at delivery, some offensiveness of discharge, possibly some feverishness.* We will assume that saprophytic organisms are found in the liquor amnii, but no pathogenic organism. Primary union of the uterine wound might fail in these cases, and unless an utero-abdominal fistula formed fatal peritonitis would follow.

In the table of 1,282 cases collected by me and published in the January number of the *Journal of Obstetrics and Gynæcology of the British Empire*, there are apparently many cases under this group, though a bacteriological investigation was not made, I believe, in any of them. I will allude to three such cases, though it is quite possible they were cases of mixed infection.

Thus Case 341 was treated by Cæsarean section and died on the fourth day. The membranes had been ruptured forty-six hours, and there was a stinking vaginal discharge. Mother's pulse 160. Child alive, but died in sixteen hours.

Case 811 was also treated by Cæsarean section and died. She had had attempts made to deliver by forceps, and when the uterus was opened it was found to contain air which was very fœtid. The child lived.

Case 841 was an almost exactly similar case, the woman dying of sepsis on the third day. The child lived as in the last case, in spite of the fœtid smell in the amniotic cavity.

As I have already indicated, extraperitoneal Cæsarean section and all forms of pelviotomy are better avoided for septic cases, and I have shown by Table I that classical Cæsarean section has a death-rate of from 10·8 to 34·3 per cent. There remains, therefore, only craniotomy or some variety of Cæsarean hysterectomy; but I wish first to speak of an alternative prophylactic measure adopted by Dr. R. D. Maxwell, of Queen Charlotte's Hospital (Case 1,275),[1] in doubtful cases of infection where the membranes are ruptured, performing Cæsarean section. A soft pewter or other pliable tube is passed up *per vaginam* to the fundus uteri, and the amniotic cavity, and incidentally the fœtus, is freely irrigated with normal saline solution at 100° F. Possibly a saturated boric solution would be more useful if micro-organisms were present. The method is, of course, available in all degrees of pelvic contraction. It might also be usefully employed before craniotomy to dilute any suspected infection in the amniotic cavity and so reduce the risk of infection of the tissues if they were abraded during the operation.

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[1] Case 1,275. Contracted pelvis. In labour forty hours. Membranes ruptured. Forceps previously applied. Pulse 110. Marked forceps grip on head. Mild maternal pyrexia for some days after operation. Child lived.

It is possible that this method might have been successful in Cases 341,[1] 811,[2] (?) 841, or in 946,[3] 1,045,[4] and many others. These last two and such cases as 1,090,[5] 1,121,[6] show, however, that even those which appear most unfavourable may recover without such aid. Infecting germs in these last four cases, if present at all, were probably saprophytic, as the patients survived.

Whether it is possible to irrigate the amniotic cavity so thoroughly that all putrefactive organisms can be washed away or rendered harmless is only a question which can be settled by experience.

If most of the liquor amnii had drained away, and especially if the uterine muscle were contracted down on the child, such irrigation could only be very partial. The difficulty, too, of dislodging germs from fœtal apertures and irregularities, or those luxuriating in a vernix caseosa, is evident. Still, it is an additional method at our disposal, and may prove of great value. In any case it must dilute the poison, and so to speak "lessen the dose."

If Maxwell's method does *not* prove reliable, and there is no constitutional or bacteriological evidence of infection by virulent pathogenic organisms, it is probable that craniotomy would be the best treatment, for at this stage the infection is recent, the germs therefore few, and the placental site and decidual membrane is protected by the membranes from infection. If craniotomy were decided upon it might be possible to irrigate the amniotic cavity before and after the birth of the child, and after the third stage the decidual cavity can be also freely irrigated with iodized water and the risk of infection thus greatly minimized.

If, on the other hand, Maxwell's method or some modification of it should prove serviceable, it would largely prevent craniotomy with living children in those "suspect cases" where it could be shown bacteriologically that there was no virulent infection present.

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[1] [2] Cases 341, 811, already alluded to (*see* p. 173).

[3] Case 946. Patient of Dr. H. Williamson. Forceps frequently tried. Finally head perforated. Vagina and bladder torn. Patient apparently dying. Pulse 130. Classical Cæsarean section. Patient survived but there was prolonged suppuration and formation of temporary abdomino-vesical fistula.

[4] Case 1,045 (*see* p. 163). Recovered, but with utero-abdominal fistula.

[5] Case 1,090 (Dr. Hewetson). In labour forty-eight hours. Membranes ruptured thirty-six hours. Repeated attempts at forceps delivery. Liquor amnii found. Cæsarean section performed. Stitch abscess, otherwise good recovery.

[6] Case 1,121 (Dr. Victor Bonney). Head perforated and most of vault previously removed. Vulva black and excoriated. Bladder full and catheter could not be passed owing to impaction of head. Cæsarean section performed. Convalescence excellent, though sloughs formed on cervix and vagina owing to injuries before admission.

(4) *Fourth Type of Case.—In labour many hours, membranes ruptured, frequent examinations made or attempts at delivery. Pathogenic germs of potential virulence found. Constitutional evidence of infection.* In this class of case it is probably correct to assume that Maxwell's method would be useless, and that such rapidly multiplying and invading pathogenic germs as streptococci, &c., could not be successfully dealt with, even in a case very recently infected, by any irrigating fluid which was not so powerfully antiseptic as to cause serious injury to the fœtus.

Craniotomy would no doubt be preferable to such futile attempts to destroy the infecting germs, but it must be remembered that abrasions of the cervix and vagina during the extraction of the mutilated fœtus after craniotomy are almost inevitable, and if virulent germs such as streptococci were present in the amniotic cavity the maternal tissues would become infected and septicæmia would occur. Probably, therefore, if such germs were proved to be present, a radical operation would be preferred. In other words, where there are constitutional evidences of sepsis, and virulent germs are found, some form of Cæsarean hysterectomy is indicated.

(5) I would also mention a fifth group—cases where gonococci are found microscopically or when gonorrhœa is known to exist.

I wish, in conclusion, to discuss the relative advantages of the two varieties of subtotal hysterectomy and abdominal panhysterectomy in virulent infection.

(A) First, Supravaginal Hysterectomy with Intraperitoneal Treatment of the Stump.

Here let me draw your attention to Table II (*see* p. 176), taken from my recent report upon Cæsarean section.

This Table divides the 230 cases "suspect" in Groups C and D of Table I into two other groups according to their treatment by Cæsarean section only, or by supravaginal hysterectomy with intraperitoneal treatment of the stump. It will be seen that in Group C where the membranes were already ruptured the mortality of Cæsarean section alone was 11·4 per cent., and in Group D where attempts at delivery or frequent examination had been made the mortality of Cæsarean section alone was 37·9 per cent. In these two groups, C and D, 14 cases were treated by supravaginal hysterectomy with intraperitoneal treatment of the stump, and of these all the mothers survived and 11 of the children. Ten of these 14 "suspect" or infected cases which recovered were operated upon in Glasgow by Cameron (1), Jardine (6), and Munro Kerr (3), the others by Berkeley, Brewis, and Purslow. It is probable that several of these cases were infected by pathogenic organisms, and if so it makes the results of the radical operations more striking.

TABLE II.—SHOWING METHOD OF TREATMENT * OF THE 230 "SUSPECT CASES" IN TABLE I WHICH MAY HAVE BEEN EXPOSED TO INFECTION (1891 TO 1910).

	Cases	Treated by Cæsarean section only	Deaths	Treated by supravaginal hysterectomy	Deaths
C In labour, membranes ruptured ...	166	158	18 (11·4 per cent.)	8	0
D Frequent examination or attempt at delivery	64	58	22 (37·9 per cent.)	6	0
Total	230	216	40 (18·5 per cent.)	14 †	0

* The early cases dealt with by supravaginal hysterectomy, where extraperitoneal treatment of the stump was the technique adopted, cannot be here tabulated owing to insufficient details regarding the "rupture of the membranes."

† Cases 336, 358, 370, 400, 404, 405, 432, 494, 568, 713, 736, 751, 843, 1,255.

It is not, however, safe to assume that supravaginal hysterectomy with intraperitoneal treatment of the stump is an operation without risk, for I find that in the 1,058 cases of contracted pelvis collected by me 53 altogether were treated by this operation after Cæsarean section with seven deaths (or 13·8 per cent.). As above stated, 14 of these were for "suspect" or "infected cases," all of which recovered, so that there were seven deaths (or 17·9 per cent.) in the remaining 39 cases. Of these 39 cases, 18 were "not in labour," and yet had two deaths (11 per cent.). Why the operators chose the more radical operation in these cases, unless as a means of sterilization, I do not know. The danger of infecting the peritoneum during extraction of the child by the preliminary Cæsarean section is discussed when speaking of panhysterectomy. If the cervix is left in these infected cases it should be rendered as harmless as possible, either by applying a strong antiseptic application such as liquor iodi or pure carbolic acid to the cervical canal from above, or, as some suggest, a strip of gauze should be passed downwards into the vagina through the cervical canal. The covering of the stump with peritoneum should be very accurately and carefully executed.

(B) Supravaginal Hysterectomy (Porro's) with Extraperitoneal Treatment of the Stump.

This method more effectively shuts off the peritoneal cavity from the infected cervix and vagina than any other variety of hysterectomy, in addition to which no ligatures are left in the peritoneal cavity. The child can be extracted when it is fairly certain that the peritoneal cavity is effectively protected, for it is possible to strip off some of the uterine peritoneum, and stitch it to the parietal peritoneum, and to encircle the supravaginal cervix lightly with the serrenœud ready for tightening before the uterine incision is made.

This variety of subtotal hysterectomy is strongly advised by Spencer in such septic cases as inoperable cancer of the cervix complicating labour. Thus, in my collected cases of inoperable cancer of the cervix complicating labour, whilst the mortality of conservative Cæsarean section alone was 33 per cent.[1] (twelve cases), in supravaginal hysterectomy with intraperitoneal treatment of the stump there was one death in five cases (20 per cent.), and with extraperitoneal treatment of the stump (three cases) there was no mortality.

Its comparative safety, too, is shown by the fact that when it was adopted as a routine treatment for all cases of contracted pelvis in 1876, it at once reduced the mortality from 88 per cent. to 50 per cent. It is also interesting to notice, as I showed in Table V in my paper published in the *Journal of Obstetrics and Gynæcology* for January, 1911, the mortality of the cases of contracted pelvis treated by Cæsarean hysterectomy with extraperitoneal treatment of the stump was 14·2 per cent., which compares favourably with the 13·2 per cent. mortality of cases treated by intraperitoneal treatment of the stump. When it is further remembered that the cases treated in this way were operated upon mainly before 1895, when many were septic before coming under notice, the comparative safety of the operation with extraperitoneal treatment of the stump is highly suggestive. This variety of subtotal hysterectomy is particularly indicated for gonococcal infection.

Case 441, which is not included in Table II owing to insufficient details regarding rupture of the membranes, was a case of gonorrhœa with abscesses of both Bartholin's glands, and Dr. Jardine, who treated it by subtotal hysterectomy with intraperitoneal treatment of the stump, considered that the patient died by the infection spreading up the cervix and invading the peritoneum. Probably he would agree that that treatment of the stump is contra-indicated in acute gonococcal infection, and that such cases should be dealt with by extraperitoneal treatment of the stump, or possibly by panhysterectomy.

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[1] All these fatal cases were, however, before 1902, being four out of the six cases recorded (Cases 2, 24, 88, 188, 221, 256). The other six cases recovered.

(C) Abdominal Panhysterectomy.

This operation is theoretically the ideal one for cases infected with potentially virulent pathogenic organisms, as it involves removal of the cervix with its infected glands. In actual practice, however, there are rather serious risks in the operation as now done.

(*a*) One is the difficulty of performing the preliminary operation of Cæsarean section without infecting the peritoneal cavity during its performance; for I suppose it is a fact that it is not possible to perform panhysterectomy so rapidly that the child would have a fair chance of survival if it were extracted after the removal of the uterus, even in Doyen's operation, where the uterine vessels are divided at a late stage.

To endeavour to prevent infection of the peritoneum during the delivery of the child, the uterus must first be eventrated and the abdominal incision then closed by temporary sutures. Other measures to shut off the peritoneal cavity, such as packing round the uterine neck, should also be taken.

A fundal incision should then be made into the uterus, using either Fritsch's transverse or Caruso's sagittal method. As soon as the child is extracted the uterine wound is accurately closed again. Panhysterectomy can then be performed, Wertheim's clamps being used to prevent infection from the vagina when it is cut across; or, to prevent the shortening of the vagina which this operation involves, the uterus can be removed by Doyen's method, the cervix having been previously closed by ligatures, as advised by Spencer.

(*b*) The risk of infection of the peritoneum from the septic vagina after the uterus is removed is more difficult to avoid. If Spencer's method of ligation of the cervix were adopted as a preliminary step in the operation, the vagina could be sterilized at the same time by swabbing freely with tincture of iodine. Effective sterilization of the vagina is difficult after the uterus is removed.

The treatment of the top of the vagina is important. Personally, I think its cut edges should be inverted a little and the inverted sides brought together by sutures, a small opening being left in the centre of the canal for a gauze drain. Munro Kerr adopts this method without, I believe, inverting the vaginal edges.

The question of how to treat the peritoneum on the pelvic floor is also important. Should it be stitched across transversely by continuous Lembert sutures, or should it be closed by Spencer's method of purse-string suture, or should it be left open to drain *per vaginam*, and, if so, should a gauze drain be used?

The success of panhysterectomy in these cases will depend upon a perfected technique in these small details.

In conclusion, I am not without hope that a rapid bacteriological examination of films prepared from the liquor amnii or cervical contents in "suspect" cases when the membranes are ruptured will enable the treatment of these cases to be put on a more scientific basis. If so, classical Cæsarean section would be performed with perfect security if the fluid were sterile; Cæsarean section, preceded by some variety or some evolution of Maxwell's intra-amniotic irrigation, and by eventration of the uterus, might be considered safe if non-pathogenic germs were found; and a radical hysterectomy would be considered desirable if pathogenic micro-organisms were discovered.

It is possible, also, that some such scientific basis may do away with the need for craniotomy with living children in well-organized obstetric clinics, but craniotomy would still be required in general practice where prompt bacteriological investigation and special technical experience might not be forthcoming.

The time may come when the advance of bacteriological science will enable the obstetrician, who has found his patient infected by a particular micro-organism, to confer a rapid protection or immunity against a general or local tissue infection, and so be able to perform classical Cæsarean section without danger. This protection or immunity may take the form of a bacteriological irrigation of the uterus and vagina, or some counteracting injection inoculation, such as has been recently proposed to prevent peritonitis after operations for appendicitis.

Meanwhile, till such a time arrive, we obstetricians must perfect our surgical technique, and stimulate our bacteriological colleagues to render their diagnosis of the variety and significance of the germs present more reliable.

Appendix to Dr. Amand Routh's Paper.

TESTS MADE BY MR. LEATHEM TO ASCERTAIN WHETHER EXAMINATION OF FILMS FROM CERVIX UTERI, &C., WAS SUFFICIENTLY RELIABLE TO BE ACTED UPON.

No. of case	Variety of case	Report on examination of films made from swabbing	Time taken for examination of films	Result of cultural examination	Time taken for cultural examination
1	Swabbing from vagina, taken some days after vaginal hysterectomy for fibrosis uteri; discharge was at the time offensive, with an absorption temperature of 101° F.; patient recovered well.	Films crowded with different varieties of bacilli, both Gram-positive and Gram-negative; Gram-positive diplococci (? staphylococci) present; pus cells present. Conclusions: Staphylococci may be present; *Bacillus coli* may be present; pus cells are present, and therefore the presence of pyogenic organisms is to be suspected.	15 mins.	Cultivation produced growths of *Bacillus coli* and *Staphylococcus aureus*, with saprophytic bacilli.	24 hours.
2	Swabbing from endocervix just before application of forceps; patient in second stage for twenty hours; frequent vaginal examinations had been made.	Films showed a large number of cocci, all Gram-negative, and a few Gram-negative bacilli; no pus cells. Conclusions: The cocci are probably non-pathogenic; their being Gram-negative shows that they are not staphylococci, streptococci, or pneumococci; they might be gonococci, but the absence of pus cells is against this; very few bacilli are present, but these are Gram-negative, and the presence of *Bacillus coli* cannot be excluded with certainty.	15 mins.	No growth of any pathogenic organism was obtained.	24 hours.
3	Swabbing from endocervix; normal labour, with delayed second stage; some vaginal examinations made.	Films showed a very small number of bacilli, all Gram-positive; no pus cells. Conclusions: Probably no pathogenic organisms present.	10 mins.	No growth of any bacillus was obtained; two agar plates were planted, and the only growth obtained consisted of four colonies of sarcina.	24 hours.
4	Swabbing from endocervix; puerperal septicæmia twelve months previously; metrorrhagia last few months; curettage; much adenoid tissue removed.	Films showed a few bacilli, mostly too small to be *Bacillus coli*, and all Gram-positive; a few Gram-positive diplococci; pus cells present (few). Conclusions: *Bacillus coli* probably not present; the Gram-positive diplococci may be staphylococci.	16 mins.	Cultivation produced Gram-positive bacilli (3 varieties, all apparently saprophytic) and a few colonies of sarcina; no staphylococci.	Planted on Friday; no visible growth on Saturday; then not examined until Monday.

Appendix (*continued*).

No. of case	Variety of case	Report on examination of films made from swabbing	Time taken for examination of films	Result of cultural examination	Time taken for cultural examination
5	Swabbing from external os before curettage for somewhat offensive leucorrhœa and metrorrhagia.	Films showed many Gram-staining cocci, mostly in pairs; one chain of 4 cocci and one of 5 cocci seen; the bacilli are both Gram-positive and Gram-negative; some of the diplococci resembled pneumococci in shape, and a film was therefore stained with capsule stain, but no capsule was seen. Conclusions: *Bacillus coli* may be present; staphylococci and streptococci may be present.	17 mins.	Cultivation produced growths of *Bacillus coli*, streptococcus, and a few colonies of *Staphylococcus aureus*.	24 hours.

NOTE.—According to the indications laid down in the paper, the following would be the treatment which the author would have adopted as a result of the above reports: Case 1, hysterectomy; Cases 2 and 3, Cæsarean section preceded by intra-amniotic irrigation and by eventration of the uterus; Case 4, a doubtful case—embryotomy or hysterectomy; let patient decide by her wishes; Case 5, hysterectomy.

Dr. Hastings Tweedy said it was hardly necessary to congratulate Dr. Routh on his splendid industry. His communication was worthy of the great school of obstetrics in London—a school of which Britishers were all so justly proud. The discussion which would follow this communication was bound to modify our views, and he trusted it would have the far-reaching result of relegating craniotomy on the living child to its place amongst the obsolete barbarities of the past. It was melancholy reading to find this procedure still advocated by some of the most skilled operators and ablest teachers at home and abroad. Could it be urged that it exercised a curative effect on the maternal sepsis, then a plausible, but in his opinion an insufficient, argument would be forthcoming to justify it; but no such suggestion was tenable, and they were forced to the conclusion that its popularity resulted from a desire to make Cæsarean section and its allied operations appear miraculously successful in the hands of these great operators.

For his part, he was sick of Cæsarean section statistics. The technique of the operation no longer differed in the hands of good operators; should a patient die, her death would not be due to the operation but to the complications of labour. For this reason he trusted Dr. Routh would pardon him for finding fault with his manner of tabulating his statistics. The high death-rate under C and D should not be attributed to Cæsarean section, for a similar death-rate would most likely have followed craniotomy. When tracheotomy had to be performed in diphtheritic cases death, should it follow, was not attributed to tracheotomy, and why then speak of the high mortality from Cæsarean section.

If streptococci were demonstrated in the uterus, and if their presence was associated with rising pulse and temperature, it seemed to him that their duty was clear, namely, the quick delivery of a living child and the total removal of the affected uterus. Such an operation could be quickly and safely accomplished, and could be made free from the danger of further contamination during its performance. Some might shrink from so radical a procedure on indications not absolutely convincing, yet amongst these would be found the advocates of craniotomy on the living child, and the sterilization of a woman after Cæsarean section.

In suspect cases where bacteria could not be demonstrated, the operation of hysterotomy offered many advantages over that of Cæsarean section. The objections to it were unpractical. Cellular tissue need not, and should not, be opened up to a degree greater than that which obtained in any ordinary abdominal section. The delivery of the child could always be made an extra-peritoneal operation. The peritoneum was not injured to a greater degree than in supra-vaginal hysterectomy, and as to displacement of the bladder, such would not occur in those who knew how to perform it properly. The operation was very easy. The incision passed through the thinned-out cervix, and extended almost down to the external os, through tissue relatively free from blood-vessels and lymphatics. Should suppuration follow, the pus would find it way into the vagina, and would not pour into the peritoneal cavity. The weakened scar due to suppurative non-union would be placed so near the external os that the normal course of subsequent pregnancy would not be endangered by it. Adhesions between intestines and scar could not take place.

But under favourable conditions, and particularly with the os fully open, they possessed in pubiotomy a still better operation, and he could not believe that the great profession in London had refused to give it a trial. He rather concluded that their attitude was still one of expectancy. They were waiting for more convincing proof than had as yet been presented to them, and he therefore came before them that night to present this further proof. From an experience of nine such operations, he could say it was easier in its performance than craniotomy. He was acquainted with no other operation where asepsis could be assured in so simple a manner. Patients walked on the seventeenth day, and suffered no pain or inconvenience at any time after the operation. The complications of which they had heard so much—namely, hæmorrhage and vaginal lacerations—were entirely due to carelessness and faulty technique. It did not pretend to cure sepsis, and should sepsis be present before its performance, the patient might die in spite of it, and thus afforded its detractors a favourable opportunity to compile statistics.

He asked them not to dwell too much on foreign statistics, but to accept the experience of their own countrymen until they found by personal observation that it was not reliable.

In reply to a question of Dr. Spencer as to whether the operations mentioned by Dr. Tweedy were performed in suspected or septic cases, he said he was endeavouring to show that in suspected cases it was a suitable operation. Though pubiotomy could not claim to be a cure for sepsis, he would deliver a woman in a suspected case by it. If the woman was septic she would possibly die. The operation was as easy as craniotomy; asepsis was more perfectly established in it than in any other operation. A case which would go wrong with craniotomy would perhaps go wrong with pubiotomy, but a case which would recover from craniotomy would also recover after pubiotomy had been performed. He asked that the latter operation might be given a chance.

Dr. H. Briggs desired to join with others in commending the enthusiasm and energy displayed by Dr. Routh in connexion with the report he had drawn up. From a sort of sidelight, the treatment of suspect cases came up for discussion that evening. A lurid light had been thrown on the large question by the statistics. For his own part, he felt more or less as if he were an impostor among those present, for in his whole experience he had not operated upon a suspect—i.e., if a suspect is an infected case. He was very anxious to learn from gentlemen who said they operated upon suspect cases the differences between the local and the general infections. Where should one draw the line? He did not believe that bacteriology had arrived at the stage when it could give an intimation as to where a local infection would stop. Most of the infections were and remained local, but some were general. He believed that in suspect cases there would be found a series of gross errors. Many suspect cases, after an unimpaired convalescence, were classed as clean cases. It was quite true that many cases might be saved where the organisms were local, and quite possible that those operators who did not operate upon suspect cases were refusing to operate upon cases which were entirely local. If at the end of the discussion he had learned when an infection in a suspect case was local, and when it was general, his visit would not have been in vain. There were many kinds of suspect cases.

It was found that a specialist was always ready to state some method of operating which he believed to be infallible, some short cut or some circuitous route, which general surgeons had adopted elsewhere, but given up long ago. Of all the persons in the world not to be trusted, it was the narrow-minded specialist in the absence of the general pathologist. He would like to hear the opinion of general pathologists on some of the subjects in this debate. An advocate of any particular operation had only to ask himself in regard to ophthalmia neonatorum where the organisms came from and in what proportion of cases they were found. He did not believe that the practical localization of the suspect infections under discussion was yet within reach, and he was inclined to adhere to his original Cæsarean section attitude, and to limit himself to clean cases, and refuse suspect cases, although he knew that, compared with some of his neighbours and friends, he must be refusing operation in cases where the infection was probably local. He questioned whether Dr. Routh had exhibited the darkest side of the results in the suspect cases. He doubted whether the table so admirably compiled by his ability was a table which showed the whole truth. He believed some cases escaped. Gratitude was due to Dr. Routh. He himself was still a strong believer in the induction of premature labour. He knew of many cases upon which Cæsarean section might have been performed, satisfactorily treated by the induction of premature labour.

In answer to a question addressed to him by Dr. Routh as to what he would do if he had to see a woman in labour whom he considered to be suspect, Dr. Briggs replied that he did see such cases, and he did not hesitate to perforate, because he believed, on general pathological grounds, that one could not distinguish between local and general infections.

Dr. FREDERICK EDGE said he did not wish to treat the subject on general terms, but would give a few details as to how the operation should be performed in the suspect cases. The scope of Cæsarean section should be much enlarged. It was the straightest, the least mutilating, and, apart from sepsis, the least dangerous of all the methods of delivery in cases of contracted pelvis. The question was one of avoidance of sepsis, which at first was confined to the vagina and the lower cervix; and which, when introduced into the uterus, was generally kept at bay for some hours by the membranes covering the uterine wall. That was especially pointed out by Dr. Maxwell in his method of intra-amniotic irrigation. The sepsis of ordinary puerperal fever was almost always introduced after the raw surface of the uterus was exposed by the separation of the placenta and membranes. That was why the old practitioners could take their forceps in their trousers pockets to keep them warm and apply them, and yet have no sepsis, because the germs were within the amnion. Could not that knowledge be applied in some way to the performance of Cæsarean section? Could not the operation be performed in such a way that the upper segment of the uterus was kept practically free from infection? He had had three interesting instances in which he performed Cæsarean section in suspect cases. The first was in the country, the membranes had been ruptured twelve hours before, and for eight hours attempts had been made at delivery. The uterus was in a state of tonic contraction, the contraction of the pelvis was not extreme, and although he could have perforated, still he suspected that the uterus might be ruptured, and so he decided that Cæsarean section would be safer. He first washed the uterus out as soon as the child was delivered, and after the placenta and membranes were removed he lightly packed the uterine cavity with iodoform gauze. In sewing up, one of the stitches caught in the gauze and gave trouble, but the patient recovered. That was a case where there was undoubted sepsis, and the clinical course showed it. In that case he thought it was due to the irrigation of the uterus and drainage that the patient recovered. In the second case attempts to deliver had been made, and he did ordinary Cæsarean section without hysterectomy, and, although the patient suffered from black vomit, she recovered after the stomach had been washed out two or three times. In a third case the rectum had been badly torn and the child destroyed in the previous labour outside, and a fistula resulted in the bladder large enough to admit the hand. He sutured that, but a small fistula remained high up behind the symphysis, rather to the right, and drawn outwards and upwards by scar tissue. Meanwhile she became pregnant, and was very desirous of having a living child. He let her go to term, and then performed Cæsarean section. There was no temperature, and he did not think the vagina was septic. He did not think there was danger, but in five days after Cæsarean section she was dead of acute peritonitis. Evidently the peritoneum was infected by the lower portion of the membranes, which were unruptured; they had been soiled from the vagina. There could be no question in this case of fouling by amniotic fluid.

He did not think hysterectomy would be specially suitable. If he had to repeat that operation he would cut off the cervical pole of the membranes, and push them down into the vagina with iodoform gauze. It was the intra-amniotic manipulation which was the safe one. He agreed that bacteriological help was not to be relied upon in critical cases in the country; even in town the reports were not always competent and trustworthy. Therefore, in doubtful cases, he would be inclined not to do hysterectomy, but ordinary Cæsarean section with those precautions. Since hearing of Dr. Maxwell's irrigation he would use that first. He would then eventrate the uterus and protect both the abdominal incision and the incision in the uterine wall. That was done regularly now. He would then deliver the child. Except in very foul cases, he did not think it likely that the child was at fault, but that it was the lower cervical pole of membrane which carried the germs. After delivering the child he would again irrigate, and then he could separate the placenta and membranes and leave the cervical segment to be pushed down into the vagina. It was an open question whether the uterus should be washed out; but he did not think it should. Light packing with gauze would be less likely to result in infection spreading. The incision should be as far away as possible from the cervix in septic cases. Two cases—Dr. Hewetson's and Dr. Victor Bonney's—though seemingly foully septic, recovered after Cæsarean section. If there could be more command of the sepsis, safety would lie in the future in what might be called normal Cæsarean section without hysterectomy. That, possibly with drainage of Douglas's pouch, unless the case were certainly septic, would have more and more success. If the uterus were taken away he had always performed the subtotal hysterectomy, because it was so safe and simple. Still, if hysterectomy were necessary, he believed that ultimately Dr. Herbert Spencer's panhysterectomy would be almost universally adopted. Finally, he intended in future to endeavour to push the placenta and membrane into the vagina—i.e., to conduct the third stage as much as possible as in a normal labour, *per vias naturales*.

Dr. ARNOLD LEA first added his tribute to Dr. Routh for his painstaking and laborious research. Dr. Routh's figures showed that those cases in which the membranes had ruptured showed a mortality of from 10 per cent. to 15 per cent.; after attempts at delivery had been made the mortality was as high as 34 per cent. At St. Mary's Hospital, Manchester, patients were frequently admitted who had been many hours in labour, and had been subjected to many examinations by midwives and doctors, the latter after having used forceps. These he regarded as "suspect" cases, though he would be glad to hear an exact definition of the term. If the child was living the general condition of the patient must be considered in deciding on the procedure. A temperature of 100° F. or 101° F., and some quickening of the pulse, had not been regarded as contra-indicating Cæsarean section. In sixteen cases of this "suspect" type in which operation was done during labour, after rupture of the membranes, and after interference by forceps, all but one recovered. In the last seven years none of the cases he had had of that type had died. The morbidity was 30 per cent. These results seemed to indicate that even after rupture of membranes for many hours, with interference, many cases escaped serious infection. Much depended on the cleanliness and care of those in charge of the patient. It was a pity for doctors under these circumstances to make attempts at delivery, and if there were more than one medical man each usually made an attempt. If such cases were sent to hospital earlier the results would be much better. The most essential requirement, as could be seen from the paper, was early diagnosis of the existence of infection. The clinical signs of such infection were admittedly uncertain. The increased temperature and pulse were often merely indications of toxæmia and bacterial intoxication from saprophytic vaginal organisms, especially if the membranes had been ruptured for some hours, and there was rapid subsidence of symptoms after the Cæsarean section. Much had been hoped for from bacteriological investigation of the amniotic fluid, and he believed that the organisms could be detected in "film" preparations, either by introducing a tube into the cervix and obtaining a specimen of the amniotic fluid, or by examining the cervical secretion itself. In hospital practice a report could be obtained in half an hour. The chief organisms found in puerperal infection were streptococci, staphylococci, *Bacterium coli*, and occasionally the gonococcus. In addition, anaerobic bacteria were often present. For present purposes those organisms might be present in the passages of a healthy woman, and there was not as yet any rapid method of distinguishing between saprophytic and pathogenic bacteria. The hæmolytic power of streptococci was valuable, but it required twenty-four hours to give a definite answer on that test. Still, the discovery of streptococci in the amniotic fluid or the cervix in abundance would be presumptive evidence of pathogenicity.

With regard to the technique of the operation, the vagina should be thoroughly disinfected, especially before the operation; also immediately afterwards with alcohol. By care in the opening of the abdomen much could be done to avoid infection of the peritoneum and amniotic fluid, even when it appeared to be putrid. A drainage-tube was useful. He had no experience of the supra-symphyseal method of Cæsarean section, but the transverse incision was often valuable. He referred to cases of diffuse cellulitis following the transverse operation. It would seem that the pelvic cellular tissue is almost as vulnerable to pathogenic bacteria as is the peritoneum. In cases of obvious infection, complete abdominal hysterectomy was the only safe plan in his opinion. He could not speak very hopefully of bacteriological diagnosis in the present stage of that subject.

Dr. HERBERT SPENCER said that not only the Section, but all obstetricians and gynæcologists in the country, owed a debt of gratitude to Dr. Routh for the immense labour and considerable expense which he had incurred in bringing forward his admirable paper on Cæsarean section. It was a monumental work, which would take its place beside Truzzi's work on the Porro-Cæsarean section, which was given to the world ten years ago. The difficulty met with in the cases under discussion had been well pointed out by Dr. Lea, as well as the difficulty of discussing the subject without a definition of the term "exposed to infection." Was one to conclude that a patient was exposed to infection because the membranes were ruptured and examinations had been made? If that were so, many had been operating upon "suspect" cases for years without knowing it, and every woman in labour ought to become infected, because she always had the membranes ruptured, and nearly always was submitted to internal examination. If a woman had not been in labour a long time and the membranes had been ruptured, he was just as willing to operate by conservative Cæsarean section, as if she were not in labour and the membranes were intact, provided she had been examined with due precaution. It was a sad thing that in 1911 conditions were such as to justify a paper being brought before the Section with such a title, implying that the general practitioner allowed cases of contracted pelvis to be long in labour, and yet not to be surrounded by every precaution against sepsis. It was urgently necessary to teach the student how to treat these cases, and he really did think that in London they were taught in that matter better than in some other parts of the kingdom. In London one did not see the dreadful cases of sepsis resulting from repeated examinations and instrumental attempts by several practitioners—at least, not nearly so frequently as appeared to be the case in Glasgow, for example—in which city Dr. Munro Kerr had given his experience. How was one to know that the cases were infected? He would pay attention to the patient's general condition, rather than to the fact that the temperature was 100° F.; that was often reached in a normal labour, with an aseptic recovery. Bacteriological investigation might be important in the future, but in the present state of knowledge he would not trust it very much. What should be done in seriously infected cases? He meant such cases as those from the slums of a large city where two or three strong doctors had taken a turn at a case with contracted pelvis, and perhaps killed or mutilated the child. In these gravely infected cases Cæsarean section should not be performed. Where forceps had been applied the child's life was, in many cases, seriously compromised. It was wrong not to do craniotomy in such a case. If the uterus were torn much, he would supplement the craniotomy by vaginal hysterectomy.

There was one type of case, of which an example appeared in the paper, of neglected labour, which was one of the most difficult which the obstetrician could be called to—namely, to perform Cæsarean section in a case of neglected shoulder or trunk presentation after the child was dead, and in which the uterus was retracted and the pelvis small. Some years ago he published a method of meeting those difficult cases and avoiding the possible alternative of Cæsarean section, which he invited trial of by those who had not done it. If one cut through the spine of the child and put on Braxton Hicks's cephalotribe, the points of which met, and made traction, the soft tissues could be drawn down, the child could be cut into two, and be delivered without difficulty. He had done that successfully on three occasions.

With regard to symphysiotomy, pubiotomy, and extraperitoneal Cæsarean section, he did not think they should be performed in cases of infection. Dr. Hastings Tweedy had given a highly coloured description of the operation of extraperitoneal Cæsarean section in aseptic cases, with which this discussion was not concerned, and the procedure seemed to be so favourable, in Dr. Tweedy's opinion, that one would almost conclude it was the future lot of women to be delivered by extraperitoneal Cæsarean section. Yet, on looking at the literature, it would be found that there were cases of rupture of the bladder and peritoneum, and one in which the plexus of veins had bled so furiously that the operator (Döderlein) had had to abandon the operation for the classical Cæsarean section. The matter was not always so simple as Dr. Hastings Tweedy had found it. Moreover, in extraperitoneal Cæsarean section it took a long time to deliver the child, and there was increased risk to its life in consequence, and the wound did not heal at all well, as shown by Döderlein's experience and advice to drain in all cases. With regard to gonorrhœa, he thought it would be best, if the cervix was probably infected, to remove the whole uterus. For the cases of obstructing fibroids and for cases of cancer where labour was not advanced, he thought Cæsarean section with total abdominal hysterectomy should be preferred. For advanced cases of cancer which were not operable, he thought Dr. Routh's paper showed that the operation which he (Dr. Spencer) recommended many years ago, the old Porro's operation, was the most suitable. With regard to Dr. Tweedy's and Dr. Edge's views on Doyen's operation, members would remember a remarkable case which Dr. Turner published some time ago of a suppurating fibroid after labour which was a very successful instance of Doyen's operation in a gravely infected case.

The remaining question was as to how to avoid infection of the peritoneum and abdominal wound when doing Cæsarean section in suspected cases. Dr. Edge had made some valuable suggestions, which should be borne in mind. Precautions should be taken against infecting the abdominal wound, but he did not think it was practicable or necessary to disinfect a putrid amniotic cavity. If the whole of the infected tissues were removed by total abdominal hysterectomy, it did not matter much if the healthy uterine peritoneum was temporarily soiled.

Dr. THIELE said that in examining a large number of swabs from the cervix and vagina, the first thing noticed in hospital work was that one always met with bacteria: diphtheroid, cocci, and non-Gram-staining rods of the coliform type; and there were thus always present organisms which are potential sources of danger. It was always difficult to know what the organism was from direct examination of the swab; thus, when seeing a Gram-staining isolated coccus or diplococcus, one could not say whether it was streptococcus, staphylococcus, pneumococcus, or another organism of that description, and cultivation always took time. Short of that, the pathogenicity of the organisms could not be determined without prolonged cultural and other tests. It was very rare to find organisms in such number and of such definite character that one could be sure of them. Even where the labour had lasted for some time and the amniotic cavity had been exposed to the cervical and vaginal infection for hours, it was very rarely that one could find pathogenic organisms spreading in very large quantity; even in a very intense infection the number of organisms might not be at all numerous; and, moreover, organisms which appeared to be absent on direct examination might turn up in the cultures. Therefore, from the bacteriological point of view, there remained much to be desired in regard to detecting the nature and virulence of infection in possible cases of this sort.

Dr. Arthur H. N. Lewers desired to associate himself with the congratulatory remarks of previous speakers concerning Dr. Routh's valuable contribution, and the great industry which had been bestowed upon drawing up the table. His own experience within the limits set by the title of the paper comprised two cases. It was difficult to say exactly what should be called infected cases, and the same difficulty had been felt by other speakers. The first case had been examined by several maternity assistants in the outdoor department of the London Hospital, and as there was no progress after several hours, the patient was admitted into hospital, and after the membranes had ruptured, axis-traction forceps were applied for seven or eight minutes by the resident accoucheur. She had been examined by several people outside, the membranes were ruptured, and forceps had been applied. He believed, however, the precautions taken by the maternity pupils and the resident accoucheur were such that the case was not infected by them. He was sent for, and as the child was alive, and as it was impossible to deliver with the forceps, a conservative Cæsarean section was done, and the patient made an uninterrupted recovery. He had had a more striking case than that, and it occurred since he sent the cases in which Dr. Routh asked for. It was certainly an infected case. The patient came from thirty miles out of London, was a dwarf, and had a pelvis with a true conjugate of about 2⅝ in. She had been three days in labour, and had been examined by several practitioners in the town, who had been for a long time in doubt as to what should be done. After being in labour that long time she was sent up to the London Hospital, and was admitted late in the evening. When he saw her the temperature was subnormal, the pulse 140; she was having very strong pains, and altogether her condition was certainly serious. But the child was alive, the fœtal heart being quite strong. He was unwilling to perforate a living child, and he thought the danger of Cæsarean section in some of the cases of prolonged labour had been somewhat exaggerated, because many of the figures quoted applied to a period of time when the operation was much more serious than was the case at present. The patient stood the operation fairly well, and the child was delivered alive. Ultimately the recovery was good. The existence of infection was proved by the formation of an abscess, which opened spontaneously in the lower angle of the abdominal wound, and also into the vagina. After the abscess opened, the temperature fell. Dr. Routh's Table I covered a period of twenty years, and showed a mortality in suspect cases of

17 per cent., and a mortality in other cases of nearly 3 per cent. Probably many of the cases in which the mortality was 17 per cent. belonged to the earlier ten years of that period, and the figures were likely to be much better in the second period, owing both to the improved technique and to more complete asepsis. He could remember the time when in surgical practice generally instruments were not boiled; this was not much more than twenty years ago, if, indeed, it was quite so long. He confessed he would be unwilling to wash out the uterus before the operation. He doubted whether, with the fœtus in it, one could wash out sufficiently to get rid of everything which should be removed; and there was danger in doing anything which could be avoided if Cæsarean section was going to be done afterwards. The risk of carrying in something, in his opinion, was much greater than the chance of completely washing out septic material, if present. There was a great advantage in using silkworm gut sutures in the uterine wound, as recommended by Dr. Gow. If the uterine aspect of the wound should become septic, silkworm gut, being non-absorbent, was safer than catgut, or silk, which would absorb anything septic which happened to be near.

Mr. ERNEST H. SHAW said he had not had much experience in the line dealt with by the paper, his experience had been principally general hospital work and private work on cases after childbirth. Therefore he could not say anything about examination of films at the time of labour. In his routine work he had divided his cases into two main types: (1) those in which there was some chronic infection of the cervix, uterus, and appendages; (2) cases of infection after childbirth or abortion. In the first group he had been concerned chiefly with the detection of gonococci, and while examining films and cultures for this purpose had been struck with the abundance and variety of the bacteria in the cervix and vagina, and also by the common occurrence in the cervical canal of the white staphylococcus. He thought it was desirable to know as much as possible about the types of organisms which were present in non-pregnant cases, because he thought it was the later entrance of some of these organisms into the body which produced the bad septic cases. He often found a Gram-positive bacillus known as the pseudo-diphtheria bacillus in the cervix. The organisms in the vagina included Gram-negative and Gram-positive bacilli, and various kinds of staphylococci and streptococci. With regard to infection of the uterus at childbirth, that might be due to any of the organisms he had mentioned, but he believed the commonest was the streptococcus, both the long-chained and short-chained forms. The next commonest, and the only one he had found in addition, was the *Staphylococcus albus.* He was surprised to find it in several cases of what was termed puerperal fever. He attached more importance to that organism than most observers did, because he had found it in infective endocarditis, in abscesses, and in a case of definite septicæmia at the Great Northern Hospital. In this case he found the white staphylococcus in the blood of the patient during life and in the blood and spleen after death. Recently he had been following a routine examination of swabs and smears from the cervix and uterus in cases of infection after childbirth; microscopically, culturally, and by blood culture and blood-count. He had been trying to find out whether in a given case the condition was localized or general. The cases which he had been able to demonstrate to his own satisfaction as septicæmia had been those in which streptococci were present in the blood. In some of the cases which he had investigated he had been told that consultants had diagnosed septicæmia on clinical data, he had found most of them free from organisms in the blood; therefore he regarded them as simply examples of sapræmia. He concluded from that that it was difficult to ascertain the degree of infection from the clinical signs, i.e., whether the infection was local or general—a most important point for prognosis. He suggested that as the streptococcus was the most common organism found, each case should be treated as one of streptococcal infection until the contrary was proved. He believed that the injection of polyvalent antistreptococcic serum did good in such cases. If he obtained a pure culture, he also tried the use of vaccines. He was convinced they did no harm, and had seen some cases do well under this treatment. If the growth were mixed, he subcultured the predominant organism and prepared a vaccine from this. In cases of labour infection, he suggested that the vaccine should be made from the organism grown from the patient, either from the blood or cervix. When Cæsarean section was done he did not see why some polyvalent antistreptococcic serum should not be put into the uterus, into the vagina, and into the peritoneum. If the uterus were removed, why should not some serum be applied to the stump? Some could also be projected down the vagina, and some could also be used subcutaneously.

Dr. W. J. Gow said the point on which he desired to comment was as to what should be done in cases where it was known or suspected that the amniotic fluid was infected with pathogenic micro-organisms. That was the point on which Dr. Routh invited opinions. There had been some confusion between infection of the patient and infection of the liquor amnii. When discussing the treatment of a certain kind of case, it was better to distinguish between those two groups. What he would say applied to infection of the amniotic fluid. Obviously, if a patient was infected with streptococci during delivery, she ran risks of dying of puerperal fever, whether she were delivered by Cæsarean section or by any other method. But there were cases in which the patient was free from infection constitutionally, and yet the liquor amnii contained pathogenic organisms which, when ordinary Cæsarean section was done, would inevitably escape into the peritoneal cavity. In the cases which Dr. Routh called "suspect," it was the liquor amnii which was suspect, not the patient. Forty of his own cases were not in labour, or were only in the first stage of labour, and the membranes were either unruptured, or only just ruptured. There were no deaths. There were eight cases in which the membranes had been ruptured for some time, and of those one died. In this case the membranes had been ruptured forty-eight hours, and several attempts had been made to deliver with forceps before she came to hospital. She died on the fifth day, and a staphylococcus was cultivated from the peritoneal lymph. The important questions raised by Dr. Routh could only be answered conjecturally in the present state of knowledge, because only lately had the importance of infection of the liquor amnii been appreciated. The difficulty was to know on what grounds one should regard that fluid as suspect. The bacteriologist was not always available when wanted, and a rapid verdict was required. Moreover, there was no present assurance that his verdict when given would be absolute; therefore the obstetrician must act on suspicion. It would be agreed that suspicion was justified when the membranes had been long ruptured, and there had been several attempts at instrumental delivery, especially the latter. There were many organisms in the vagina which, when introduced into the liquor amnii, rapidly multiplied, and were very undesirable inhabitants of the peritoneum. How should those cases be dealt with? He had dealt with the cases precisely as with those where the liquor amnii was not infected, with the result that one death occurred. Some of them could best be dealt with by craniotomy, especially in cases where the child's life had either been already lost, or was seriously embarrassed by numerous attempts at forceps delivery. The other alternatives were abdominal hysterectomy or conservative Cæsarean section, special efforts being made in the latter case to avoid peritoneal infection.

Conservative Cæsarean section would probably give quite good results in the future if the following precautions were taken: Where attempts had been made some hours before to deliver by forceps, it would be well, where practicable, to irrigate the uterine cavity, as suggested by Dr. Maxwell (this was not practicable when the head was jammed in the lower uterine segment, or in the pelvic brim), then to eviscerate the uterus, protecting the edges of the uterine wound with a sterile towel or sterile gauze, and taking care that none of the liquor amnii got into the peritoneal cavity. Frequently the changes in the liquor amnii were manifested by a highly putrefactive odour. It was the peritonitis which killed the patient, and the peritonitis was set up by the infected liquor amnii gaining admission into the abdominal cavity.

(The discussion was adjourned until February 3.)

Obstetrical and Gynæcological Section.

February 3, 1911.

Dr. H. MACNAUGHTON-JONES, President of the Section, in the Chair.

Discussion on the Indications for, and Technique of, Cæsarean Section and its Alternatives, in Women with Contracted Pelves, who have been long in Labour and exposed to Septic Infection.[1]

DR. R. DRUMMOND MAXWELL said he could not claim to have a great experience of Cæsarean section, since he had done only four cases, but he had been fortunate enough to have had opportunities of studying the methods and technique of many of the senior members who had taken part in the debate. That experience had certainly impressed him with the necessity of exercising the greatest care in investigating these cases from the point of view of prior sepsis. As a Resident Medical Officer in Queen Charlotte's Hospital in 1903, Dr. Maxwell recollected that the routine inquiry made in these "suspect" cases embraced not only the nature of the previous obstetric manipulations, but even went further in the attempt to find out who had performed them. What could only be defined as the "antiseptic sense" was rarely acquired other than by holding a responsible resident surgical appointment, and if infection could not be guaranteed against after examination *per vaginam* by the skilled, how much greater was the risk after examination and manipulation at the hands of those with a very imperfect practical standard of antiseptic details. He could recall cases where an element of doubt could be entertained reasonably on this point and embryotomy was performed on the living child rather than Cæsarean section. As to whether such precautions were necessary or not, by way of answer he could only point to the statistics of Dr. Gow, who had been one of the first to emphasize precaution in this direction, and whose results in Cæsarean section amply justified his teaching.

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[1] Adjourned from February 2.

All the Members present had probably seen Cæsarean section performed in cases where there was reason to regard the patient as more certainly exposed to infection than those even of Dr. Routh's "suspect" group. One was bound to say one had seen these patients recover, but before accepting these cases as evidence that all precautions were unnecessary one would have to investigate the puerperal morbidity most carefully. Some of these cases caused the very greatest anxiety. There was the gravest ileus in some; in others the belly wound suppurated; again, sometimes localized abscesses pointed and burst by the abdominal wound or *per vaginam*. Now, the border-line between the cases that eventually recovered after grave morbidity and those that died of a rapid peritonitis in two or three days was such a very narrow one that he (the speaker) held that all serious puerperal morbidity should be frankly accepted as evidence that embryotomy or some modification of technique should have been carried out rather than the classical Cæsarean section. In the particular case that he had in his own experience, the patient, a rachitic dwarf, primipara, could be reasonably regarded as of the "suspect" group. Inquiry as to the nature of the manipulations and the care with which they had been performed made one feel that, in the presence of a live child and the absence of clinical signs of infection, some modification of Cæsarean technique rather than embryotomy was indicated. The method adopted consisted in ante-partum irrigation of the uterine cavity with normal saline solution, associated with incision of the eventrated uterus.

Dr. Maxwell had great difficulty in convincing himself that in any case where forceps had been applied the amniotic cavity had escaped infection. True, an almost negligible infection from the point of view of delivery *per vias naturales*, but of the gravest import from the possibility of peritoneal infection. It was important that we should be quite clear on the distinction between infection of the amniotic contents and infection of the patient. In those circumstances he felt that any attempt to get rid of the infected liquor amnii, or at least to dilute its toxicity, was a rational procedure. The technique was perfectly easy in his case; a soft pewter douche tube was passed up to the fundus past the fœtal head, and normal saline, temperature 105° F., was allowed to flow for five minutes. The uterus was then eventrated, and incised at the fundus after most carefully packing off of the peritoneal cavity. Subsequently, after closing of the uterus, towels and rubber gloves were changed and the belly sewn up with fresh instruments. He would not claim that this must be the routine treatment of the "suspect" case. The method obviously had its limitations. For instance, in the case recorded by Dr. Lewers on the previous night, where the fœtal head was firmly impacted in the brim, an attempt to displace the fœtal head would have been unjustifiable in the presence of the almost tonic action of the uterus. Again, as regards the irrigation of the uterus, he would not attempt to convey the impression that it was a true lavage of the uterine cavity, but he was confident that he removed a large amount of the remaining "waters" by this method, and by "eventration" prevented absolutely any drop of uterine contents entering the belly cavity. As regards the ethical aspect of embryotomy, Dr. Maxwell said he would have no hesitation in the presence of clinical signs of sepsis, of sacrificing a living fœtus in utero.

The bacteriological aspect of the paper was one of its most interesting features, and all who read it must have been struck with the consistently accurate forecasts of the infective organism made by Mr. Leathem from simple "smears." Dr. Routh had kindly asked him (the speaker) if he could corroborate this investigation independently. Dr. Maxwell, acting with the assistance of one of the bacteriological staff of the London Hospital, had made six similar observations on material removed from the uterus in cases of puerperal infection. In two cases the culture proved sterile; in the remaining four the cultures grew either streptococci or *Bacillus coli*, while the examinations of the smears had been negative. Dr. Maxwell said that Dr. Bulloch had not been surprised at this finding, and that the latter had given it as his opinion that even if streptococci had been seen on a "smear" they would not be proof of infection, since they might not necessarily be pathogenic germs, any more than the presence of the pneumococcus on a swab taken from the upper air-passages would be evidence of acute lobar pneumonia. Therefore, he was unable to bring evidence to corroborate Mr. Leathem's report, but he thought it was quite reasonable to expect that the bacteriologist might soon discover some differential methods of staining that would clearly and rapidly distinguish between a pathogenic and a non-pathogenic organism.

Mrs. Scharlieb said she felt diffident in contributing her few remarks to the discussion, because she had but a small number of Cæsarean sections to her credit. But all Fellows present would wish to thank Dr. Routh for his most valuable paper, and for giving all—those with few cases as well as those with many—an opportunity of recording their views. All would admit that there was great difficulty in knowing which cases were infected or suspected of infection, and which were not. One of her cases, No. 923 in Dr. Routh's list, was distinctly unfavourable. She had a generally contracted and much flattened pelvis, with a conjugate of 2½ in., she had been long in labour, Bandl's ring was present, the membranes had been ruptured twenty-four hours, her pulse was 108, and there was constant vomiting. The child, although presenting by the head, passed meconium, and yet the patient recovered after ordinary Cæsarean section without any drawback. Therefore she could not suppose that the patient had been infected before delivery. With regard to bacteriology, all must hope that in the future bacteriological science would so advance that the results could be obtained both more certainly and more quickly than hitherto. It was the duty of those who undertook those cases to have a bacteriological examination made of the liquor amnii and of any discharge which might be present, not only with the view of guiding the treatment at the moment, but also in order to furnish a vaccine, which might prove of inestimable value in the subsequent treatment.

Dr. Maxwell's suggestion of interamnial investigation seemed a very good one, but where there had been repeated examinations, not only was the amnial cavity infected, but in many instances the cervix and the lower portion of the uterus were damaged. If so, no disinfection of the amnial cavity, and no ordinary Cæsarean section, would avail to save the patient from infection. In such a case she thought the wisest plan was to perform abdominal panhysterectomy, leaving a sufficient opening in the vaginal vault through which drainage could be carried out, either by iodoform gauze or by bismuth gauze. If in spite of the delay which had occurred the child was alive, the uterus could be eventrated, the abdominal wound temporarily closed behind it, and ample packing done with sterilized towelling to prevent, if possible, infected liquor amnii from entering the abdominal cavity. An incision should be made to permit of the removal of the fœtus, in whatever position seemed to be most convenient at the moment, carefully avoiding the placental site. As soon as the child was removed and the incision in the uterus closed, panhysterectomy should be performed, an operation which, under the circumstances, presented remarkably little difficulty. She could not believe that in any infected cases either extraperitoneal hysterectomy, or symphysiotomy, or any variety of pubiotomy, was advisable. The latter two had a very limited field of application in any case, and in the infected cases surely it must be difficult, if not impossible, to avoid infection. One ought not to allow any further opportunity for infection than was absolutely unavoidable. The establishment of a hystero-abdominal fistula might be a refuge for the destitute, but it was not a method which would be elected. She thought all would prefer the absolute removal of the whole of the infected organ by panhysterectomy, and general drainage. No doubt all obstetricians detested the idea of doing craniotomy on a living child; still, sometimes it was a regrettable necessity, especially where everything was unfavourable to operation, and it gave the chance of saving the woman's life; there was no reason in sacrificing two lives when one sufficed.

Teaching should be so directed that the younger members of the profession should be encouraged to do abdominal work, so that they would not be taken at a disadvantage in a critical case, and could give the patient the benefit of panhysterectomy or a conservative Cæsarean section, as might be indicated in cases with narrow pelves, so narrow that there was no hope of delivery *per vias naturales*. She thought it a pity not to say a word in favour of the old-fashioned English treatment of inducing premature labour. In early days in India, when she had no skilled chloroformist or skilled assistant, she would have failed to save many lives of mothers and infants if she had not been able to bring about induction. She had never found it dangerous to the mother, nor had she seen a woman even ill in consequence of the induction of labour. A considerable percentage of children lived after it, especially if it were possible to postpone the operation of induction until the child was not only technically viable, but sufficiently developed to have a really good chance of life.

Sir FRANCIS CHAMPNEYS expressed his admiration of Dr. Routh's work. His paper, read before the recent International Congress in St. Petersburg, was a model of public-spirited investigation. Dr. Routh had started with an open mind in search of the truth, and his investigations had brought us all sensibly nearer to that end. The search must have been most laborious, but its value was directly proportionate to the pains taken—which was not always the case. In any examination of the subject of Cæsarean section his name must always be remembered.

The speaker only intended to allude to one point. In the abstract Dr. Routh referred to "the inevitable abrasions of the cervix during the operation" (of craniotomy). Now craniotomy consisted in perforation of the fœtal head, and he did not see why this should inflict any "inevitable abrasions." Dr. Routh probably included in the term "craniotomy" the further operation of extraction, but the two were separate operations, and extraction need not always follow perforation. The first perforation for contracted pelvis which he ever saw was left to Nature, and expulsion followed without difficulty. He believed that the danger of "craniotomy" was essentially the danger of extraction, and this might be considerable. In any future statistics of craniotomy he thought that the cases should be divided into two classes—those in which extraction did not, and those in which it did follow perforation. It was obvious that natural expulsion was not likely to occur when the conjugate was less than a certain size—say 3 in., or at least 2¾ in.—the labour being at full time, and the child of average size. As regarded the ethics of craniotomy, he had already given his opinion at length in a former debate a few weeks ago, and need not repeat it. The perforation of a living child was, of course, a most painful and unpleasant operation, but it ought to be less painful than allowing a mother of a family to die for want of it.

Dr. A. J. Wallace said there recently occurred in his practice two cases of probable infection in which Cæsarean section was done, and he wished to bring before the Section the method adopted in those cases. He had been very fortunate in the past in that only one case of infection had occurred amongst his conservative Cæsarean sections, and that patient died. He did the operation against his better judgment. In October last a patient came into hospital (a primipara, aged 22) after she had been three days in labour. Many attempts had been made at delivery, and her pulse on arrival was 140. Her pelvis had a conjugate of only 2½ in. The child was alive. He did not feel disposed to attempt delivery by any means other than Cæsarean section. He recognized that the patient was a "suspect case," and that there might be danger in opening the peritoneal cavity. For some years he had been endeavouring, in cases of contracted pelvis, to bring about a condition of affairs whereby a second and subsequent Cæsarean section might be made by an incision direct from the skin into the uterus without opening the peritoneal cavity. The method he adopted was to sew the parietal peritoneum to the wall of the uterus. In one case he had done extra-peritoneal Cæsarean section by that method. In the first "suspect" case referred to the abdominal incision was made, and the parietal peritoneum was sewn to the wall of the uterus by a suture which was duplicated, two rows of overlapping sutures all round, the resultant opening being 6 in. in length and 3 in. wide. A vertical incision was made into the uterus, and, after the extraction of a living child, a drainage-tube was placed through the abdominal wall, down to the uterus. From the first there was a foul purulent discharge through the drainage-tube; the patient's pulse was rapid, and on the third day the wound showed signs of opening up. When opened up it was found that universal sloughing was going on. In course of time a slough came away from the uterus. The patient ultimately recovered, and the utero-parietal fistula closed up. The second case was dealt with in a similar way. The patient came after labour had been in progress thirty-six hours, and forceps had been repeatedly used. A purulent discharge occurred from the two drainage-tubes used. The uterine wall did not slough. He inoculated the wound with the lactic acid bacillus, which he thought was of use in such cases. Two days afterwards all the offensive purulent discharge had ceased. That was his usual experience with the lactic acid bacillus when inoculated into offensive wounds. He considered that in the first case certainly, and perhaps in the second also, life had been saved by the preliminary use of the utero-parietal suture.

Dr. W. S. A. GRIFFITH expressed his regret that he was unable to be present the previous day at the opening of the discussion, and wished to express his admiration for the work Dr. Routh had done in collecting and analysing the statistics of Cæsarean section. He wished to point out that, while it was obviously the duty of all who had the opportunity for investigating their "suspect" cases to do so in the most accurate and scientific manner possible, it would still, in the emergencies of practice, be necessary for the great body of practitioners to depend on their experience and powers of observation, rather than on scientific methods of bacteriology, for the determination of the method of treatment most suitable in the case before them, especially as even the most experienced bacteriologists were often unable to state more than in general terms the exact nature and virulence of the organism present. The same criticism, he submitted, applied to the custom of most writers, of whom Dr. Routh was one, to apply methods involving mathematical accuracy in the measurements of the pelvis by which to determine the treatment applicable to the various degrees of contraction. He would ask

Dr. Routh, did he, or anyone present, determine his treatment by those methods except in the rare cases of excessive contraction? Was he not guided as much by other important factors? We could neither rely on our exact measurement of the conjugate, nor even on our calculation of the duration of pregnancy.

With regard to the term craniotomy, which, like so many terms in obstetrics, was so loosely used, he would point out that it was not the perforation that endangered the mother's life, but the method of extraction which followed it.

He had only had one bad septic case on which to perform Cæsarean section; in this there was in addition a mass of fibroids for which he performed hysterectomy after delivering the child. That patient died, the only fatal case in his series of 24 cases.

Dr. A. W. Russell, as the only one from Scotland who had as yet spoken, desired to join in the expressions of admiration of the paper, a most valuable piece of work, embracing as it did a historical series of British cases of Cæsarean section. Dr. Routh had done right to limit the scope of the present discussion, though some of the expressions used on the first evening did not seem to harmonize with that limitation. Of course, "exposed to infection" did not necessarily mean "suspect" or infected. It was not always the cases presumed to have been exposed to infection that went wrong. Sometimes the patient, who had been resident in hospital for some days before the operation and had not been much handled, and then only by the hospital staff, became septic. He was sure Dr. Routh's list contained such cases. Therefore no precautions must be slackened, but obstetricians must be on the alert to take advantage of all improvements which would increase the success, as well as widen the scope, of the operation. He had now had thirty-three cases of Cæsarean section—intraperitoneal and extraperitoneal—with no maternal death. Twenty-nine of these appeared in Dr. Routh's list. In three of them he attempted extraperitoneal section, and in one since that list was completed. His cases were probably unique in regard to the frequency of contracted pelvis as an indication for the operation. It had been the only indication for operation, except in one case, in which it was a last resort owing to repeated eclamptic attacks some time before the full term of pregnancy had been reached. The patient was in such a bad condition that if he had tried to induce labour in the usual way the convulsions might have recurred, and her life would almost certainly have been lost. In that case he had consulted with his colleague, Dr. Munro Kerr, and had the benefit of his assistance at the operation. Both mother and child survived, although the latter at first weighed under 3½ lb.

In regarding cases as "suspect," he had not considered the previous history, or attempts with forceps, so much as the state of the mother at the time, and the vitality of the child. But he knew the risks of handling. He had a great dislike of the barbarous procedure of craniotomy. He was glad to hear one speaker emphasize the desirability of the obstetrician being also an abdominal surgeon, or at least an operating gynæcologist. He had invariably taken great care in the preparation of the exposed parts, and had tried to have the patient in hospital some days before the operation. There was good accommodation for that in the new Maternity Hospital in Glasgow. He laid great stress on the disinfection of the vagina by copious douching, and scrubbing of the parts, which he personally supervised. The disinfection of the surgeon and his assistants was also important; they wore rubber gloves, armlets, and masks. He had never eventrated the uterus before incising it, but carefully packed it round with large swabs. He had everything ready beforehand, in order to get through quickly. When the child was removed, the placenta and membranes were peeled off and the interior of the uterus was thoroughly cleansed—a procedure he considered very important. He even used the scalpel in a case to scrape the surface of the uterus where there seemed to be softened membrane, with possible infection. This was one of his cases which had phlegmasia, but afterwards recovered. He thought an operator would be justified in doing panhysterectomy in such a case, but he had risked the conservative operation. With regard to morbidity during the recovery of the patient, he did not hesitate to wash out the uterus if it seemed necessary. For sewing up the uterine wound he used catgut, usually chromicized, and put a continuous suture of the peritoneum over the top of the interrupted sutures. In some cases in which abdominal or a second Cæsarean section had to be done afterwards, the wound had been found to be thoroughly strong from the previous operation.

With regard to extraperitoneal Cæsarean section, as he was one of the very few who up to the present had done the operation several times in this country, he might be allowed to refer to its scope in the kind of cases under discussion. It was now admitted that it was not freely available for actually septic cases, as was expected by its original promoters. There was a class of cases, however, in which the classical intraperitoneal operation would seldom be considered—viz., the cases that were advanced in labour with the presenting part bulging forwards above the symphysis and the lower uterine segment stretched. In his cases he followed the method of Sellheim and Döderlein, using Pfannenstiel's transverse incision, distending the bladder with sterile fluid, and using blunt dissection through the cellular tissue to the cervix. In undoubted cases of infection that were otherwise favourable he would certainly say there was a place for extraperitoneal section, either draining into the vagina or keeping the wound temporarily open by sterile gauze packing till the danger of spreading infection was over. He could corroborate Dr. Tweedy, who on the previous evening had said that with the right technique it was practically as easy an operation as the intraperitoneal method and with no more likelihood of hæmorrhage. He would prefer to describe it as complementary to the usual intraperitoneal operation, and specially available in cases of advanced labour, even where the patient on this account had been exposed to infection. Had there been time he would have liked to say more on the subject, but what he had said might serve to indicate the results of his experience in Glasgow.

Mr. A. N. LEATHEM said that when Dr. Routh first spoke to him of the possibility of ascertaining the presence or absence of pathogenic organisms by means of examination of films he (Mr. Leathem) expressed his opinion that a fairly reliable report could be given as to the presence of certain pyogenetic cocci, provided they were present in considerable numbers. The question as to the identity of bacilli was more difficult, and when a Gram-negative bacillus was present, and was of about the same size as *Bacillus coli*, one would have to regard it as being in all likelihood the *Bacillus coli*. The number of swabbings he had examined was of course far too small to enable conclusions to be formed as to the reliability of this attempt at diagnosis as to whether or not the amniotic cavity had been infected. But he did think that the conclusions he arrived at by the examination of films were sufficiently justified by the results of the subsequent cultural examinations to encourage further investigation in this direction. He was quite aware of the weak point in his examinations, namely, that these swabbings were not swabbings of amniotic fluid, but from situations where the bacteria were likely to be more numerous than one would expect to find them in the amniotic fluid; but he took it that Dr. Routh made a point of the examination of swabbings from the cervix, as, if that contained pathogenic organisms, these were practically certain to have been introduced into the uterus if frequent examinations or attempts at delivery had been made.

It had been pointed out how difficult it was to identify organisms in films, and that a single Gram-positive coccus or pair of cocci might be anything—staphylococcus, streptococcus, or pneumococcus. With regard to the pneumococcus, that could, as a rule, be easily identified by suitable staining. With regard to the others their identification, unless the streptococci happen to be in distinct chains, was largely guess-work, and he thought in the conclusions he made from examination of films the words he used were: staphylococci or streptococci "may" be present.

On the question of distinguishing between the pathogenicity or non-pathogenicity of organisms seen in films he was not prepared to say anything. If staphylococci or streptococci were seen in films he would assume that they were pathogenic, not saprophytic. He did not feel at all hopeful that for some time to come it would be possible, as had been suggested, to distinguish between pathogenic and non-pathogenic streptococci by any means of differential staining. He admitted that the attempt to recognize pathogenic organisms in films made direct from swabbings would probably come to this—that the presence in the films of Gram-positive cocci, unless they were obviously sarcinæ, or the presence of Gram-negative bacilli, would lead to the idea that dangerous infection was present, and that this view might in some cases lead to measures that were unnecessary. Still the error would be on the side of safety. Professor Bumm made use of this method of examining films made direct from the amniotic fluid, and appeared to find it satisfactory. The washing out of the uterine cavity and vagina with antistreptococcic serum after Cæsarean section would be thought to be of very little value. He would be much more inclined to use perfectly fresh horse serum if that could be procured.

Dr. C. HUBERT ROBERTS said that, like many other speakers, he found great difficulty in defining a "suspect" case. Many cases *must* be "suspect," but they could not say to what degree, and yet conservative Cæsarean section seemed successful, at all events in the cases he had operated upon or seen operated upon at Queen Charlotte's Hospital and elsewhere. He did not pretend to have such a large experience of Cæsarean section as some operators, and he had lost one case. This was doubtless due to infected liquor amnii, as a utero-abdominal fistula formed, with fatal results, the patient dying of peritonitis about three weeks after the operation (which was a conservative one).

He confessed that in spite of what other speakers had said, especially Dr. Hastings Tweedy, he was convinced there were cases where craniotomy would have to be performed on the living child—sad as this might be—but, surely, in remote districts, and under unfavourable circumstances, far away from any hospital, craniotomy would save more mothers than Cæsarean section hurriedly performed, or performed by someone who had never done it before. Indeed, but for craniotomy, many mothers under such conditions must die undelivered. This, he thought, was also the view of several distinguished obstetricians in Scotland.

At Queen Charlotte's Hospital from time to time most serious cases of obstructed labour were admitted, in which every possible attempt at delivery had already been made outside, and where, clinically at all events, infection seemed very evident. In such cases the child was dead or dying, and craniotomy or embryotomy was performed, yet such women often went through a normal or almost normal puerperium. He contended that if Cæsarean section were done on such cases it would have added a grave risk to the mother and would not have saved the child.

Now, as to the question of Cæsarean hysterectomy in infected cases, he admitted, with others, that it was the ideal operation, but he did not know how to make a sufficiently correct diagnosis in order to say that "this is a case for preservation of the uterus, or this is a case for complete removal." Even bacteriologists seemed to admit that they could not help much; moreover, in desperate cases there was little time for delay. Though he joined issue with those who thought that complete removal of a septic uterus after Cæsarean section was the best operation in certain cases and under suitable conditions, he felt disposed to support those who treated the infected stump extraperitoneally after removal of the uterine body by some modified method of Porro's operation (the uterus being eventrated before opening) and especially if the mother was *in extremis*. He had treated one such case by this method, after Cæsarean section, in which there was a partial rupture of the uterus. This patient recovered, though her convalescence was slow.

He thought, too (and this point was mentioned by Dr. Herbert Spencer) that the most important point of all was to foresee obstructed labour, partly by timely and frequent examination of women in their pregnancies, and, better still, by the proper tuition of students and midwives, so that they might recognize pelvic contraction or malpresentation before labour came on, or before the hopeless obstruction occurred.

Finally, he was one of those who thought that the operations of symphysiotomy and pubiotomy had only a very limited field in obstetrics, and in really septic cases he would not perform them at all.

With others he sincerely congratulated Dr. Amand Routh on the admirable paper he had brought before them for discussion.

Dr. T. W. Eden remarked that, as the twentieth speaker in the debate, it would take more ingenuity than he possessed to say anything new on the subject. He had twice performed craniotomy on the living child in a septic uterus, and he expected to have to do it again some day. Despite the stigmatization by the Rotunda Hospital authorities, in Dublin, of craniotomy as a criminal offence, he did not modify his opinion. Any abdominal operation under such conditions must be extremely dangerous, and it was foolish to kill the goose which laid the golden eggs! With regard to the septic cases, most of the speakers took the view that the danger in the Cæsarean operation was that the peritoneal cavity might be infected by the liquor amnii. He agreed that that was a factor, but he did not think it the principal one. The chief risk was in the infected condition of the uterus itself. In most cases he supposed it was an ascending infection beginning in the vagina or in the cervix. The lower uterine segment had been stripped of its membranes, and if infection reached the vaginal mucous membrane or the cervical canal, it must spread by continuity to the tissues of the lower uterine segment. Possibly in some cases the infection might be from the beginning intra-amniotic, but one did not know how long it would be limited there, for the amniotic membrane was a very delicate one, and permitted absorption rapidly. Infection of the peritoneum by liquor amnii could surely be got over by careful technique. In abdominal surgery one was dealing day by day with areas of infection through transperitoneal operations, and by careful technique they were got through without disaster. Surely the same should be true of the cases now under discussion. He would eventrate the uterus first, carefully packing off the abdominal cavity above and below with sterilized towels and swabs, including the edges of the abdominal incision. It would then be possible to open the uterus and evacuate the contents, and sew it up again, without anything coming into contact with the peritoneal cavity at all. And if the surface of the uterus were irrigated before returning it to the abdomen, he did not see why there should be peritoneal infection. Dr. Roberts regarded his case in which a utero-parietal fistula developed as a support to the view that the peritoneum had become infected by liquor amnii. But surely the utero-parietal fistula was a more likely result of septic infection of the uterus than of infection of the peritoneum. If the uterus was septic, the uterine incision would not heal, and the septic matter would pass into the peritoneal cavity, and thence into the parietal incision, forming a fistula. If from extreme pelvic contraction in a septic case, craniotomy was impracticable, and Cæsarean section had to be done, the only safe plan was to remove the uterus. The danger of extraperitoneal Cæsarean section was that the infected uterus was left. After all, the difficulty in practice was to decide which cases were so bad as to come into the category of definitely septic cases, and he suggested to Dr. Routh a rough distinction between those obviously infected and those only suspected. In the two cases in which he performed craniotomy the liquor amnii was offensive, and the patient had a temperature of over 100° F. When both of those two features were present one should conclude there was definite infection of the uterus. Among the suspected cases he would include those in which a long period had elapsed since the liquor amnii had drained away, or in which repeated attempts at delivery had been made by persons who, without intending any offence, he would speak of as unskilled persons, or where the surroundings made asepsis difficult to carry out. With care in technique conservative Cæsarean section should get one safely through. Where there was no reasonable doubt of the presence of infection in a case, and the abdomen was opened, the uterus should be removed, and personally he would prefer a supravaginal operation, providing for free drainage, both suprapubic and vaginal.

Dr. SAM. CAMERON found it difficult adequately to express his admiration for the successful manner in which Dr. Routh had accomplished his arduous task. Dr. Routh's contribution would become classical. He (Dr. Cameron) thought that very frequently it was impossible to state whether a case was infected or not. Even if a case were infected there was no reason why Cæsarean section should not be performed. He could not agree with Dr. Eden that the existence of a foul-smelling discharge was an indication for craniotomy. The worst cases of septicæmia were those in which the discharge was not offensive. The presence of a foul-smelling discharge might indicate infection by putrefactive bacteria which did not seriously endanger life. He would not even attach any great importance to the presence of streptococci in the amniotic fluid, because the presence of such organisms did not necessarily mean that the performance of Cæsarean section would cause the case to terminate disastrously. The recognition of micro-organisms necessitated the presence of a skilled bacteriologist, and usually the cases did not permit of delay. Moreover, it was impossible to foretell the virulence of the organism, as the same organism might affect different patients in very different ways. Therefore he would not allow the presence of streptococci to influence his operative procedure. He had been guided almost entirely by the general condition of the patient. When a case, however, had been repeatedly examined before her admission to hospital he removed the uterus by supravaginal hysterectomy. So long as a woman was not moribund, and the child was still alive, he performed Cæsarean section, but never craniotomy.

As regards the results which were being obtained, he might mention that in the new Glasgow Maternity and Women's Hospital, Dr. Russell had performed thirty-three cases without a death. Dr. Murdoch Cameron and himself had performed Cæsarean section in twenty-one cases of contracted pelvis in the new hospital, and thus, in the wards of this institution, Cæsarean section had been performed in fifty-four successive cases without a death. In at least five of Dr. Cameron's cases, the cases had been tampered with before their admission to hospital. In one case the child's face had been abraded with the forceps, and yet the patient, after supravaginal hysterectomy, made an uninterrupted recovery. These results seemed to show that craniotomy should be discouraged.

In cases where repeated examinations had been made, or where forceps had been applied, Dr. Cameron's technique was briefly as follows: After the abdominal cavity had been opened into, the uterus was dragged into the wound. Large swabs were then introduced into the cavity, and the lower portion of the uterus was encircled with towels so that the fluid might not escape into the peritoneal cavity. When the uterus had been emptied a large swab was thrust into the uterine cavity and the edges of the uterine incision were covered with gauze. Supravaginal hysterectomy was then performed.

In future he intended to introduce the lactic acid bacillus into the vagina when dealing with septic cases, as he had obtained fairly satisfactory results in gonorrhœal cases from this method of treatment. In his opinion supravaginal hysterectomy should be preferred to panhysterectomy, and he questioned if the introduction of gauze into the vagina in order to obtain drainage was of any benefit whatsoever. He did not feel inclined to perform extraperitoneal Cæsarean section, because it took a longer time to perform. Consequently the uterine wound was exposed to the air for a considerable time, and, in addition, the surrounding cellular tissue was liable to get bruised during the various manipulations. Again, in septic cases, a septic organ was allowed to remain in the body when its removal would be an advantage. In conclusion, Dr. Cameron stated that it was his intention to continue performing Cæsarean section in preference to craniotomy in all cases where the mother was not moribund and the child was alive, as he was strongly of opinion that the application of forceps by a practitioner, or even the presence of organisms in the liquor amnii, did not justify the destruction of a living child.

The PRESIDENT (Dr. Macnaughton-Jones) said that it would be difficult in a few words to convey to Dr. Routh the appreciation of the Fellows, not only those who had taken part in the debate, but also of those who had not joined in it. He (the President) was both pleased and proud to be the medium of conveying to him the warm admiration of the Section for his most exhaustive and valuable communication. As reporter on this subject at the International Gynæcological Congress at St. Petersburg, as seen by his paper in the current *Journal of Obstetrics and Gynæcology*,[1] Dr. Routh had most worthily represented this Section.

The main questions on which this discussion hinged were, to his mind, sepsis—a living or dead child—uselessness or inadvisability of trying recognized obstetrical methods—the child's life as against the mother's. He had barely time to say a few words embracing these four points.

He disliked this term "suspect," introduced into obstetrics, he presumed, from that used for criminals so classed. The mere fact of examinations having been made, or an attempt at delivery having failed, did not in the vast majority of cases, unless reckless or wholly unwarrantable practice had been pursued, justify the conclusion that a woman was threatened either with sapræmia or septicæmia. If such a practice had been pursued, she was in all probability no longer a suspect, but an actual septic victim to wrong treatment. The various opinions expressed by the bacteriologists who had spoken only showed that they were not yet on sufficiently firm ground to rely on bacteriological examination to determine this. He would not himself be influenced by a hurried microscopical report, without culture, as to the course he ought to pursue, whether that report were positive or negative. Assuming that a skilled bacteriologist—and the opinion of none else would be of any value—assured him that he found pathogenic germs in the microscopical field, he would be influenced far more by the clinical signs and symptoms present, though such a report would be confirmatory of his decision as to the course he should pursue. Sir Almroth Wright cautiously used the expression "might be possible to detect," in his reply to Dr. Amand Routh as to the value to be placed on bacteriological examination, and the latter himself said, "With our imperfect knowledge in the presence of both virulent and putrefactive organisms one would have to treat the case from the point of view of the most virulent germs."

Were they not rather inclined to make too great a bogey of this "suspect" sepsis in the face of their modern aseptic and antiseptic possibilities? He said this, regarding it both from the point of view of treatment and operation, no matter what method of delivery was adopted. Dr. Maxwell's plan of attempting to sterilize the amniotic fluid was valuable, and, in view of operation, was an important advance. He might here suggest that one of the most valuable bactericides was cyllin. It was quite innocuous in the proportion of one in four hundred, which, in warm solution, was destructive to the *Bacillus coli*, *Staphylococcus aureus*, and the streptococcus. This would indicate its free use both locally to the parts, in the vagina, and its injection by Dr. Maxwell's method.

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[1] *Journ. of Obstet. and Gynæc. Brit. Empire*, 1911, xix, pp. 1-233.

Accepting the presence of a local sapræmia, or a commencing systemic sepsis, and that the case was then seen for the first time, at any stage of a labour in which delivery by any other than operative methods was impossible, they were at once brought face to face with the remaining question, Was the child living or dead, and was the performance of Cæsarean section, or one of its alternatives, the right course to pursue? He presumed that they were all fairly agreed that if the child was dead, whether under "suspected" or recognizable septic conditions, cephalotripsy or craniotomy was the correct one. Then arose the question of a living child. Personally, in view of modern asepsis and improved technique, he would favour in those cases in which the nature and degree of the septic invasion did not contra-indicate it, *the patient being surrounded by the precautions of a hospital technique*—Cæsarean section. The choice of the particular method must lie with the individual operator and his experience of its results. In cases where the only alternative was hysterectomy, and a choice had to be made between the supra-vaginal operation and panhysterectomy, *cæteris paribus*, he would prefer panhysterectomy, though here again the decision must depend on the local condition.

As regards consideration for the child's life as against that of the mother, when forced to make a choice he would unhesitatingly move in the direction of the mother's safety rather than that of the child. The wishes of the woman, however, must not altogether be ignored; her anxiety for maternity, on the one hand, her distinct disinclination for operation, on the other. During nineteen years of public midwifery work, both in the Poor-Law service and ten years on the staff of the Cork Maternity Hospital, he had lost but one mother with the child from contracted pelvis, and here the fatal issue was unavoidable from previous prolonged interference on the part of an ignorant midwife. He was absolutely certain that he had not performed cephalotripsy or craniotomy more than twice on a living child, and once he had to perform embryotomy in the case of a small dwarf. The mother recovered.

Tempora mutantur nos et mutamur in illis. It was possible that he would now remove such cases to hospital and perform Cæsarean section.

Dr. Routh remarked in his paper how doubtful, or even hopeless, and also how divergent, many of the opinions were that were expressed at the International Congress with reference to Cæsarean section or its alternative. Professor Bumm specified four conditions under which craniotomy was indicated, viz. :—

(1) "That in primiparæ, when infection is undoubtedly present, craniotomy is the best and surest treatment, for by it one life already compromised is lost; whereas, by all other methods, two lives would probably be sacrificed."

(2) "If the amnion has been ruptured for some time and there is even only a slight elevation of temperature, classic Cæsarean section is fatal."

(3) "In all cases of bacteria of any kind we object to the section, and make the perforation of the child."

(4) "When bacteria are found in fever cases, we perforate the child."

Pestalozza, in definitely infected cases, decides that embryotomy should be performed.

Sellheim, in similar cases, advises embryotomy.

Munro Kerr, who, he regretted, was not able to be present that night, had stated[1]: "In recent years I have made it a rule never to perform Cæsarean section in cases which have been interfered with prior to their coming under my care unless the pelvic deformity is so extreme as to render craniotomy impossible or more dangerous to the mother. I am compelled, therefore, not infrequently to perforate a living child."

Dr. Routh himself said: "Extraperitoneal Cæsarean section and all forms of pelviotomy are better avoided for septic cases, and as classical Cæsarean section has a death-rate of from 10·8 to 34·3 per cent., there remains, therefore, only craniotomy or some variety of Cæsarean hysterectomy." Also in two other types of case he advised craniotomy.

Jardine, Whitridge Williams, and Singer were also quoted by him as regarding craniotomy as the operation of election in the presence of sepsis when it could be performed. In the face of such opinions, he did not think that anyone was justified in applying the term "criminal" in connexion with craniotomy, nor could one yet regard it, as described by the distinguished Master of the Rotunda in this debate, as one of the "obsolete barbarities of the past." Such an extreme position was, he maintained, not justifiable, and he did not consider it would be right to allow it to pass unchallenged by the Section.

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[1] "Operative Midwifery,' 1908, p. 404.

Dr. AMAND ROUTH, in reply, after thanking his hearers for the kind reception of his paper, spoke of the great difficulty in clinically deciding whether one of these "suspect cases" was infected. A general infection might be present and yet might not declare itself till a rigor occurred on the third day. In any case, infection would occur via the genital passages, and as there was no sure way of recognizing early infection clinically, he had strong hopes that a bacteriological examination of the liquor amnii would be sufficiently reliable to decide the point.

Assuming that the liquor amnii was infected, and ordinary Cæsarean section was performed, death from peritonitis would very likely occur in three or four days, or general septicæmia might lead to a fatal result in six or seven days. Peritonitis in these cases resulted from escape of infected liquor amnii into the peritoneal cavity at the time of the operation, or to failure of primary union of the uterine wound and subsequent escape of infected lochia, or to infection of the uterine sutures. He thought that Cæsarean section, preceded by intra-amniotic irrigation and eventration of the uterus, with careful protective packing round the uterine neck and a fundal uterine incision, would suffice to protect the patient if the liquor amnii was alone infected. If, however, the uterus itself was infected, nothing but a hysterectomy would suffice. As to whether a panhysterectomy, which removed a possibly infected cervix, was better than a subtotal hysterectomy, further experience was needed. In his collected cases, fourteen "suspect" cases were treated by subtotal hysterectomy with intraperitoneal treatment of the stump without mortality, whilst 216 similar cases were treated by classical Cæsarean section alone, with a mortality of 17 per cent. In his collected cases panhysterectomy had only been done once (by Donald, of Manchester, in 1897) in cases of contracted pelvis, and this case was not apparently "suspect." No evidence, therefore, was at present available except by the analogy of the operation in cases of cancer of the cervix obstructing labour.

It was satisfactory to find that no one advised extraperitoneal Cæsarean section or pubiotomy for definitely septic cases, and only very few would thus operate in "suspect cases." Dr. Wallace's extraperitoneal operation, effected by suturing the uterus to the parietal peritoneum round the abdominal incision before opening the uterus, was practically identical with Veit's modification of Sellheim's utero-abdominal fistula operation. Undoubtedly Dr. Wallace owed the survival of his patients to the peritoneal cavity having been shut off, but there were safer ways of treating such cases.

Craniotomy, though greatly disliked by all, was still occasionally required, and to pretend to consider it a criminal act, as Dr. Hastings Tweedy called it, was to put sentiment before the realities of science. He hoped Dr. Maxwell's intra-amniotic irrigation would be another step towards saving "suspect cases" from having their children thus destroyed, for it would enable Cæsarean section, properly safeguarded, to be adopted. The risk of infection through the abraded areas which must occur if the child was artificially extracted after the craniotomy would be reduced to a minimum if spontaneous delivery, as advised in some cases by Sir Francis Champneys, were adopted instead of forcible extraction.

Dr. Tweedy had said how humiliating it was to see in Dr. Routh's collected cases how many cases of Cæsarean section had been previously treated by craniotomy. He thought, on the other hand, it was an evidence of progress of which they might be proud, and he showed that the increasing safety of Cæsarean section and its alternatives was gradually but surely preventing craniotomy. He had taken at haphazard one year, 1908, and he found that out of 142 cases of contracted pelvis, twenty-two women, whose thirty-one children had been previously delivered by craniotomy, were in that year safely delivered of living children by Cæsarean section. At Queen Charlotte's Hospital the saving of fœtal life in the last ten years as compared with the preceding ten years, by the adoption of Cæsarean section and late induction of premature labour, instead of craniotomy and early induction, represented 36 per cent.

Yet craniotomy could not be entirely dispensed with, especially in general practice, where prompt bacteriological assistance and skilled technique in abdominal surgery might not be available.

In spite of the fact that some speakers had said that bacteriology could not yet be an efficient guide as to the presence and significance of the infective germs, he had great hopes that in the future such help would be forthcoming, and that further improvement in differential staining and other methods would enable the obstetrician to know the extent of his patient's infection, and guide him as to the operation he should perform, and possibly also assist him by in some way strengthening the resistance of the patient. He hoped obstetricians would help by having the liquor amnii examined bacteriologically in all "suspect cases," and at the same time make a note of the operation performed and its results in every case. In time the evidence thus collected would enable the treatment of these cases to be put on a real scientific basis.

Obstetrical and Gynæcological Section.

March 2, 1911.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

Thyroid Tissue in an Ovarian Embryoma.

By R. Drummond Maxwell, M.D.

This specimen, a small ovarian cyst on the right side, was removed from a patient aged 47. The patient's symptoms brought her to the Outpatient Department of the Samaritan Hospital complaining of pelvic pain, and a cyst the size of a tangerine orange was discovered on making an internal examination. There was no special tenderness on examination, and the cyst could be easily displaced from the pelvic cavity, so an expectant attitude was adopted for several months. The patient became markedly neurasthenic with exaggeration of menopause symptoms; therefore removal of the cyst was decided on. This was performed, the other ovary being noted as normal. On examination of the external aspect of the cyst a solid brownish-red nodule was seen projecting on its surface, the size of a grape, and was taken to represent unaltered ovarian tissue, a small portion of which had undergone cystic change. An incision through the nodule presented such remarkable macroscopic resemblance to thyroid structure that sections were taken of it which proved its identity with normal thyroid gland structure. The remaining cyst contents were clear viscous fluid containing no other evidence of a teratomatous nature. There appeared to be no abnormality in connexion with the thyroid gland in the neck, and the patient made a good recovery. She was seen for several months in the Out-patient Department, with no amelioration however of her menopausic symptoms of flushes and headaches. This condition, on later inquiry, had progressed unfavourably and led to her admission to a lunatic asylum for dementia.

The late Dr. Hamilton Bell[1] described, in June, 1905, two instances of this rarely demonstrated type of ovarian growth, though the condition is probably a good deal more common than would be inferred from the small number of cases shown. Two microphotographs accompany Dr. Bell's article, and would serve admirably to record the microscopic features of the present tumour. In addition the colloid stain used in the sections gives results identical with those of normal thyroid colloid substance stained with the same reagent. In one of Dr. Bell's cases the patient had no abnormal change in connexion with the thyroid gland as in this case. The other, however, had a thyroid enlargement on the right side of the neck suggestive of a parenchymatous goitre. The view taken by Dr. Bell was that both his cases represented colloid degeneration of an ordinary cystic adenoma of the ovary.

The sections from Dr. Bell's specimens were submitted to the Pathological Committee, who reported on the identity of the ovarian growth with normal thyroid tissue, but were unable to express an opinion as to its origin. Of the various suggestions raised in explanation of this tissue in an ovarian cyst, viz. (*a*) a metastatic deposit from a normal or cancerous thyroid, (*b*) a teratoma, (*c*) an endothelioma, or (*d*) a peculiar degeneration in the walls of an adenomatous ovarian cyst, I have no difficulty in accepting the teratomatous explanation even in the absence of other epithelial or mesoblastic structures. A diverticulum from an epithelial surface results in the most typical feature of the ordinary teratoma, the sebaceous gland, and in the same way I would suggest that a diverticulum from a hypoblastic surface will at times produce true thyroid gland tissue.

Report of Pathology Committee.—The Committee reported that they were of opinion that Dr. Maxwell's description of the tumour as thyroid tissue in an ovarian embryoma was correct.

Dr. HERBERT SPENCER had seen thyroid tissue in ovarian tumours on several occasions. He had recently removed a teratoma containing a thyroid gland. It would be interesting to see the effect of the removal of thyroid tissue in ovarian tumours upon the patient, and upon the normally situated thyroid. He was at present making inquiries in this direction.

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[1] *Journ. of Obstet. and Gyn. of Brit. Emp.*, 1905, viii, p. 92.

Clinical History of a Case of Involution of an Ovarian Cyst, associated with Vesicular Mole.

By R. Drummond Maxwell, M.D.

Under this heading I refer to the occasional disappearance of lutein cysts in the ovary when associated with vesicular degeneration of the chorion. Two cases of hydatidiform mole lately came under my notice almost simultaneously at the Samaritan Hospital.

In one patient, aged 38, multipara, after three and a half months' amenorrhœa, spontaneous expulsion took place at the patient's home, and although it was decided to admit the patient to hospital for exploration, and complete the evacuation of her uterus, when this was carried out neither the finger nor light curettage brought away any macroscopic evidence of the disease. Examination under anæsthesia revealed no notable enlargement of either ovary, both of which were identified by bimanual examination.

In the second case, the patient was a young primigravida, single, and aged 23. The only ætiological facts discovered were that the male and female parents stood to each other in the relation of uncle and niece, and there was a disparity of over thirty years in age between them. Here again, after amenorrhœa for twelve weeks, spontaneous expulsion took place at the patient's home. Exploration of the uterus at the hospital within twenty-four hours brought away only a few small vesicles, probably lying loose in the cavity, so completely had the uterus emptied itself. Under anæsthesia no sign of ovarian enlargement was detected, but I attribute this to the mobility of the cyst, which was probably lying well above the umbilicus. On the third day the hypogastrium was examined, and, although all bleeding had ceased, it was found that a tumour occupied this region up to within 2 in. of the navel. Bimanual examination showed at once that this was not due to uterine enlargement, but was a large right-sided ovarian cyst, the size of a fœtal head, which could be displaced from the middle line and freely pushed into either flank and above the navel. A small nodular, movable mass felt in the left posterior quadrant was almost certainly a corresponding left ovarian enlargement, the uterus, well contracted, being felt in the normal position. My colleague, Dr. Cuthbert Lockyer, gave me the benefit of his special experience in this subject, and agreed with me that an expectant attitude was indicated, though the excessive mobility of the cyst, as predisposing to twisting, gave me some anxiety. The remarkably rapid disappearance of the larger and smaller cysts impressed me very much. In three weeks' time no trace of the larger cyst could be detected at all. The smaller left-sided cyst I never felt again. I am certain this disappearance was not due to rupture, since I examined the patient about six times in the three weeks, and was able to appreciate the progressive diminution in size. The patient had been seen three or four times since in the Outpatient Department, and remains in good health, eight months after she left hospital. Seen one month ago and examined *per rectum*, the uterus is normal in size, and no ovarian enlargement detected.

Reference to the literature shows that not many cases of cyst involution have been recorded. Goullioud [3] reports a case in the *Revue de Gynécologie et Chirurgie abdominale*. Mr. Darwall Smith[1] has abstracted this report in the *Journal of Obstetrics*. Dr. Russell Andrews [1], in February, 1909, reported in this Section four cases of association of corpus luteum cysts with hydatidiform mole. His cases are interesting from the point of view of a conservative attitude, for in two of the cases twisting occurred, and in another suppuration, which forced him to operate. In the fourth case no such complications were present, and the cysts disappeared under observation with no ill-effect. Based on this last case, Dr. Andrews formulated the view, which was accepted by the meeting, that "smaller (lutein) tumours need not be removed unless twisted or suppurating." My case would prove that an enlargement the size of a fœtal head will similarly involute, and that rapidly. Fraenkel [2], in August, 1910,[2] records a case of his own, with bilateral ovarian enlargement, where one tumour actually twisted, but operation was postponed owing to pulmonary signs suggestive of chorio-epithelioma. These signs cleared up, and four months later no trace of either enlargement of the ovaries could be detected. To this record he adds those of three others—Goullioud's cases quoted above, one subsequently becoming pregnant, and one case each by Albert and Kroemer. There is no record of Dr. Russell Andrews's case, quoted in our *Proceedings* eighteen months before, which shows that Fraenkel does not pay us the reciprocal compliment of reading English literature on the subject.

REFERENCES.

[1] ANDREWS, RUSSELL. *Proc. Roy. Soc. Med.*, 1909, ii (Obstet. Sect.), p. 196.
[2] FRAENKEL. *Monatschr. f. Geburts u. Gyn.*, Berl., 1910, xxxii, p. 180.
[3] GOULLIOUD. *Rev. de Gyn. et Chir. abdom.*, Par., 1907, xi, p. 17.

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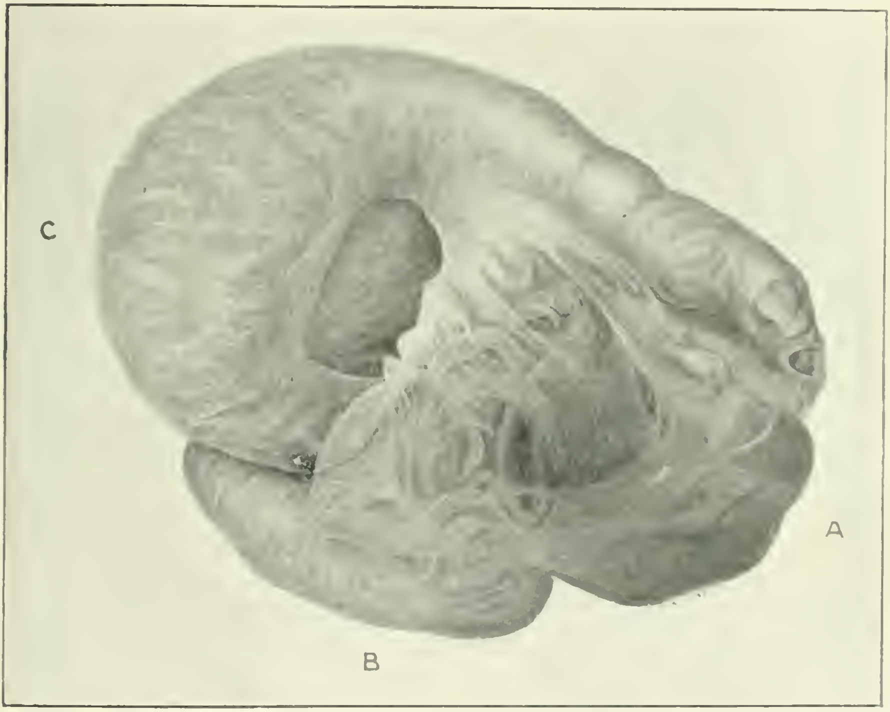
[1] *Journ. Obstet. and Gyn. Brit. Emp.*, 1907, xii, p. 328.

[2] *Brit. Med. Journ.*, January 14, 1911 (Epitome), p. 7.

Hydrosalpinx with Ovarian and Peritoneal Cysts.

By H. Macnaughton-Jones, M.D.

The adnexal mass was removed from a patient, aged 28. There had been one pregnancy five years previously, one year after which she had been curetted. There was an obscure history of "an abscess bursting" three years back, followed by a uterine discharge and persistent pain. On removal, the intermediate and dependent cyst was filled with a bright orange-coloured fluid. The ovarian cyst contained dark sanguineous fluid.



Hydrosalpinx (C) with ovarian (A) and peritoneal (B) cysts.

The description of the specimen is given in the subjoined report by Dr. Cuthbert Lockyer, which also explains his view of the formation of the peritoneal cyst. Dr. Macnaughton-Jones showed the specimen, as it was the only one of the kind that he had met with :—

" The specimen represents the *left* uterine appendages, and consists of a retort-shaped hydrosalpinx, a semicystic ovary, and a serous cyst. The tube is distended to form a thin-walled clear sac. Its uterine end is undilated for the distance of 1 in. Its abdominal ostium is not apparent owing to the dilated end of the retort-shaped swelling being adherent to the ovary and to an intervening serous cyst. The hydrosalpinx has a smooth peritoneal coat, and is devoid of adhesions on its anterior, posterior, and convex upper surfaces. The ovary is displaced inwards, and lies under the uterine end of the hydrosalpinx. It presents a thin-walled, unilocular peripheral cyst which hangs down in a loose, dependent fashion from the solid hilum. To the latter is attached the stump of the ovarian ligament. The serous cyst mentioned above lies between the hydrosalpinx and the cystic ovary, and behind the mesosalpinx. It represents a perimetric cystoma, its outline being completed by adhesions due to perimetritis. In the recent state it was distended with orange-coloured fluid. The colour was due to its proximity to a blood-cyst in the ovary. The hydrosalpinx measures 5 in. from its bulbous extremity to its cut uterine end. Its greatest circumference measures 7 in. The ovary measures 2½ in. by 2 in. in its maximum diameters. The serous cyst interposed between the ovary and cystic tube measures 3½ in. from above downwards, and 2 in. in width at its dependent part, where it projects for a distance of 1 in. below the level of the rest of the specimen."

Mr. ALBAN DORAN suspected that the peritoneal cyst was lymphatic. As a rule, the big yellow blebs seen in the broad ligament associated with uterine fibromyoma drained away when the broad ligament was divided, but he had operated on cases where this did not occur, the lymphatic spaces or ducts communicating with the collection of lymph being closed by inflammatory changes.

Hysterectomy followed by Double Phlebitis.

By H. Macnaughton-Jones, M.D.

This specimen was brought before the Section principally for its clinical interest. It was a multi-nucleated myoma, removed from a patient aged 45, a nullipara. She had suffered for some years from severe hæmorrhages, for which she had been curetted a few years previously. At the time of operation she was greatly emaciated and extremely anæmic, and altogether in a very unfavourable condition. The tumour was delivered with great difficulty, being firmly jammed in the pelvic cavity. On the fifth day proctoclysis was commenced, and as there was some distension of the abdomen the wound was opened, and a suprapubic drain inserted. Exploration revealed nothing, and only some serous fluid escaped. The proctoclysis was continued on and off for five days, but the suprapubic opening was not closed for ten days. The first symptoms of phlebitis appeared in the left femoral region on the thirteenth day after operation, and the swelling extended to the entire limb. It had returned to its normal size by the twenty-fifth day. On the thirtieth the right thigh commenced to swell, and followed the same course as the left, being the more swollen of the two. The swelling subsided completely in about ten days, and from that date there were no untoward symptoms. The patient has since done well.

The important lesson to be learned from the case—and one that was often overlooked—was the increased risk run from grave anæmia, the result of bleeding from fibroids, and also the unfavourable conditions consequent upon this complication, which increase the operative mortality. Whatever be the precise physiological cause, there was no doubt that the condition of the blood in such cases tended to thrombosis; also the possible effect of traumatism in the forcible delivery of these tightly wedged pelvic tumours has to be remembered.

In this case, as generally, the left leg was the one first affected, and it was rather remarkable that the attack in the right did not begin until the thirtieth day after operation. In Kelly's "Operative Gynæcology" (the 1898 edition), out of some 1,100 operations, he had only had double phlebitis in both legs in one instance, and in his classical work on

"Gynæcology and Abdominal Surgery" (1907), written with Noble,[1] out of 7,130 women operated upon, thrombosis occurred in forty-eight. Of these, 727 had fibroid tumours, and nineteen of the forty-eight cases resulted from operations on fibroids.

The therapeutic points of interest in the case were the good results following the proctoclysis with suprapubic drainage as recommended by J. B. Murphy, and the benefit obtained on two occasions when the bowel was obstinate from subcutaneous injections of infundibulin (pituitary extract).

It may be interesting to note that in this, as in other cases in which I had to deal with extreme anæmia, the patient was kept on "Carnine"; in the present instance for seventeen days, in addition to other nourishment.

DISCUSSION.

Dr. AMAND ROUTH was glad the President had brought forward this case. He thought there were two, if not three, varieties of post-operative phlebitis. One form occurs within two or three days of the operation, and is due to the loss of blood which had preceded the operation, leading to hydræmia and excess of fibrin ferment associated with co-existing varicosity of the veins caused by the pressure of the fibroid tumour. This variety causes little or no pyrexia. The other variety comes on at the end of the second or third week after operation, is sometimes ushered in by a rigor as in true phlegmasia dolens, where the lymphatics are mainly involved, and is probably either the result of infection from a germ with a long incubation period, or due to an accumulated toxæmia, the toxic products circulating in the blood-stream, and becoming stationary in the unhealthy femoral or saphenous veins. The symptoms often come on with much suddenness, and can hardly be due to a thrombus slowly spreading from the uterus or broad ligament via the internal and external iliac veins. Very few of these cases of femoral phlebitis have any preceding pelvic induration. He hoped some of the pathologists of the Obstetrical Section would work on the vexed subject of puerperal and post-operative phlebitis and lymphangitis.

Mr. ALBAN DORAN believed that the danger of thrombosis after hysterectomy on subjects highly anæmic from prolonged hæmorrhages was high. A few years ago he operated on a patient aged 36 in this condition, removing a small fibroid uterus by supravaginal amputation. The patient at the end of a month returned to her home in Lincolnshire. On the thirty-eighth day she had an attack of acute phlebitis in the right leg, followed by phlebitis in the left leg, and death occurred on the forty-second day after the operation. He would like to know if the phlebitis in this case was certainly of septic origin. After oöphorectomy for bleeding fibroids, once extensively practised, phlebitis was frequent.

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[1] "Gynæcology and Abdominal Surgery," Philad. and Lond., 1907, i, p. 674.

Dr. ARTHUR GILES concurred in the President's view that phlebitis was more common after hysterectomy for fibroids, when patients had lost a great deal of blood previously, than it was after other abdominal operations, and he believed that the anæmia was one of the determining factors. In some cases the condition was one of an infected thrombosis, and the fact that in the President's case the swelling of each leg was preluded by a rise of temperature, raised the suggestion whether there might not have been an inflammatory element in that case. He had recently come across a rare complication following hysterectomy for fibroids. About ten days after the operation the patient developed gangrene in one foot, and a few days later in the other foot. The gangrene extended some distance up both legs, and lines of demarcation began to appear. The case was diagnosed as a floating thrombus above the bifurcation of the aorta. There was no question of doing an amputation, as the patient was far too weak, and she was removed to a home for incurables. The operation was done four months ago, but the patient was still alive up to two weeks ago. It was a remarkable comment on the slowness of movement of medical opinion that it should be necessary for the President of the Section to call attention to the fact that prolonged hæmorrhage was an important reason for operating in cases of fibroids, as many operators had urged this point for many years. They were indebted to the President for giving the weight of his official position to the renewed pronouncement on this subject.

A Demonstration on the Origin of the Follicle Cells of the Ovary.

By A. Louise McIlroy, M.D.

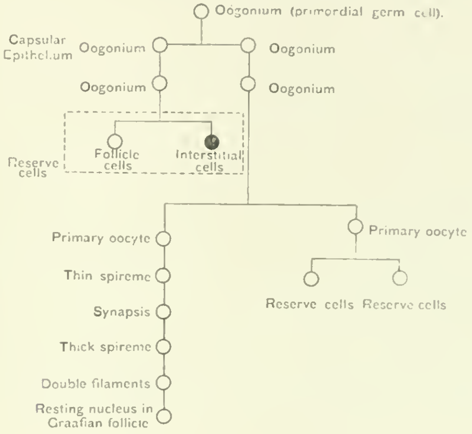
The majority of observers are in agreement as to the origin of the germ cells or ova from the genital ridge, but there is much divergence of opinion as to the origin of the cells of the stratum granulosum or follicle cells. The two theories held are: (1) That the ova and follicle cells have a common origin; (2) that the follicle cells are derived from the stroma cells. The latter view is based upon the spindle-like appearance of the follicle cells, and the fact that many ova are seen to be lying in close contact with the stroma cells. The present investigation was carried out with the object of clearing up this controversial point. The results go to prove that the development of the cells in the ovary takes place according to a fixed rule in all mammalian types. The medullary cords are the first structures to appear in the genital ridge; they atrophy early. All the remaining cells in the ovary are oögonia. The next structure to be differentiated is the surface or *capsular* epithelium—i.e., so-called germinal epithelium. The oögonia multiply by mitosis—somatic division—and form two divisions according to their ultimate function.

The cells of one group of oögonia go through a complicated process of maturation, ultimately becoming oöcytes or ova within a Graafian follicle. These remain in a prolonged resting stage until ovulation and fertilization take place. They divide by heterotypical division.

The other group of cells have two intimate functions—one set of cells remain as reserve cells acting as pabulum for the growing oöcytes. Another set of cells develop further, surround the oöcytes, and form the cells of the stratum granulosum. The remaining cells go to form the interstitial cells of the ovary which are concerned with the internal secretion of that organ. The method of division of the follicle cells is unknown; it is probable it takes place by direct fission. The follicle cells are therefore derived from the oögonia and not from the stroma. The stroma is only supporting and vascular in function, and its growth is mutually correlated with that of the oögonia throughout the development of the ovary. The explanation of the fallacy of the theory of the stroma origin of the follicle cells is that observers have not made use of specimens of ovaries sufficiently early in embryonic life. In the later stages there is a similarity between the appearance of the follicle and the stroma cells.

(The demonstration consists of lantern slides of drawings and photographs of early ovaries—fœtal and post-natal—of the puppy, kitten, pig, and human.)

DIAGRAM OF OÖGENESIS.



A full account of these observations is published in the *Proceedings of the Royal Society of Edinburgh*, 1910, xxxi, pp. 151-78.

In conveying the thanks of the Section to Miss McIlroy for her communication, the PRESIDENT expressed his admiration for the exhaustive research it showed, and for the laborious work which she had carried out in arriving at the conclusions she had brought before them, as also for the series of beautiful microscopical illustrations with which she had elucidated her views.

Primary Abdominal Pregnancy in a Rabbit.

By W. Blair Bell, M.D.

Considerable interest and speculation still surround the ætiology of ectopic pregnancy in the human female. Formerly tubal pregnancy was considered to be the only authenticated form of primary ectopic gestation. More recently, however, the occurrence of primary ovarian pregnancy has been well established by a number of observers; and in the last few years three possibly genuine cases of primary peritoneal implantation (primary abdominal pregnancy) in the human subject have been reported by Galabin [1], Hirst and Knipe [2], and Witthauer [4].

The supposed rarity of ectopic gestation in the lower animals has often been commented upon as indicative of the pathological rather than accidental origin of these deviations from the normal state of affairs in woman. But while *primary* ectopic pregnancies are almost unknown in animals, secondary abdominal pregnancy is quite common, especially in rodents of the genus *Lepus* (hares and rabbits). Such occurrences are due to the rupture of the uterine cornua, with the gradual extrusion of the fœtuses, the placentæ in some instances remaining attached to the original sites, while in other cases the ova have formed fresh attachments to the peritoneum (usually in the neighbourhood of the stomach, owing to the tendency the ovum would have to seek the lowest level). In fact, as far as I can discover, the only genuine cases of ectopic gestation in animals hitherto reported have been of the nature of secondary abdominal implantations subsequent to rupture of the uterine cornua. Many isolated instances of this condition have been recorded, and Kamann [3] in a very complete account of a case occurring in a rabbit gives a full bibliography of the subject. In his own case a single fœtus was found in the abdominal cavity. It was proved to be a case of secondary implantation, for sections through the uterus showed a well-marked point of rupture.

According to Kamann, in none of the other cases recorded in animals (about forty in number) was there sufficient evidence of primary ectopic implantation having occurred. He rightly holds that no case can be considered proved unless serial sections of the genital tract (and I would add of the ovaries) have been made, to exclude the possibility of a healed laceration, such as was found in his case. He concludes by stating his belief that there is no such thing as primary abdominal pregnancy.

I hope to prove that my case was undoubtedly of that nature. If such a thing can occur under physiological conditions in animals it surely can occur in women. There can be no doubt that the majority of cases of tubal pregnancy in woman are physiological rather than pathological—except in the results—being dependent upon the time and position of the ovum when fertilization takes place, and on the activity of the trophoblast. We must remember, too, that in the evolution of the human uterus we have acquired an organ that is probably less accessible to the ovum than is the bicornute uterus of the lower animals.

During the progress of some experiments on the physiology of the female genital organs which I was carrying out in June, 1909, I had occasion to perform the following experiments upon an adult multiparous doe which had been in the laboratory ten days, and during that time had not been near a buck.

(1) The abdomen was opened by a small median incision between the last (inguinal) mammæ. The uterus was examined and found to be active, non-pregnant, and normal. The ovaries were removed. These did not contain any recent corpora lutea evident to the naked eye, such as is always the case in early pregnancy. The abdomen was then closed.

(2) A month later the abdomen was again opened through the old healed scar. There were no adhesions. The uterus was partially atrophied. One horn was ligated, and the abdomen closed.

A month later the rabbit was killed, and a post-mortem examination made. The uterus (the ligated horn being slightly distended) was removed entire, and also the ductless glands. When the abdomen was opened up a large tumour was found in the upper part. On examination in situ it was found to be attached to and depending from the gastro-colic omentum by a broad narrow pedicle. The tumour was free elsewhere.

On macroscopical examination immediately after removal the tumour gave one the impression of being an old encysted blood-clot. At the time, however, I examined it no further myself, but handed it over to Dr. H. E. Annett, Professor of Comparative Pathology in the University of Liverpool. He made three incisions into it, and on discovering fœtal bones kindly returned the specimen to me for further investigation.

When the specimen had shrunk somewhat bones could be seen projecting on the surface (fig. 1).[1] Mr. Thurstan Holland kindly took a skiagram of the specimen, which was found to contain the complete skeletons of four full-term fœtal rabbits (fig. 2). The age was estimated by comparison with a skiagram showing the skeletons of young rabbits immediately after birth.

Histological sections made from various portions of the soft parts of the tumour only showed old blood-clot; no placental structure could be seen. The presence, however, of the undisturbed skeletons was quite sufficient evidence of the nature of the specimen.

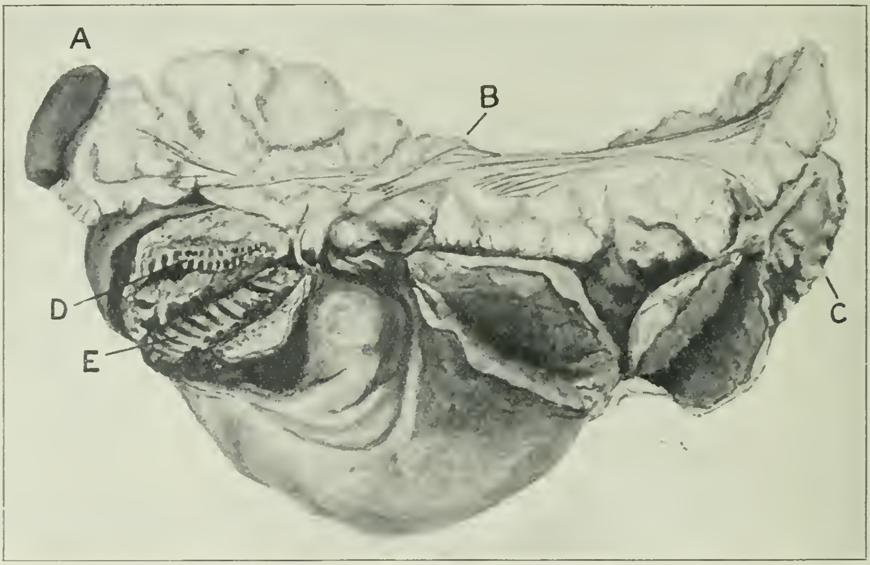


FIG. 1.

The posterior aspect of the specimen removed. It is somewhat shrunken and shows the three incisions made into it. **A**, portion of spleen; **B**, gastro-colic omentum; **C** and **D**, fœtal vertebræ; **E**, fœtal ribs.

It was now necessary to ascertain whether this was a case of primary abdominal implantation or one of secondary abdominal pregnancy. The former, as already stated, has never been definitely proved to occur in animals, while the latter is fairly common. To arrive at a conclusion the ovaries, the Fallopian tubes and the uterine cornua were cut in serial sections. This proceeding occupied a long time, and to lessen the labour of examination only every fifth section was mounted, even then many thousand sections had to be examined. In the ovaries seven fairly recent corpora lutea of the same date were found, two in one ovary and five in the other, and some degenerating follicles. I believe from my experience of rabbits' ovaries that the former are some months old, and represent the corpora lutea corresponding to the fœtuses found in the abdomen. I may mention that in the ovaries of pregnant rabbits one can usually count the same number of recent pink-coloured corpora lutea as there are fœtuses in the uterine cornua. The degenerating follicles represent a subsequent œstrum without connexion with the buck, for it is believed that in the rabbit Graafian follicles only rupture during copulation—otherwise they degenerate. I have been unable to detect any evidence of recent pregnancy or of rupture of the wall in any part of the uterine horns or Fallopian tubes. It has already been

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[1] The specimen has been presented to the Royal College of Surgeons' Museum.

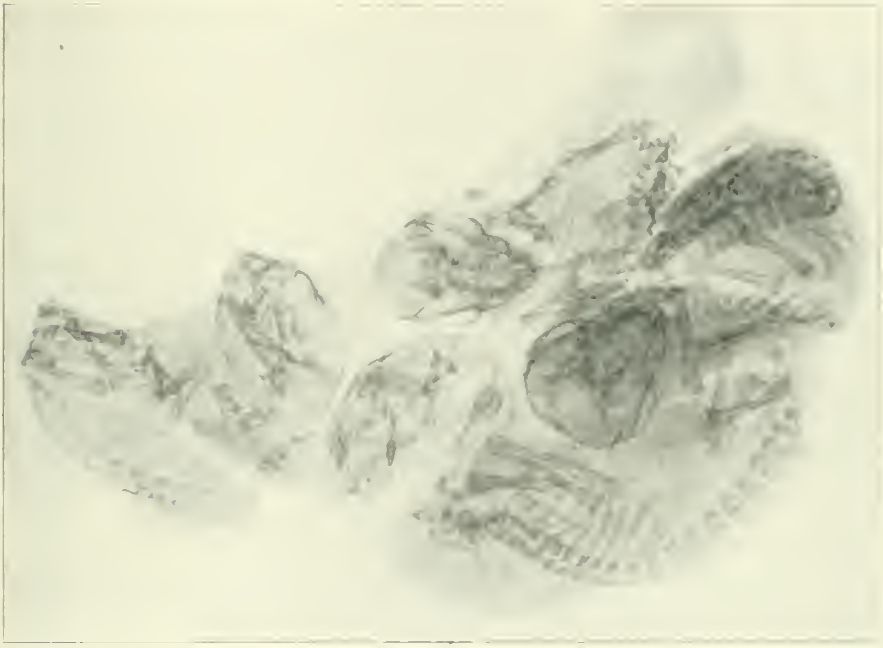

FIG. 2.

Skiagram of specimen after removal (front view). The skeletons of four fœtuses are clearly shown.

mentioned that there was not the slightest macroscopical evidence in the way of a scar or an adhesion. One would not, of course, expect to find evidence so long after of a coincidental intra-uterine gestation, had such a condition existed, but I am sure one would have found evidence of rupture of the cornua had there been any.

In all probability the specimen was several months old, and it is obvious, of course, that the operation could in no way have been responsible for the condition. In case any may be sceptical on general grounds, it may be pointed out that at the time of the operation the uterus was certainly not pregnant, even in the early stages. After a very extensive experience in operations on rabbits in all stages of pregnancy, I feel I could not have been mistaken. Apart from my own observations there are the facts that the rabbit had been in a cage alone for some time before operation, and there were no *recent* corpora lutea in the ovaries—a conclusive point. But even supposing none of these observations had been made, it is still to be remembered that the *ovaries alone* were removed at the first operation, and that this procedure in rabbits leads to death of the ovum and abortion in the early stages of pregnancy. So that even supposing the operation had led to abortion into the peritoneal cavity—and this could not have occurred without leaving signs of rupture of the uterus—implantation and development of the ova would not have taken place owing to the previous removal of the ovaries. However, there is no doubt that the rabbit was not pregnant in the uterus at the time of the first operation, nor were there any signs that the uterus had been recently pregnant, nor that it had ruptured.

It may also be asked how it was that the tumour was not felt in the abdomen at the time of the first operation. I may say it was not discovered *as a tumour* until the whole abdomen was laid open; for to anyone accustomed to dealing with rabbits it is quite a common experience to feel in the abdomen large masses of food in the stomach or fæcal collections in the colon. Whether I felt the tumour or not during the operations I cannot say. If I did, being, as it was, unexposed and away in the upper abdomen it would have made no further impression on my mind than that it was a collection of food or fæces.

I have been quite explicit about all that happened to the rabbit in order to meet any objections that could possibly arise in the minds of those who might otherwise have been sceptical. There is nothing further to add, except that it is remarkable how the *four* fœtuses should be so mixed up together; for in my own experience of a very large number of microscopical sections I have found that the Graafian follicles in rabbits' ovaries—unlike those of bitches—contain only one ovum. At the same time many follicles rupture simultaneously during copulation (and only during copulation) with the buck. One cannot assert that no ova found their way into the uterine horns, as the course of the pregnancy was not observed; and intra-uterine pregnancy may have been coincidental with the abdominal pregnancy. Indeed, the number of corpora lutea seem to point to this conclusion. But it certainly adds considerably to the interest of the case that four fertilized ova should have been implanted together.

It is, of course, quite obvious that the site of implantation is a natural one in the circumstances, for the ova would travel along the curve of the ventral aspect of the abdomen.

The conclusion one must inevitably arrive at from a very careful study of this case is that primary abdominal implantation can occur in the lower animals, and that it cannot therefore be considered unlikely to happen in the human subject.

REFERENCES.

[1] GALABIN. *Trans. Obst. Soc. Lond.* (1896). 1897, xxxviii, p. 91.
[2] HIRST and KNIPE. *Surg., Gyn. and Obst.*, Chicago, vii, p. 456.
[3] KAMANN. *Monatschr. f. Geburts. u. Gyn.*, Berl., 1903, xvii, p. 588.
[4] WITTHAUER. *Zentralbl. f. Gyn.*, Leipz.. 1903, xxvii, p. 136.

Dr. AMAND ROUTH was very glad that Dr. Blair Bell had been able to show the specimen at the Obstetrical Section. In 1898 Dr. Pembrey had shown[1] a somewhat similar specimen from a rabbit, but had subsequently (1904), with Dr. Bellingham Smith,[2] read a short paper on the same case, showing that after all it had not been a case of ectopic gestation but of ruptured uterus. In the discussion in 1898 Mr. Bland-Sutton had given some valuable information on the subject, and stated that there was no evidence of the occurrence of tubal pregnancy in the lower animals except in one doubtful case in a baboon, and he then believed that all the recorded cases were subsequent to rupture of the uterus. Dr. Blair Bell's specimen was believed to be a primary abdominal gestation, no uterine or cornual scar having been found, but it was difficult to prove that the condition was primary. Dr. Galabin's case of supposed primary abdominal gestation[3] in a woman was also not absolutely proved.

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[1] *Trans. Obstet. Soc.* (1898), 1899, xl, p. 253.
[2] *Trans. Obstet. Soc.* (1904), 1905, xlvi, p. 283.
[3] *Trans. Obstet. Soc.* (1896), 1897, xxxviii, p. 88.

The Nature of Hæmatocolpos Fluid, with Deductions as to the Chemical Composition of the Menstrual Discharge and the Cause of its Normal Non-coagulability, also of the Acidity of the Vaginal Secretion; and the Character of the Obstructing Membrane, with its Bearing on the Development of the Vagina; together with a Report of Three Cases of Hæmatocolpos.

By W. Blair Bell, M.D.

Hæmatocolpos is an unusual, if not very rare, condition, and I record the following cases, not so much because of their clinical interest as because of the detailed chemical examination made of the retained secretions in the second and third cases.

Case I.[1]

P. D., aged 18, admitted May 4, 1909. The patient had never menstruated, and complained of severe periodic attacks of pain, and of a gradually increasing swelling in the abdomen. For the last three months the pain had been very severe and of a cramping character for a few hours at regular monthly intervals. There had also been dysuria at these periods. On abdominal examination the patient was found to have a central cystic swelling rising out of the pelvis and reaching to the umbilicus. On the top of this a small hard body could be felt. This was the uterus riding on the top of the distended vagina. The tumour was not tender on palpation. On separating the labia the vaginal orifice was seen to be occluded by a bulging membrane. On careful examination the hymen was plainly visible, stretched round the periphery of this structure.

Operation was performed on May 6. The septum was incised, and about 3 pints of a thick chocolate-coloured fluid evacuated. After evacuation an attempt was made to see or feel the cervix, but it was too high up. The vagina was then washed out with sterile water, and a drainage-tube with gauze wick inserted.

The patient was discharged on May 21. The vagina was still capacious and the cervix out of reach.

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[1] Cases I and II were admitted under the care of my colleague, Dr. Grimsdale, and I am indebted to him for permission to investigate them.

After-history (July, 1910): The patient has menstruated normally since the operation. The vaginal orifice is a circular aperture admitting the finger with difficulty; the cervix is easily reached, and the uterus is normal in position.

CASE II.

C. T., aged 15, was admitted on May 9, 1910. The patient had never menstruated, and was complaining of retention of urine. For five months there had been increasing difficulty in micturition. On abdominal examination, a large oval cystic swelling rising out of the pelvis extended about 2 in. above the umbilicus. On the top of this tumour and to the left of the middle line another small hard swelling (the uterus) could be made out. On separating the labia, a thin bluish membrane was seen bulging at the vaginal orifice. The hymen appeared to be irregularly attached to the surface of this, forming strands reaching to the periphery, where the membrane became tougher.

Operation was performed on May 10, and 4 pints of chocolate-coloured fluid were evacuated. The vagina was then washed out with sterile water. The cervix was so high up that it could neither be seen nor felt. The patient was discharged on May 23.

After-history: Two weeks later the vagina was found to have shrunk considerably, and the cervix could just be felt. Two months later the patient reported that she had had two normal periods, but refused to be examined further.

After this paper had been written Dr. T. W. Eden very kindly sent me material from a third case to investigate, with permission to add the results to my communication. I append the clinical history in Dr. Eden's own words.

CASE III.

A. S., aged 15, was admitted to the Chelsea Hospital for Women on February 7, 1911. For six months previously she had complained of attacks of slight abdominal pain occurring at irregular intervals; these pains had never been severe. On Saturday, February 4, she had an attack of severe pain, chiefly in the lower abdomen; this lasted for twenty-four hours, and was accompanied by complete retention of urine. The catheter was passed, and thereafter the pain was relieved and did not return acutely, although a certain amount of discomfort persisted. On examination, an elastic, ill-defined swelling was felt in the hypogastric region; on the summit of this swelling, and inclined to the left side, was a small solid body recognized as the uterus. The external genitalia were normally developed, with the exception of the hymen. This formed a septum completely obstructing the entrance to the vagina; it was crossed by a well-formed median raphe, and in its anterior part was translucent, showing clearly the dark colour of the retained fluid. On rectal examination, a tense elastic swelling was felt, which practically filled the pelvic cavity.

The hymen was divided by a median incision, and each lateral half clipped away for microscopic examination. About half a pint of dark chocolate-coloured viscid fluid escaped. The vagina was well douched with a large quantity of sterile saline solution until all the retained fluid had been removed. On examination, the external os was found closed, and no sign of dilatation either of the uterine cavity or of the tubes could be detected.

The main points of clinical interest in connexion with these cases are: (1) The large size of the collections in the vaginæ of quite young girls in the first and second cases; and (2) the fact that in none of the three cases was there any attempt at dilatation of the uterus. I have, however, always been of opinion that hæmatometra must be a rare and very late complication of hæmatocolpos.

I am not aware that full examination of the retained secretions in a hæmatocolpos has ever been made. Yet there are many undecided points of interest to be discussed presently on which light is needed, and can only be obtained by such an examination as that which I will now describe.

I regret to say that a chemical and bacteriological examination was not carried out in the first case, for, unfortunately, the material was lost owing to the breakage of the vessel containing it.

EXAMINATION OF THE HÆMATOCOLPOS FLUID.

A histological examination was made in each case. The solid contents consist of red corpuscles in all stages of disintegration, epithelial cells from the uterus and vagina, and above all a very large proportion of living polymorphonuclear leucocytes, such, in fact, as is seen in menstrual blood on the first day of the discharge.

Miss Louise McIlroy, who was present at the operation in Case III, examined some of the fluid, and informed me that the leucocytes were alive, as they readily took up tubercle bacilli. This is what one would expect, as there is a definite diapedesis of living leucocytes during menstruation.

A bacteriological examination made in Cases II and III showed the fluid to be sterile.

A chemical examination (conducted in the Bio-chemical Laboratory in the University of Liverpool) was made in the Cases II and III.

Case II.

The fluid is chocolate-coloured and extremely viscid.

Total solids	10·00 per cent.
Water	90·00 ,,
Total nitrogen	1·43 ,,
,, protein from N	9·00 ,,

Mucin: There is a large quantity present. The amount was not definitely estimated, but will be referred to again presently.

Pigments present are oxyhæmoglobin and a little methæmoglobin.

Fibrin ferment is absent.
Fibrinogen is absent.

Lactic acid is present, the fluid having an acidity to phenolphthalein equivalent to 15 c.c. $\frac{N}{10}$ NaOH per 100 grm. of fluid.

Calcium: 300 grm. of the fluid yielded 0·0529 grm. of CaO. This is equivalent to 0·0126 per cent. of calcium.

Case III.

The fluid, of which only a small quantity is available, is dark red in colour, but not nearly so viscid as the previous specimen (Case II).

Total solids	10·10 per cent.
Ash	0·34 ,,
Water	89·90 ,,
Blood	25·00 ,,
Total nitrogen	1·36 per cent.
,, protein from N	8·50 ,,
Amount of protein from the blood present...	5·00 ,,
Remaining protein (mucin)	3·50 ,,
Solids undetermined	1·60 ,,

Mucin : There is only a small quantity present *(vide infra)*.

Pigments present are oxyhæmoglobin, with a trace of methæmoglobin.

Urea could not be detected.

Fibrin ferment is absent.
Fibrinogen is absent.

Lactic acid is present, but the amount of material is insufficient to estimate the quantity. It is, however, obvious that there is far less than in Case II.

Calcium : There is insufficient material to estimate the amount present.

There are many points of considerable importance to be deduced from these results. It must, of course, not be forgotten that the contents of a hæmatocolpos do not come only from the interior of the uterus. There are also to be taken into account the secretions of the cervix and the transudations, and, in Case II, the secretions of the vagina. So that while important deductions can be made in regard to the nature of the menstrual and other secretions, no absolute quantitative estimates can be made of the composition of the menstrual discharge alone by an examination of the fluid contained in a hæmatocolpos.

To discuss the more important points. It was found that the mucin formed a large proportion of the secretions in Case II. The exact quantity was not estimated, as it would have been a matter of considerable difficulty, but it was roughly calculated to be about 80 per cent. of the solid contents, leaving 20 per cent. of blood. This large quantity of mucin is to be accounted for by the presence of glands in the vagina, to be described later. In Case III the quantity of mucin was much smaller (about 33 per cent. of solid contents).

The absence of hæmatin was perhaps the most interesting point in regard to the blood-pigment.

The protein content was about the same as that of ordinary blood.

The next point is one of very considerable importance, namely, that neither fibrin ferment nor fibrinogen were to be detected. While this chemical work was in progress a paper by Cristae and Denk appeared.[1] In this they gave the results of their experimental inquiry into the non-coagulability of menstrual blood. They also found an absence of fibrin ferment, which they state is retained or destroyed by the endometrium. The importance of this fact clinically is considerable, for it brings us to the conclusion that the formation of intra-uterine clots during menstruation must be due to an abnormal condition of the lining membrane of the uterus, or to a very profuse and rapid flow of blood.

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[1] Cristae and Denk, *Wien. klin. Wochenschr.*, 1910, xxiii, p. 234.

It may be pointed out that post-partum blood clots because it does not pass through the endometrium.

While mentioning the recent paper of Cristae and Denk, I must take exception to their presentment of my views on the relationship of the calcium metabolism to menstruation, which they appear to have but imperfectly understood ; and also to the conflicting results they have obtained in regard to the calcium content of the systemic blood during menstruation, since they relied upon Wright's clotting test for their estimation of the calcium content—a wholly inaccurate and unreliable method for the purpose, for the calcium contained in blood is only one factor, and that a small one, in the clotting process, a point that is too often overlooked. I have never correlated the calcium content in any way with the non-coagulability of menstrual blood.

As I have also found that there is no fibrin ferment or fibrinogen in normal menstrual secretion it may be taken as proved that the absence of fibrin ferment and fibrinogen is the cause of the non-coagulability of menstrual blood, which will clot readily if normal blood be added to it. I hope shortly to publish further researches in regard to this point in connexion with normal and abnormal menstrual discharge.

The next point of interest is the presence, in considerable quantity, of lactic acid. It is common knowledge that Döderlein[1] has attributed the formation of lactic acid in the vagina to the so-called vagina bacillus, but it is clear that, even though certain bacteria can give rise to lactic acid, this cannot be considered the normal method of production in the vagina, for, as already stated, the hæmatocolpos fluid was sterile (although the vagina bacillus was carefully looked for) and yet it contained lactic acid.

The actual method of origin of the lactic acid is therefore still unknown, although it is probably derived from the decomposition of mucin by enzymes without the intervention of bacteria. In this connexion it is interesting to note that the amount of lactic acid present in Case III was much less than in Case II, in which the quantity of mucin also was much greater.

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[1] Döderlein, "Das Scheidensekret," Leipz., 1892.

The quantitative amount of calcium in normal menstrual discharge cannot be easily estimated from an examination of hæmatocolpos fluid because the dilution of the menstrual discharges with mucin and vaginal transudations makes it difficult to estimate accurately how much of the whole consists of uterine menstrual discharge. By my method of calcium estimation[1] the ionizable calcium in the fluid from all these cases was more than double the amount contained in normal systemic blood. Further, the total quantity of calcium in the fluid (0·0126 per cent. from Case II) is almost exactly double that found in normal systemic human blood.[2] Consequently it is obvious that the proportion is far higher, as I have shown in previous communications, in menstrual blood undiluted with mucin from the uterine cavity. The chemical examination of this fluid, therefore, entirely confirms my views in regard to the amount of calcium contained in the uterine excretion (as well as in the other excreta) during menstruation.

EXAMINATION OF THE VAGINAL MEMBRANE.

Case II.—The histological examination of the obstructing membrane gave results of much interest. The sections show that the outer surface of the membrane is covered with stratified squamous epithelium (figs. 1 and 2), which in parts appears to be rolled in such a "rolling in" representing the under surface of one of the strands of hymen (already alluded to) which is normally covered with stratified squamous epithelium on both surfaces. When, however, one examines the inner surface of the obstructing membrane it is seen that the lining epithelium consists of columnar epithelium which dips down and forms glands in many places (figs. 1 and 3). But on closer examination we see that even on this inner surface there are patches of squamous epithelium which appear to merge with the columnar epithelium already mentioned (fig. 3). What is the interpretation of this? I think it is simple. It has been recognized since first pointed out by Matthews Duncan that in these

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[1] See *Brit. Med. Journ.*, 1907, i, p. 920, and 1909, i, pp. 517, 592.

[2] Human blood contains about 48 per cent. corpuscles (Schäfer). The serum contains 0·155 part of CaO per 1,000 parts (Oppenheimer). This is equivalent to 0·1036 part of Ca per 1,000 parts. The corpuscles contain 0·1 part of $Ca_3(PO_4)_2$ per 1,000 parts (Oppenheimer). This is equivalent to 0·04 part of Ca per 1,000 parts. From these figures the amount of calcium in human blood is calculated to be 0·072 part per 1,000, or 0·0072 part per cent. That is to say, human blood contains 0·0072 per cent. Ca as compared with hæmatocolpos fluid which contains 0·0126 per cent. Ca.

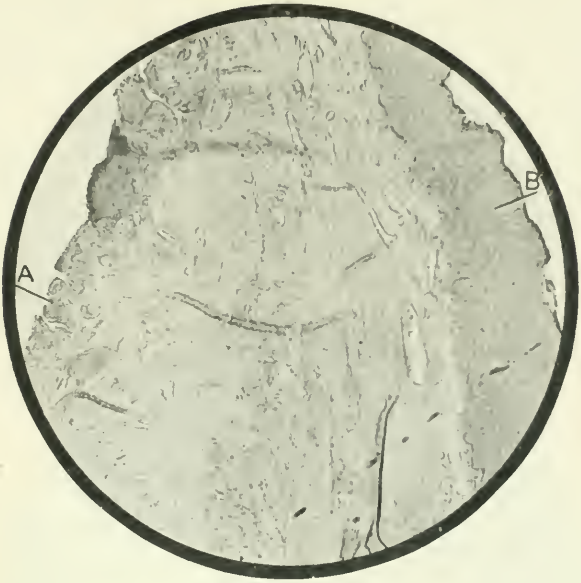

FIG. 1.

Section through the occluding membrane from Case II, showing squamous epithelium on the outer surface (**B**), and columnar epithelium with glands and some squamous epithelium on the inner surface (**A**). (× 75.)

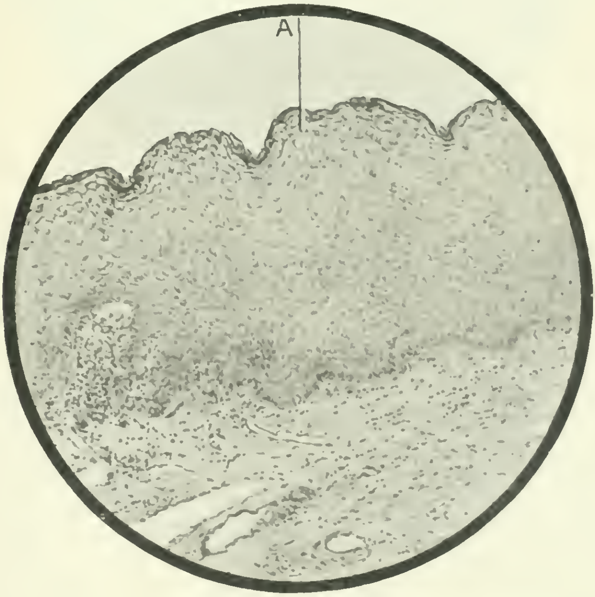

FIG. 2.

Section through the occluding membrane from Case II, showing the outer surface covered with squamous epithelium (**A**). (× 200.)

cases of hæmatocolpos with occlusion of the lower end of the vagina the obstructing membrane is rarely the hymen. It is usually the lower end of the vagina. I have already in a paper read before this Section[1] pointed out how the vagina may be developed in two ways. In one, the commoner, the urogenital sinus forms the lower third, and in the other the vagina is formed entirely from Müllerian downgrowths. Occlusion of the vagina is not likely to occur in those cases in which the urogenital sinus plays no part in the development of the vagina, for in them the mesoblastic columns have further to go, and may fail to reach the surface. This was probably a case of that description, and it is therefore evident why we find the vagina lined with columnar epithelium. One might reasonably expect to find the vaginal tube, which is derived from Müllerian downgrowths, lined with columnar or cubical epithelium, and the conversion into squamous epithelium to be a subsequent change.

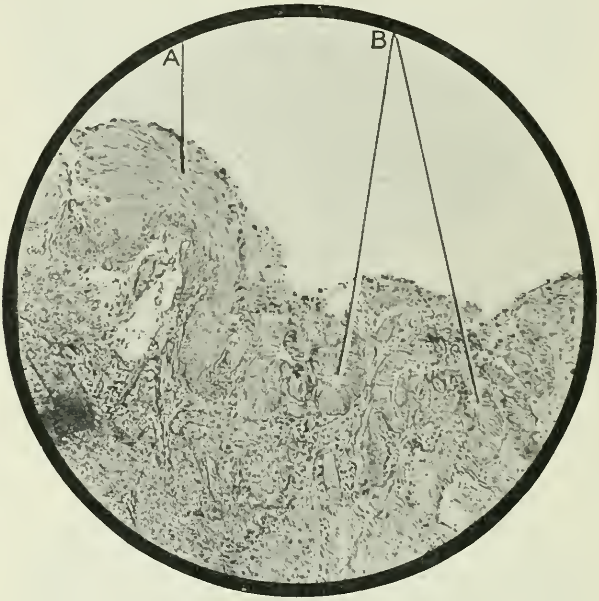

Fig. 3.

Section through the occluding membrane from Case II, showing inner surface covered with squamous (**A**) and columnar epithelium with numerous glands (**B**). (× 200.)

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[1] *Proceedings*, 1909, ii. p. 311.

This I offer as the explanation of the admixture of columnar and squamous epithelium found in sections of this membrane.[1]

Case III.—The obstructing membrane from this case has squamous epithelium on both surfaces (fig. 4). But it will be observed that this forms a much thicker layer on one (? outer) side than on the other. The fact of there being squamous epithelium on both surfaces does not prove it to be an imperforate hymen, indeed, the macroscopical appearances of the membrane as to whether it were surrounded or crossed by hymen would have to be taken into account in coming to a decision. I am of opinion that the thin layer of epithelium may indicate that it is of vaginal origin. In a section of a normal hymen it will be seen the epithelium is of rather more equal depth and similar in arrangement on the two surfaces.

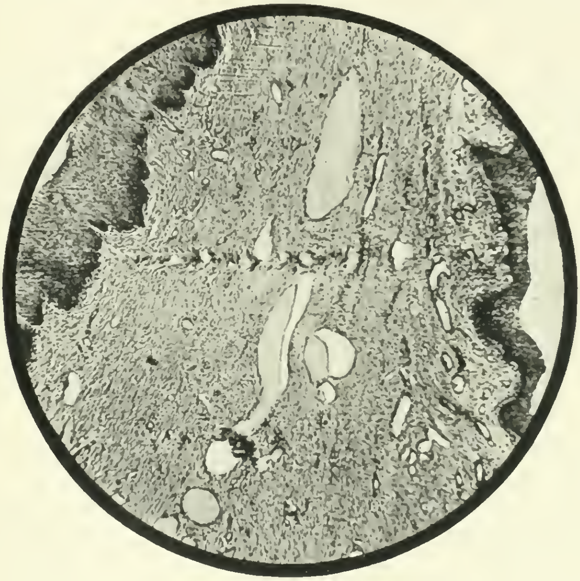

FIG. 4.

Section through the obstructing membrane from Case III. It is covered with squamous epithelium on both surfaces, but there is a thicker layer on one side than on the other. (× 75.)

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[1] Since the writing of this communication, Victor Bonney and Bryden Glendining have published a paper read before this Section on a condition they have designated "Adenomatosis vaginæ" (*Proceedings*, p. 18). The histological findings in their case are of a similar nature, and I think the construction I have put on my observations are applicable to theirs.

The conclusions to be drawn from the foregoing observations are:—

(1) Hæmatometra rarely occurs with hæmatocolpos, and can only be a very late complication.

(2) The non-coagulability of menstrual blood is due to the absence of fibrin ferment and fibrinogen, probably effected by the endometrium.

(3) The lactic acid present is not produced by bacteria.

(4) The calcium content of menstrual discharge is very high.

(5) The vagina is for the most part developed from the Müllerian ducts, and its lining epithelium may therefore occasionally be of a columnar type, and glands be present.

The PRESIDENT said that it was not often the Section was treated in the one evening to two such original communications as those which Dr. Blair Bell had brought before them that night. They were peculiarly interesting, and the Section was much indebted to him for his presence, and for the delightful demonstration he had given them. He (the President) should have mentioned in speaking of one of his cases that it was to Dr. Blair Bell they were specially indebted for their knowledge of the action and use of infundibulin ("pituitary" extract). It was on reading his original paper on the subject that he himself had used it in the first instance.

Obstetrical and Gynæcological Section.

April 6, 1911.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

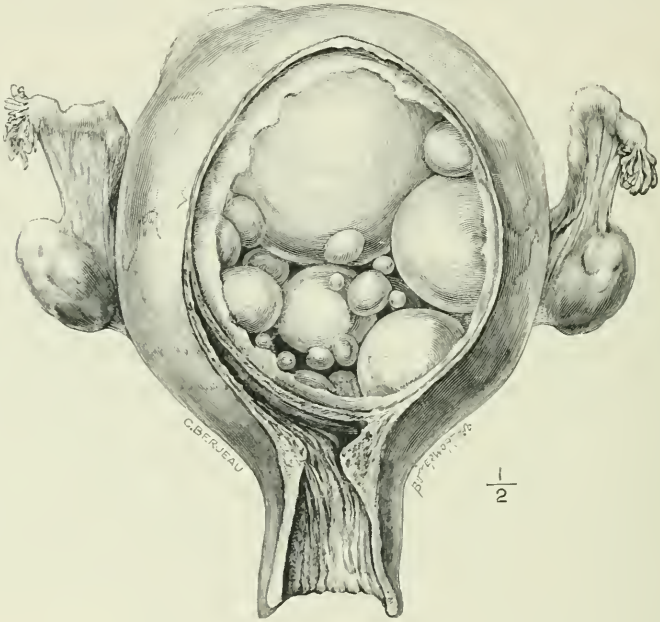
Hydatid of the Uterus.

By Arthur E. Giles, M.D.

Miss E. H., aged 32, was sent to me at the Prince of Wales's Hospital, Tottenham, by Dr. Howard Distin, of Enfield, in December, 1910. The patient had noticed for some time a swelling in the lower part of the abdomen, but she had neither pain nor discomfort from it: menstruation was regular and moderate, and she had felt perfectly well until the last month, when she suffered a good deal from nausea. On examination a rounded tumour was felt just above the pubes; on bimanual examination the swelling had the feeling of a soft tumour, the size of a fœtal head, connected with the front and left side of the uterus, and a diagnosis was made of interstitial or subperitoneal fibroid. The patient was admitted to the hospital for operation.

On opening the abdomen the tumour was found to arise from the anterior and left aspect of the body of the uterus. It felt cystic, and I thought it could not be a fibroid. If the enlargement of the uterus had been uniform I should have thought the condition one of pregnancy; but excluding this and fibroid I feared some degenerating malignant condition, and decided to do a panhysterectomy with removal of the appendages. After the completion of the operation I cut into the tumour and its true nature was at once manifest. As I had not suspected hydatid when the abdomen was opened I did not make any search for echinococcus colonies in the liver or elsewhere, but there was no obvious enlargement to suggest that such were present. The patient had a smooth convalescence, and is quite well at the present time.

The tumour has the form of a rounded cyst, bulging out of the anterior uterine wall, and more prominent on the left side. It is about 10 cm. in diameter, it contains numerous daughter and grand-daughter cysts, and on microscopic examination the characteristic hooklets were found. The cyst is in the actual substance of the anterior uterine wall, the uterine tissue being considerably thinned out. There is about the same thickness of uterine tissue and capsule on the peritoneal and on the mucosal aspects of the cyst. The condition is exceedingly rare, only seven or eight cases have been reported, and I have not been able to find a single one in the literature of this country. As to the mode of infection, there is nothing to suggest that the parasite had entered either from the cervical canal or through the peritoneal investment of the uterus, and one is driven to the conclusion that the blood-vessels formed the channel of infection; at the same time, as there was no evidence of the disease elsewhere, the case must probably be regarded as one of primary echinococcus colony in the uterus.



Hydatid of the uterus.

Report of Microscopic Examination by Mr. Targett.—Sections from the upper pole of the ectocyst consist chiefly of hyaline fibroid tissue with a thin outer coating of compressed muscle. There is no definite lining membrane, but layers of fibrin and remains of old blood-clot may be seen on the inner aspect. The portion examined from the lower pole of the ectocyst exhibits an imperfect covering of columnar epithelium on one aspect. This is evidently derived from the endometrium of the body of the uterus. The remainder of the ectocyst is composed of interlacing bundles of uterine muscle with fibrous tissue. The true cyst-wall has the characteristic laminated structure of *Tænia echinococcus*, but no hooklets have been seen.

DISCUSSION.

Dr. BLACKER asked whether the hydatid cysts projected more on the internal or the external surface of the uterus, and what did the actual covering of the cysts consist of? He thought it much more probable that they had burrowed into the uterine wall from without than from within, and in any case it was important that the nature of the tissue covering them should be described accurately. He had operated recently upon a very large abdominal hydatid cyst, closely resembling in its physical signs an ovarian cyst, for which it was mistaken. The tumour filled practically the whole abdomen, was attached to the under surface of the liver by a pedicle measuring about 1 in. in diameter, and contained some 3,000 secondary cysts. It was dissected away from the liver, and the raw surface cauterized, the patient making a good recovery.

Mr. TARGETT referred to the recto-vesical hydatid cyst which often filled the pelvic cavity, and was formerly regarded as being within the peritoneal sac. But dissection had shown that the cyst was developed in the cellular tissue at the base of the bladder, and that the embryos were probably deposited from the pelvic veins. Similarly the uterus might become invaded by cysts from the base of the broad ligaments, or the embryos might be deposited from the uterine sinuses directly into the muscular substance.

The PRESIDENT (Dr. H. Macnaughton-Jones) said that so far as he knew this was the first genuine case of primary hydatid tumour of the uterus that had been recorded in this country. When de Vries reported his case in 1901 there had been but seven genuine cases recorded. The infection appeared to have come through the circulation, and not, as Freund contended, through the bowel. Hydatid tumours of the internal genitalia in women were very rare. He believed that Dr. Eden's case of hydatid tumour of the Fallopian tube was the only one recorded in the United Kingdom, and Mr. Bland Sutton, in his most recent work, threw a doubt on the reality of most of the reported hydatid tumours of the ovary, believing them rather to occur in the mesometrium, as it did in that of the patient upon whom he (Mr. Bland Sutton) had operated.

Diffuse Fibroma of the Ovary which has undergone Central Degeneration with Extravasation of Blood.

By R. H. Paramore, M.D.

The specimens shown are the two halves of a tumour removed from a single woman (L. L.), aged 28, whose only complaint was that at times she was unable to pass her water. The patient, a domestic servant, was admitted to the Hospital for Women, Soho Square, on November 10, 1910, under the care of Dr. Oliver, to whom I am indebted for permission to publish the case.

The patient gave the following history: Three or four months before admission she woke up one morning and found she could not pass her water. A doctor was sent for, who, having drawn off the water, examined her, and told her the womb was misplaced, and caught in the neck of the bladder. Six weeks after she again woke up in the morning, unable to pass water. Subsequently it was drawn off, and, having rested awhile, the patient found she was able to relieve herself naturally. Since then three or four similar attacks have occurred, at odd times during the day and evening (e.g., at 3 and 10 p.m.). During the month previous to admission, the patient, when attempting micturition, adopted the knee-elbow position (on advice), finding she could pass water thereby more easily, and that when crouching the stream sometimes stopped. Previous to the first inability to pass water, the patient noticed she often had difficulty in starting and continuing the stream, especially if she had gone a long time without emptying the bladder.

The monthly periods started at the age of 16. In the early years they were a little irregular, appearing about every three weeks. Not much was lost, three diapers being sufficient. During the last five years the periods have recurred every twenty-eight days, and more blood was lost; during the last two years six diapers have had to be worn. Besides the gradual increase in loss, the patient used to pass clots, but has not done so lately. She has always had some pain with menstruation, and this has gradually got worse.

On examination, a lump was found in the pelvis so intimately associated with the uterus that moving the cervix imparted a movement to the tumour, so that it simulated a uterine fibroid. No ascites was present. The urine was healthy.

On November 14, 1910, Dr. Oliver opened the abdomen and removed a tumour, ellipsoidal in shape, about 5 in. long, 4 in. broad, and 3 in. thick. Its surface, except for some adhesions and superficial depressions, was smooth and even, without any lobulation; its substance was firm, but not hard. To its upper part the stunted Fallopian tube, only 1 in. long, is attached, and in close proximity is the ovarian ligament, the distal end of which disappears into the tumour. The lowest fimbria of the tube similarly passes on to the tumour. Immediately below the ovarian ligament the tumour was densely adherent to the peritoneum, and in removing the tumour a flap has been raised from its surface at this place. The tumour was bisected in the theatre, which proceeding caused the capsule and adjacent parts to retract somewhat, and was found to have a white cortex of growth, about ¾ in. thick, and a large central part, consisting of cystic spaces of various shapes and sizes, formed by delicate trabeculæ. The spaces contained apparently unaltered blood.

On microscopical section, the cortex is seen to consist of a cellular connective tissue. In its outer part the cells are delicate and small, their processes and nuclei disposed in planes concentric with that of the capsule; more internally the cells are more numerous, better developed, and more irregularly arranged, often radiating from centres, but they do not form dense interlacing bands; more internally still, the cells become sparser, as though teased apart by a swelling or liquefaction of the matrix. Many thin-walled vascular spaces exist in the outer part of the cortex, to which they are mainly confined; whilst numerous hyaline masses, often irregularly stellate in appearance, are seen scattered throughout the rest of the cortex, in which part the vascular supply is much less conspicuous. The innermost part of the tumour has undergone a more marked degeneration, and blood has been extravasated into it. Some of the vessels are thrombosed, and in places abutting upon the hæmorrhagic area; numbers of leucocytes are present. A section of the central part of the tumour shows that the trabeculæ forming the walls of the cyst-like spaces are devoid of endothelium, confirmatory of the degenerative nature of the blood extravasation.

Although no normal Graafian follicles are present in the sections, the tumour, from its position and its relations to the Fallopian tube, and the ligament, which appears to be the ovarian ligament, seems to be the representative of the ovary. The growth is a diffuse fibroma, which has undergone considerable degeneration, from which the blood extravasations apparently have resulted.

The patient made an uninterrupted recovery, and left the hospital twenty-three days after operation.

Dermoid Tumour of the Right Ovary removed by Abdominal Ovariotomy during Labour.

By G. Blacker, M.D.

Mrs. F., a primipara, Reg. No. 1422, 31 years of age, was admitted into University College Hospital on May 15, 1910. Her last menstrual period had occurred on August 8, and she had quickened on December 8. She had always enjoyed good health, and there was no history of any previous pelvic trouble. Early in the morning of May 15 the membranes ruptured, and she had a few slight labour pains during the day.

At 12.15 a.m., on May 16, I saw the patient with Dr. B. Hosford, who, on being summoned to attend her in her confinement, made a vaginal examination, and found a tumour in the pouch of Douglas lying below the fœtal head, which he diagnosed as an ovarian cyst obstructing delivery. On examination, I found that the external os was the size of a two-shilling piece, the cervical canal almost entirely taken up and the membranes ruptured. There was a tense elastic swelling in Douglas's pouch, flattened in its antero-posterior diameter, and pushing the cervix forwards against the symphysis pubis. The tumour appeared to be about the size of two fists, and the head of the child could be felt lying above it in the pelvic brim. The patient was having pains at infrequent intervals. A diagnosis of an ovarian cyst, probably a dermoid, was made, and the patient was admitted into the hospital for treatment. On admission her general condition was found to be very good, and the fœtal heart-sounds could be heard, regular in rhythm and of normal frequency.

As attempts to push up the tumour both with and without an anæsthetic failed—no attempt, however, was made with the patient in the Trendelenburg position—it was decided to perform abdominal ovariotomy. The question of removing the tumour by the vagina was considered, but as labour had begun I decided against this route on account of the difficulty of reaching and tying the pedicle securely, and the danger of the ligature slipping and hæmorrhage occurring during the birth of the child. The abdomen was therefore prepared with iodine, and an incision made to the left of the middle line, with the patient in the Trendelenburg position. On opening the peritoneum a little clear fluid escaped. An attempt then was made to draw the tumour out of the pelvis, where it was wedged tightly in the hollow of the sacrum behind the cervix. This, however, failed completely, and the incision was therefore prolonged up above the umbilicus, and the uterus turned out of the abdomen. The tumour was now drawn up easily, and found to be a dermoid tumour of the right ovary, with the pedicle twisted anticlockwise through 180°, when regarded from the posterior aspect. The pedicle was tied in two portions with silk, and the vessels again tied separately. The left ovary and tube were found to be normal, and were not removed. Considerable difficulty was experienced in returning the pregnant uterus to the abdominal cavity, mainly owing to the fact that the patient took the anæsthetic (ether) very badly; but this difficulty was readily overcome when the anæsthetic was changed to chloroform. The abdomen was closed with through-and-through silkworm-gut sutures, silk sutures for the fascia and the fat, and a continuous horsehair suture for the skin. After the dressings had been applied the patient was put up into the lithotomy position, and the cervix, now the size of half-a-crown piece, dilated with Bossi's dilator up to 3 in. in diameter. An attempt was made to introduce a Champetier de Ribes's bag, but as this proved very difficult, owing to the fact that the liquor amnii had nearly all drained away, and the uterus was contracted tightly on to the child, the attempt was not persisted in, and the patient was put back to bed, having been under the anæsthetic for seventy-five minutes.

Some little time after there was a free loss of blood from the vagina. At 2 p.m. her condition was good, but she complained of abdominal pain and pain in the back, and was much troubled with vomiting. At 8 p.m. labour pains were occurring every few minutes, and at 9 p.m., as the cervix was fully dilated, chloroform and alcohol were administered, and the patient delivered with forceps of a male child weighing 6 lb. 8 oz. and measuring 19½ in. in length. The birth of the child took place twenty hours after the operation and thirty-six hours after the rupture of the membranes. The child was born in a condition of extreme asphyxia, but by inversion, immersion in a hot bath, the performance of artificial respiration, and the rubbing of brandy on its gums and over the heart, it was revived. As there was a good deal of hæmorrhage, the placenta was expressed and a hot intra-uterine douche given. The uterus soon responded to this treatment, and a short time after the delivery the pulse was 90 per minute and of good quality. The perineum was torn to within an inch of the sphincter, and was stitched.

The patient made an uninterrupted recovery, the wound healing by first intention, and the temperature reaching 100° F. on one occasion only. There was some difficulty in passing urine for a few days, and the urine contained some muco-pus, but this cleared up quickly, and the mother and the baby left the hospital well on June 3. They are both in good health at the present time.

The tumour removed is a dermoid of the right ovary, measuring 11 cm. by 12 cm., consisting mainly of two loculi, and containing the typical cheesy contents of a dermoid cyst and a quantity of short hair, light brown in colour. The smaller of the two loculi has attached to its wall a nipple-like projection, measuring 2 cm. by 1 cm., and this loculus contains most of the hair.

Cases in which abdominal ovariotomy has been performed during labour and the child delivered through the natural passages are still very few in number. I have found references to some twelve cases, including the one Mr. Malcolm is bringing before the notice of the Section this evening.

It is interesting to note that this is the fifth occasion on which this operation has been performed at University College Hospital, Sir John Williams having performed it on two occasions, and Dr. Herbert Spencer also on two occasions.

The present case illustrates the fact that, provided the condition of the mother and of the child remains good, the patients may be left for many hours, even after rupture of the membranes, for complete dilatation of the cervix to occur spontaneously. As soon as this has been completed, however, the child should be delivered by forceps or version, and, if there are any indications that the life of the child is in danger at the time of the operation, the cervix should be dilated rapidly and immediate delivery carried out.

The treatment of this condition by Cæsarean section can be necessary only in a very small percentage of the cases, and presents no advantages over the delivery of the child by the vagina after the tumour has been removed either by the vagina or preferably by the abdomen.

BIBLIOGRAPHY.

GUINARD et CHAUVE. *Ann. de Gynéc.*, Par., 1903, lix, p. 459.

HARTMANN. *Zentralbl. f. Gynäk.*, 1909, xxxiii, p. 1484.

HOFMEIER. *Trans. Amer. Gynec. Soc.*, Philad., 1909, xxxiv, p. 239 (discussion on Dr. H. R. Spencer's paper).

SANDBERG. *Med. Rev.*, 1899, ii, p. 161.

SPENCER, HERBERT. *Trans. Obstet. Soc. Lond.* (1898), 1899, xl, p. 14; *Trans. Amer. Gynec. Soc.*, 1909, xxxiv, p. 215.

WILLIAMS, Sir JOHN. "Cavendish Lecture," *Lancet*, 1897, ii, p. 129.

An Ovarian Tumour obstructing Delivery and fixed by Suction in the Pelvis.

By John D. Malcolm, F.R.C.S.

Obstruction to delivery by an ovarian tumour blocking the pelvic outlet is a well-recognized condition which is not very often met with. In 1887, Dr. R. Gordon McKerron collected and published[1] records of 183 cases, in some of which it was easy to push the tumour up into the abdomen, but the proportion of cases to deliveries is very small. In the Rotunda, Dublin, the Government Maternity Hospital, Madras, and Queen Charlotte's Lying-in Hospital, 5,792 deliveries were recorded[2] in the year 1909, and there were only two cases—i.e., 0·034 per cent.—in which operative treatment was required for relief of this difficulty. Both were in Queen Charlotte's Hospital and were treated by Cæsarean section. In one other case a cyst in the pelvis ruptured and did no obvious harm. I am not aware that any such case requiring operation to effect delivery has ever occurred in the Samaritan Free Hospital.

The complication is rare, because comparatively few ovarian tumours create the circumstances required for its occurrence. The growth must be of sufficient size to be caught by the pelvic brim when the ovaries ought to rise into the abdomen with the developing womb, but it must not at this time be so large or so placed that it will be pushed out of the pelvis by the fundus uteri. A few months later, when the fœtus is fully developed, the abnormal mass must have become big enough to cause an obstruction. On the other hand, it must have remained so small that it does not cause symptoms requiring surgical treatment before delivery is due. A long pedicle and an absence of adhesions increase the chances that the tumour will not attract attention until an examination is made during or in anticipation of labour.

The case related below is the only one of this kind which I have been called upon to treat. The patient, a healthy woman, aged 31, had her vermiform appendix removed in 1905 by the late Dr. George R. Fowler, of Brooklyn, New York. His son, Dr. Russell S. Fowler, has kindly informed me that it may be confidently stated that there was no obvious alteration of the ovaries in 1905. The patient married in 1909, and came in labour at full time on November 10, 1910. In the evening of that day Dr. Malcolm Mackintosh found a tumour which he diagnosed as ovarian. It filled the pelvis, leaving just room to pass the fingers through the vagina. The os uteri, which appeared to be already fully relaxed, lay above the obstruction and the head presented. The membranes were unruptured. Pains were frequent, but delivery *per vaginam* was impossible unless the tumour collapsed or was removed. The patient was therefore taken to a nursing home, and the abdomen was opened some two hours after the complication was discovered.

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[1] "Obstruction of Labour by Ovarian Tumour in the Pelvis," *Trans. Obstet. Soc.* (1897), 1898, xxxix, p. 331.

[2] Reports of these Hospitals.

The growth had a broad vascular pedicle on the left side, under which a finger could be passed between it and the neck of the womb. It was possible to push the cervix uteri containing the child's head on to the right iliac fossa so as to allow sufficient space for the tumour to pass, but the latter closely fitted the pouch of Douglas, and although it could be rotated in any direction so freely that it was obviously not adherent, although its upper part could be grasped and although the fingers could be swept all round it to a considerable depth, yet prolonged efforts to draw it out of the pelvis failed. It did not seem likely that turning the uterus out of the abdomen would make removal easier or that pushing from below would help, but it occurred to me that the difficulty was chiefly due to suction. A cannula was therefore introduced along the side of the pelvis into the lower part of the peritoneal sac and then traction easily brought the tumour forward, its displacement being accompanied by a gurgling of air through the tube. The pedicle was secured in several pieces and a smaller cystoma of the right ovary was also removed from high up in the abdomen.

After the incision was closed Dr. Mackintosh ruptured the membranes and delivered by forceps whilst the patient was still anæsthetized. The presentation was left occipito-anterior. The placenta followed at once. The abdominal wound healed by first intention, and when the parts were fully contracted the cicatrix measured 5 in. The mother made an uninterrupted recovery. Two months after delivery, and again a month later, there was a discharge exactly resembling that of a menstrual period. This was unexpected, as both ovaries had long pedicles, and it was believed that they were completely removed. The child was a male and fully developed. He was very blue when born, but breathed almost immediately, and afterwards caused no anxiety at any time. He is now growing well.

The pelvic tumour measured about 6 in. by $4\frac{1}{2}$ in. The other was about $2\frac{1}{2}$ in. in its longest diameter. Both were cystic and the larger contained a mass of hair in one of its cavities. This was of a light auburn colour, closely resembling that on the patient's head. There were also small solid areas, and the Clinical Research Association reported that "one consists of connective tissue with sebaceous sweat-glands and hair-follicles, whilst the other contains a mass of adenomatous tissue indistinguishable from thyroid gland. The cystic portions of one are lined by epidermis, of the other by flattened epithelium."

In this case the need for delivery was so urgent, and the tumour seemed so jammed in the pelvis, that it was not thought wise to attempt to remove the obstruction except by operation. The idea of performing a vaginal section was at once dismissed, because the mass seemed so hard that it might have been solid, and, of course, if it was ovarian, the pedicle was necessarily in the abdomen. The fixation of the growth by the suction created by its accurate fitting of the pelvis was the only difficulty, and this is likely to exist whenever a tumour fills the pouch of Douglas during labour, because the broad ligaments and the neck of the womb make an elongated narrow passage through which the obstructing body must be withdrawn.

Emptying the cyst by tapping, and the uterus by Cæsarean section, have been recommended and practised to overcome the difficulty in these cases, but tapping must almost certainly lead to spilling of tumour contents, the nature of which is unknown, over the peritoneum, and although Cæsarean section is not now considered a formidable procedure it is a very severe measure as part of an operation for the removal of an ovarian tumour. Yet Cæsarean section has been frequently chosen. In the 1,282 cases of this operation, collected by Dr. Amand Routh,[1] there are twenty-six in which it was undertaken because of obstruction by ovarian tumours in the pelvis. A reference to the original publications, when these are given, shows that the propriety of removing the tumours first and effecting delivery by the natural passage is for the most part not mentioned. In several cases no attempt even to examine the tumour was made, and more than one writer, after considering the different courses that might be adopted, decided that Cæsarean section was advisable. Dr. A. J. Wallace described exactly the difficulties which I encountered, but, of course, I do not know whether he could have overcome them by the means adopted in my case. He was forced to perform Cæsarean section, and even after the uterus was emptied the tumour, although not adherent, "offered a most obstinate resistance to all attempts to displace it."[1]

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[1] *Journ. of Obstet. and Gyn. of Brit. Emp.*, 1911, xix, p. [illegible].

Treatment of these cases by abdominal ovariotomy during labour does not seem to have been adopted very often. In Dr. McKerron's list there are only two instances, both operated upon by Sir John Williams in University College Hospital. The third case in this country was also in that hospital under the care of Dr. Herbert Spencer who, in reporting it,[2] mentioned another published by Spaeth[3] as completing the record up to 1897. These cases were all successful, and it seems to me that when a pelvic ovarian tumour cannot be pushed out of the way during labour the tumour should always be carefully examined and abdominal ovariotomy should be performed whenever it is practicable.

The chief point in the history above recorded is one which I have not seen mentioned in any publication, namely, that the range of cases in which ovariotomy is possible, without tapping a cyst and without Cæsarean section, may be considerably extended if it is remembered that suction is sometimes an important factor to be overcome before a tumour can be brought out of the pelvis. When this is dealt with by the manœuvre described it should be possible to remove whole any non-adherent ovarian neoplasm, whether it is cystic or solid, without emptying the uterus if it is possible to do so after Cæsarean section. Another well-known fact emphasized by the case is that there should be no delay in operating and no prolonged effort to push the growth out of the way. An ovariotomy is not nearly such a serious complication of labour as a damaged ovarian tumour, and our patient is greatly indebted to Dr. Mackintosh for the prompt way in which he made all the arrangements for the operation.

[The patient was on April 28 reported to be in "perfect health." The periods continued with slightly irregular intervals.]

DISCUSSION.

Dr. ARTHUR GILES related the following instance of ovariotomy during labour. The patient, who was of an enormous size, was sent by her doctor to a general hospital, where the opinion was expressed that the condition was one of either hydramnios or ascites, and on the supposition that it might be the former it was suggested that her doctor should puncture the membranes. He did this, but there was practically no resulting difference in the distension. When Dr. Giles saw her the uterus was fairly defined towards the left side of the abdomen, and a diagnosis of large ovarian cyst was made. At this time the os was commencing to dilate, and the patient was evidently in labour. He thought it would be dangerous to let her go through the labour with this enormous tumour present, and he arranged for her to be transferred to the Chelsea Hospital for Women. The operation was performed two hours later, at 8 p.m. By an incision extending from the ensiform cartilage to the pubes a multilocular ovarian cyst weighing 31 lb. was removed entire. Natural delivery occurred at 2 a.m., five and a half hours after the completion of the operation. The mother made an uneventful recovery, and she and the baby are now both well, the child being a year and a half old.

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[1] *Practitioner*, 1907, lxviii, p. 347.

[2] *Trans. Obstet. Soc. Lond.*, (1898), 1899, xl, p. 14.

[3] *Med. Corblatt. d. württembl. ärtzl. Verein*, Stuttg., 1897, lxvii, p. 221.

Dr. TATE was of opinion that in cases of obstruction during labour by ovarian tumours it was an advantage to postpone operative interference, if the symptoms were not urgent, till the os uteri was fully dilated. By this practice, after removal of the tumour by abdominal section, the child could be delivered by forceps, and the abdominal wound then sewn up, which was much more easily and satisfactorily done after the evacuation of the uterus. Dr. Tate had seen a case treated in this manner with very satisfactory results.

Dr. AMAND ROUTH was glad these two cases of ovariotomy during labour had been brought forward. Influenced by the teaching of Sir John Williams, he had always held that Cæsarean section under such circumstances was hardly ever justifiable, yet in the 1,282 cases of that operation collected by him there were twenty-eight instances of Cæsarean section preceding the ovariotomy. He instanced a case where he had been able to dilate the cervix and "express" the child with the other hand with remarkable ease during a laparotomy performed by Mr. Marmaduke Sheild for diffuse purulent peritonitis of appendix origin. Such "expression" after the ovariotomy might facilitate labour before the abdomen was closed.

Dr. C. HUBERT ROBERTS was much interested in both these cases, as he himself had a very similar one under his care at the present moment at the Samaritan Free Hospital for Women. It occurred in a woman, aged 29, who was admitted on March 10, 1911, in labour, which was obstructed by an ovarian tumour. Her previous obstetric history was curious, and one of the chief factors of interest. She had been married six years. Ten months after marriage she was delivered by craniotomy at full term with very great difficulty, the case being taken to be one of deformed pelvis. Two and a half years after she again became pregnant, and considering the former difficulties her doctor advised induction of premature labour at seven and a half months. The child was born alive, and lived. Her third pregnancy began about eight and a half months ago (dating from her admission on March 10, 1911). Dr. Roberts saw her with Dr. Reginald Crosse, of Balham, about a month ago, as he had discovered a tumour in Douglas's pouch. This was very evident on vaginal examination as a soft elastic mass displacing the cervix and presenting part over to the right. It could not be pushed up at the time. It was decided to wait till the child was relatively more viable, and then to perform ovariotomy during pregnancy at about the thirty-sixth week, or even later. On March 10, 1911, the patient came into labour at about eight and a half months, and was urgently admitted to the hospital in a condition of obstructed labour. A cystic tumour was found, filling nearly the whole of the left side of the pelvic cavity; it was very tense, and seemed firmly jammed down in front of the fœtal head, which could be felt through the dilated cervix, displaced upwards, and to the right. The pains were very strong and frequent, and the patient in great distress. Mr. Malcolm and Dr. Lockyer saw the case in consultation, and an ovarian tumour obstructing labour was diagnosed. It was decided to perform ovariotomy at once, unless the tumour could be pushed up. After some difficulty in the inverted position, Dr. Roberts was able to push the swelling out of the pelvis, and a living child was very rapidly delivered without assistance. The patient made an uneventful recovery. The baby has also done well, being breast-fed. A week after delivery the patient was carefully examined, and the tumour on the left side could easily be felt above the brim, about the size of a goose's egg, very freely movable. Another very similar tumour could also be made out on the right of the uterus. This was also very mobile, but harder in consistency. On vaginal examination no part of either tumour could be felt, and the cervix occupied its ordinary central position. No symptoms of twisting of the pedicle or inflammation of the tumours occurred, and the puerperal temperature was normal. Dr. Roberts decided to remove the tumours about three weeks after labour, and abdominal section was performed on April 3, 1911. The operation was an easy one, both tumours were pedunculated, and lay above and in front of the uterus. No adhesions were present, and no difficulty was found in ligaturing the pedicles. Both tumours were dermoid in character. The left-sided one was cystic, thin-walled, and contained little else but liquid fat and hair. The right-sided one was much more solid, and its walls thicker. It also contained hair and thick mammilliform process, covered with true skin. It was found possible to preserve a portion of the ovarian tissue in the pedicle of the right side, with a view to future conception. Both Fallopian tubes were left intact. The progress to date had been satisfactory, and the patient was still nursing her baby. Dr. Roberts thought his case of some interest, as it was evident from the obstetric history that two labours had really been complicated by the one or other of the dermoid tumours, whereas the case had been taken to be one of contracted pelvis. As regards treatment, he advocated ovariotomy in labour if the tumour could not be pushed up, and did not think vaginal ovariotomy or Cæsarean section was correct treatment. A point on which he asked for information was whether patients in the puerperium bore operations as well as at other times? Also supposing the tumour had been pushed up, at what particular period after delivery would it be best to remove the tumour by abdominal section?

Dr. GRIFFITH referred to a case under his care at Queen Charlotte's Hospital, in which labour was obstructed by a dermoid cyst in the pelvis, and the external os uteri was above and behind the pubes. He incised the posterior vaginal wall, and a powerful uterine contraction immediately expelled the cyst and the fœtus together. Considerable hæmorrhage resulted from a laceration of the posterior uterine wall through the lower segment, which was controlled by sutures. The ovarian pedicle was not found, yet the patient made an uninterrupted recovery.

Dr. BLACKER felt sure that the proper treatment for these cases where there was any difficulty in pushing up the tumour was abdominal ovariotomy, followed by delivery through the natural passages. In any future case, however, before opening the abdomen he would try the effect of inverting the patient and attempting to push the tumour out of the pelvic cavity with the woman in that position.

A Suppurating Ovarian Cyst, in a Girl aged 10, probably infected from Hairpins impacted in the Vagina.

By HERBERT J. PATERSON, M.C.

K. B., AGED 10, was admitted into hospital on December 16, 1903, under the care of Dr. Soltau Fenwick. Eighteen months previously she had been in hospital with abdominal pain, supposed to be due to tuberculous peritonitis. Since then she had remained quite well until December 6, when she complained of headache and pain in the abdomen, accompanied by vomiting and some vaginal discharge.

Condition on admission: Patient was thin, dark-complexioned, rather delicate-looking, and anæmic. Her breath was very offensive. The abdomen was distended, but there was no tenderness anywhere. Her temperature was 100° F. Between December 17 and December 28 patient had a hectic temperature, varying between 97° F. and 102° F. On December 31 the temperature rose to 103·6° F., was accompanied by recurrent pain in the right iliac fossa, and continued irregular with daily remissions.

On January 6, 1904, there was pus in the urine, with marked resistance in the right iliac fossa. On January 12 the temperature was 103° F., and operation was decided upon. Chloroform was administered on January 13, and an incision was made through the muscles in the right iliac region. After separating adhesions the appendix was found inflamed and enlarged, and adherent to the brim of the pelvis, and when removed it was found to contain pus. The intestines were plastered over the pelvis, so no examination of the pelvic viscera was made. The incision was closed with through-and-through silkworm-gut sutures, with a drainage tube at its lower end.

After the operation the patient seemed much better, the temperature was comparatively lower, although the diurnal variations were still rather great, but after a few days the temperature began to rise again, until, on February 4, it once more reached 103·6° F. There was more pus in the urine and more albumin. The bladder was washed out daily, and after a few days there was a remission of the temperature, only to be followed by a steadily increasing rise, with greater daily variations.

On March 2, I examined the patient under anæsthetic, and on putting my finger into the vagina I discovered the rounded ends of two hairpins, the points of which were firmly embedded in Douglas's pouch. These were removed. I thought that with the removal of the hairpins the temperature might become normal, but this not being the case it was decided to re-open the abdomen to ascertain if there was a septic focus in the pelvis. The presence of the hairpins in the vagina could not be accounted for, as, in spite of repeated coaxings, entreaties, and scoldings the patient denied all knowledge of the matter.

On March 9 I opened the abdomen in the middle line below the umbilicus. After tedious dissection and separation of adhesions, owing to the fact that the intestines were extensively matted together all over the lower part of the abdomen, a dark red rounded swelling was discovered immediately behind the bladder, adherent on all sides to the intestines. The swelling was tapped and found to contain pus. The trocar hole was sewn up, and the cyst was then carefully removed. A drainage tube was placed in the cavity left by the cyst, and the wound was sewn up with interrupted silkworm-gut sutures. The patient made an uninterrupted recovery. After the operation the temperature was only once above 99° F.

She left the hospital on April 21 in good health, and having put on weight.

On October 12, 1905, patient was readmitted. She had been for a year at a convalescent home at Enfield. During that time she had pain from time to time in the appendix scar, accompanied by some bulging. The pain was relieved by rest in bed for a few days. Patient was in good condition on admission, had grown, looked well, and had put on flesh since last in hospital. There was some gurgling in the right iliac region, and some bulging, the intestines probably being adherent to the appendix scar. There was no swelling elsewhere in the abdomen. Nothing was then done, and the patient went out on October 21.

On December 11 of the same year, she came in again for examination under an anæsthetic, which took place on December 21, with the following result: *Per vaginam*—In the upper part of the vagina was a slight annular stricture, above which was a pouch about ½ in. deep, at the top of which the vaginal mucous membrane was slightly thickened and nodular. Bimanually—High up in Douglas's pouch was a rounded swelling like the fundus, which could be traced down to the top of the pouch above described.

Before anything further could be done the patient developed scarlet fever, and was sent to the fever hospital.

Two years later, on December 14, 1907, patient was again admitted. During the interim she had been in service, and had found that the bulging of the wound in the right iliac fossa caused her pain and discomfort. Her weight then was 6 st. 11 lb. 13 oz. On December 19 patient was anæsthetized and an incision made through the scar in the right iliac fossa. A portion of the intestine was adherent to the scar at one spot, and a tag of omentum to the site of the appendix, but beyond this there were no adhesions anywhere in the abdomen. The intestine was separated from the scar, the scar tissue excised, and the wound sewn up in three layers. The central wound was treated in the same way, as it showed signs of weakness at its lower part. The wounds healed by first intention, and the patient left the hospital on January 11, 1908. Since then the patient has been in service, and when last heard from (February, 1911) was in perfect health.

This case is interesting, not only because of the rarity of suppurating ovarian cysts in children, but on account of the difficulty in diagnosis. At first it was thought that the patient was suffering from tuberculous peritonitis, but subsequently the irregular temperature, and the resistance and tenderness in the right iliac fossa, led to a diagnosis of suppurative appendicitis, for which operation was undertaken. The operative findings seemed to confirm the diagnosis of appendix trouble, as the appendix was inflamed, enlarged, buried in adhesions over the brim of the pelvis, and contained pus. For a few days the patient improved, and the temperature remained lower, but soon rose again with a renewal of its daily oscillations, so that the presence of further trouble was clear.

The discovery of the hairpins deeply embedded in the vaginal wall made one suspect pelvic suppuration, and on opening the abdomen later a suppurating ovarian cyst was found, which had probably become infected from the foreign bodies in the vagina. It is, of course, impossible to say whether the appendicitis was primary or secondary to the pelvic condition, but I am inclined to regard it as secondary.

An interesting feature of the case is that at the subsequent operation for ventral hernia all trace of the dense pelvic adhesions had disappeared.

In 1896 Mr. Bland Sutton[1] published a table of seventy-nine cases of ovarian cysts in children, of which forty-one were instances of simple cysts or adenomata, and thirty-eight cases of dermoid cysts. I have collected thirty-eight more cases, of which fifteen were dermoids. It is interesting to note that in two instances operation was undertaken for supposed acute appendicitis, as in my own case. Torsion, it is stated, occurred in four of the cases. Judging by the records, suppuration is a rare complication, as it is mentioned in three only of the whole series of cases.

BIBLIOGRAPHY.

Ovarian Cysts in Children (not including Dermoids).

Alcock. *Lancet*, 1871, ii, p. 850.
Alexander, R. J. *Texas Med. Journ.*, 1898-99, xiv, p. 163.
Arumgam, J. V. *Brit. Med. Journ.*, 1907, i, p. 1363.
Aubert. *Lyon méd.*, 1893, lxxiii, p. 156.
Baldy, J. M. *Amer. Gyn. and Obstet. Journ.*, 1899, xiv, p. 184.
Beckett, W. W. *Ibid.*, 1898, xii, p. 349.
Bell. *Lancet*, 1887, i, p. 418.
Boldt, H. J. *Proc. Path. Soc.*, New York, 1888, p. 63.
Bornstein, M. *Journ. Amer. Med. Assoc.*, Chicago, 1909, lii, p. 1102.
Bryant. *Guy's Hosp. Reports*, 1869, 3rd ser., xiv, p. 216.
Chenoweth. *Amer. Journ. of Obstet.*, New York, xv, p. 265.
Davidson, C. F. *Med. News*, Philad., 1892, lxi, p. 14.
Delassus. *Journ. de Soc. méd. de Lille*, 1903, i, p. 235.
Duchamp. *Archives de Tocologie*, Par., 1884, xi, p. 23.
Durante, G. *Bull. Soc. Anat. de Par.*, 1898, lxxiii, p. 464.
Eastman, J. *Med. and Surg. Monitor*, Indianap., 1900, iii, p. 410.
Erdmann, J. F. *New York Med. Journ.*, 1904, lxxx, p. 1175.
Girol, R. *Archiv. de ginecol.*, Barcel., 1895, viii, p. 644.
Gould. *Med. Press and Circ.*, 1892, ii, p. 218.
Hamaker. *New York Med. Journ.*, 1889, l, p. 288.
Homans. "Laparotomies, &c.," (3 cases), Boston, 1887.
Johnson, R. *Trans. Clin. Soc. Lond.*, 1896-97, xxx, p. 167.
Juon. *Gaz. Hebd.*, 1869, 2me sér., vi, p. 396.
Karczewski, A. "Medycyna," Waszawa, 1903, xxxi, p. 598.

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[1] "Diseases of the Ovaries and Fallopian Tubes," 1896, p. 176.

Keen. *Brit. Gyn. Journ.*, 1893-94, ix, p. 339.
Kelly. *Johns Hopkins Hosp. Reports*, Balt., 1894, iii, p. 672.
Koeberle. *Lond. Med. Rec.*, 1876, p. 90.
Kollock. *Journ. Amer. Med. Assoc.*, 1891, xvii, p. 890.
Loring, R. C. *Journ. Amer. Med. Assoc.*, 1908, l, p. 1350.
Lucas. *Trans. Clin. Soc. Lond.*, 1888, xxi, p. 224.
Ludlam. *Clinique*, Chicago, 1894, xv, p. 114.
MacGillivray, C. W. *Lancet*, 1907, i, p. 1487.
Monnier, L. *Rév. pract. de Gyn. et de Obstet.*, Par., 1900, xvi, p. 165; *Arch. de Tocol. et de Gyn.*, Par., 1893, xx, p. 581.
Omori and Ikeda. *Berl. klin. Wochenschr.*, 1890, xxvii, p. 118.
Parker (quoted by Bland-Sutton).
Parks. *Amer. Journ. Med. Sci.*, Philad., 1890, c, p. 258.
Péan. "Leçons de Clin. Chir.," 1885-6, 7me sér.
Idem. *Ibid.*, 1887-8, 8me sér.
Platt, W. B. *Maryland Med. Journ.*, Balt., 1889-90, xxii, p. 446.
Power, D'Arcy. *Trans. Path. Soc. Lond.*, 1897-8, xlix, p. 186.
Ramaugé, A *Ann. de Circ. Med. Argent.*, Buenos Aires, 1891, xiv, p. 201.
Reford, J. H. *Brit. Med. Journ.*, 1905, i, p. 412.
Salin. *Zentralbl. f. Gyn.*, Leipz., 1891, xv, p. 711.
Schwarz. *Arch. f. Gyn.*, Berl., 1878, xiii, p. 475.
Sims, M. Keating's "Cyclopaedia of Diseases of Children," 1890, iii, p. 735.
Sutton, S. *Journ. Amer. Med. Assoc.*, 1892, xix, p. 139.
Syme. *Austral. Med. Journ.*, xv, p. 442.
Tait, Lawson. "Disease of the Ovaries," p. 319.
Terrier. *Rév. de Chir.*, Par., 1885, v, p. 34.
Idem. *Ibid.*, 1886, vi, p. 194.
Terrillon. *Bull. Gén. de Thérap.*, Par., cxv, p. 529.
Thellung. *Zentralbl. f. Gyn.*, Leipz., 1908, xxxii, p. 1464.
Thornton. *Brit. Med. Journ.*, 1887, ii, p. 594.
Usiglio, G. *Rev. Venita di Soc. Med.*, 1890, xiii, p. 221.
Vincent. *Lyon méd.*, 1893, lxxiii, p. 156.
Walter, W. *Brit. Gyn. Journ.*, 1895-96, xi, p. 51.
Wells, Sir Spencer. "Ovarian Tumours," London, 1882.

The President remarked on the difficulty in diagnosis in some of these cases in young children when the right ovary was the one affected. In two cases he had operated on for removal of the appendix in girls under 12 the appendix in one had been fixed to the floor of the pelvis, where there was an abscess, and in the other, on whom he had recently operated, the affected appendix was incorporated with the ovary by adhesions. There had been previous attacks of appendicitis.

A New Operation for the Radical Treatment of Flat Pelvis.

By Ernest W. Hey Groves, M.S.

It must seem to be a bold thing to suggest, at this time of day, that there can be anything new, possible in the treatment of a contracted pelvis. And it is therefore with considerable diffidence that I venture to bring before this learned Society a suggestion based solely on anatomical evidence. It was in the course of an investigation as to the possibility of turning aside the pubic bones in order to get better access to the bladder, for the removal of malignant growths of that organ, that the ideas embodied in this paper first suggested themselves to me. By dividing the symphysis pubis, and then cutting the pubic bones on each side, the front wall of the pelvis can be turned aside, so as to give a good exposure of the front of the bladder and allow of a precise and accurate dealing with the ureters and vessels at its base. But whilst this may be a difficult and hazardous procedure, it is remarkable with what ease and simplicity the two pubic bones can be moved forward as a whole after dividing the horizontal ramus only, on each side. This led me aside from my original line of thought, to consider the somewhat fascinating question as to whether it is not possible to permanently enlarge the flat pelvis in its antero-posterior diameter. And I have fully convinced myself, as I hope to convince you, that this object can be attained easily by an operation, which is anatomically a very simple one, but of the clinical possibilities of which I will leave you to form your own opinion.

The principle of the operation is as follows: The horizontal ramus of the pubis is completely divided on both sides. The ascending ramus of the ischium is partly divided on both sides, so as to allow of the ready fracture of this bone. The whole of the pubic bones are then displaced forward until their posterior surfaces catch against the anterior surfaces of the parts of the rami of the pubes which remain in position. The bodies of the pubic bones remain permanently in this position, and the pelvis has its conjugate diameter increased by $\frac{3}{4}$ in. It will be convenient to consider the various steps of the operation in detail:—

(1) *The Division of the Rami of the Ischia.*—The patient is placed in the lithotomy position and the ischial tuberosities defined. A small stab incision is made over the ramus of the ischium, just in front of

the tuberosity, and through this the bone is partly sawn through with an Adams's saw. This is best done on the outer surface of the bone, so as to avoid any risk of injury of the internal pubic vessels and nerve which lie with the internal obturator muscle on its deep surface. This is done on both sides, the wounds closed each by a single stitch and dressed with a collodion dressing. The patient is placed in the supine position for the remainder of the operation.

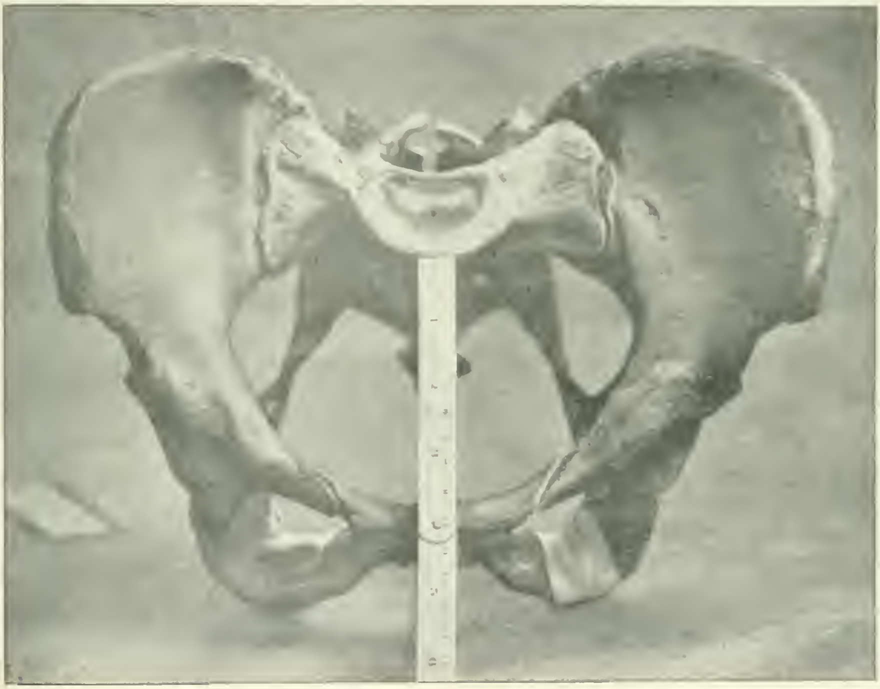


FIG. 1.

A flat pelvis with conjugate of 9 cm. The pubic rami are divided.

(2) *The Division of the Horizontal Ramus of the Pubis.*—An incision 1½ in. long is made over the inner end of Poupart's ligament and the soft parts divided down to the external abdominal ring. The inguinal canal is opened up by slitting the aponeurosis of the external oblique, and its contents displaced upwards. The fascia forming the posterior wall of the canal is cut in the line of the original incision, and the pubic ramus thus exposed. The peritoneum is readily separated from the back of the bone with the finger-tip. A search is made for the

presence of any accessory obturator vessels, and if these are present they must be divided between two ligatures. The pubic ramus is then sawn through as far outwards as the position of the femoral vessels allows. This is done by an Adams's saw held in the plane of the brim of the pelvis, the point being directed backwards and as much outwards as possible. This oblique section of the bone is the essential point in the operation, because it provides for the locking of the bones in their new position (fig. 1).

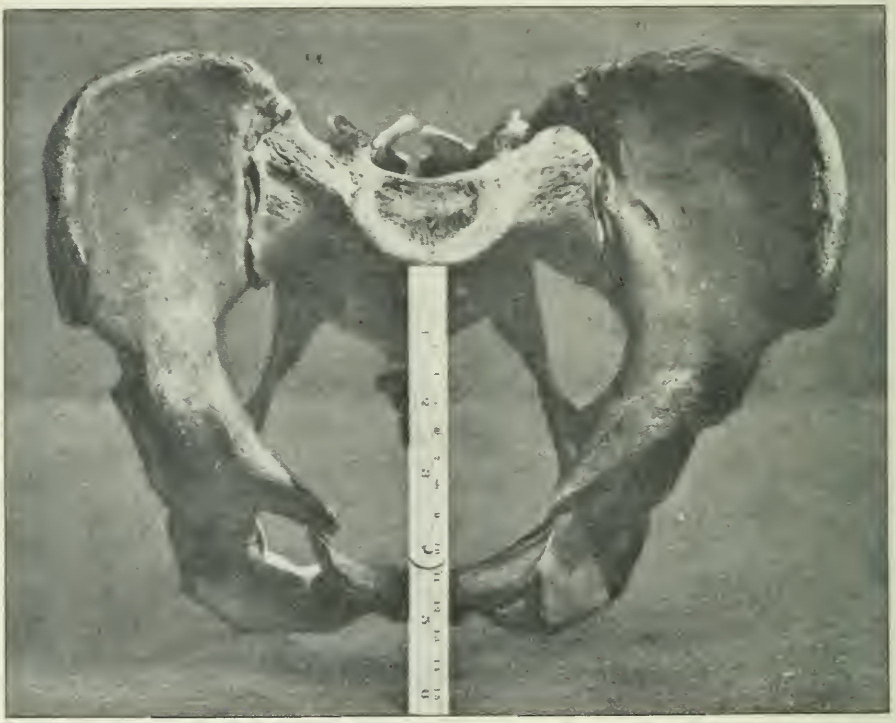


Fig. 2.

The same pelvis after the pubes have been displaced forward, giving a conjugate diameter of 11 cm.

(3) *The Displacement of the Bodies of the Pubic Bones Forward.*— The two pubic bones are now to be brought forward into their new position. This can generally be done by the mere traction of the two index fingers, but it is accomplished better by the help of a pair of instruments like spanners which I have had made for the purpose.

Poupart's ligament will require to be partly divided on both sides. Then the spanner is slipped into the wound and over the body of the pubic bone on both sides. Very moderate pressure on the handles of these instruments then serves to lever the pubic bones forward, the tendency of the bones to jamb against the rami being overcome by pressing the handles of the spanners slightly outwards, so as to bend the pubic bones inwards at the symphysis. When the bodies of the pubic bones have cleared the rami they are bent outwards, and they then rest in such a position that they cannot slip back (fig. 2). In the carrying out of this part of the operation the ischial rami fracture at the place where

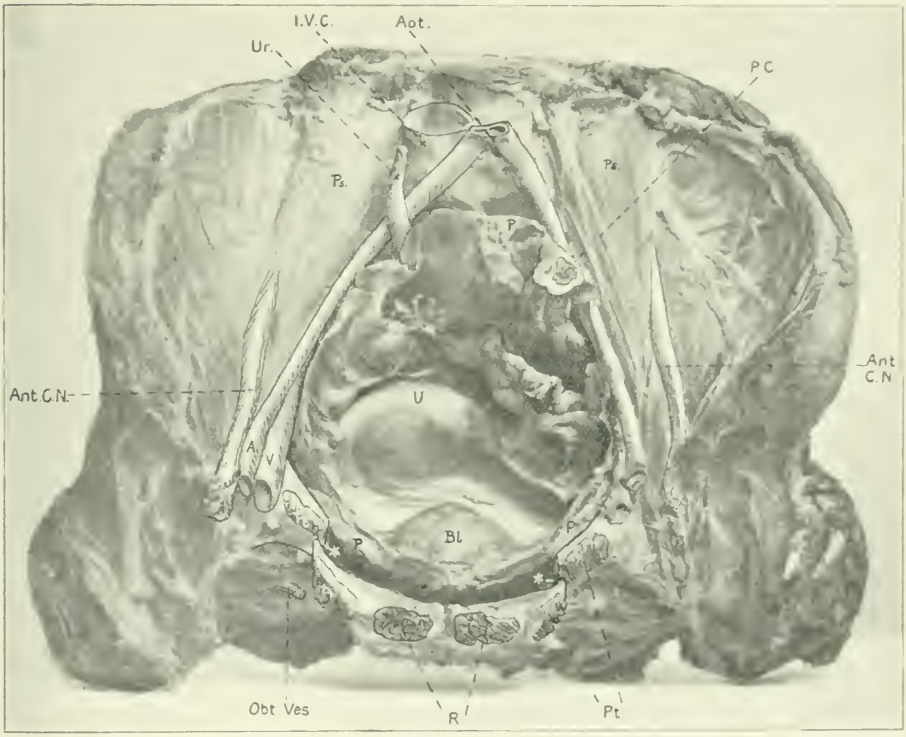


FIG. 3.

A dissected pelvis after the operation has been completed. I.V.C., inferior vena cava; Ur., ureter; Aot., aorta; P.C., pelvic colon; Ant.C.N., anterior crural nerve; Ps., psoas muscle; P., cut edge of pelvic peritoneum: A., femoral artery; V., femoral vein; U., uterus; Pt., Pectineus muscle; R., rectus muscle; Obt.Ves., obturator vessels and nerve; * * positions of the saw cuts in the pubic bones.

they have already been partly divided. The operation is concluded by a suture of the inguinal wounds.

Anatomical Considerations.—I have made several preparations which illustrate the anatomy of the operation (fig. 3). The line of the chief incisions—viz., those through the pubic rami—pass through the pectenei muscles, but do not involve any important structures. The obturator vessels and nerve lie well on the outer side of the section. If an important accessory obturator vessel is present, it will be easily seen and felt behind the pubic ramus, where it can be ligatured. The bladder and urethra are moved forward with the bodies of the pubic bones, and are not disturbed. The fracture of the rami of the ischia merely allows of a hinge movement without any separation of the bones. This is ensured by the very firm connection of the bones round the obturator foramen, by means of the strong obturator membrane, both obturator muscles and the periosteum on the deep surface of the ischia. It is perhaps scarcely necessary to point out that the whole operation is extraperitoneal.

Clinical Aspects.—As compared with the other operations which by a division of the symphysis, or pubic bone, enlarge the brim of the pelvis, this procedure has three notable advantages—viz.: (1) The gain in the true conjugate at the brim is $\frac{3}{4}$ in. to 1 in., instead of merely $\frac{1}{4}$ in.; (2) the ring of the pelvis is not opened laterally, so that there is no injury to the sacro-iliac joints; (3) the result obtained is a permanent enlargement of a flat pelvis, and not merely a temporary expedient, to facilitate a single difficult labour. This last point far outweighs all the others, because if successful it is nothing short of a radical cure of the deformity, which is the cause of the difficulty in labour.

If I had the opportunity of doing so, I would prefer to do the operation before pregnancy had occurred at all. Or if a patient already pregnant was under my advice, I should advise its being done at the end of the eighth month, so that if premature delivery was caused by it, the child would be almost mature, but otherwise the pelvic incisions would have a month in which to heal before delivery. Lastly, if the patient was already in labour, I would not hesitate to perform the operation, which in principle is no more complicated than a pubiotomy. The ideal case for its performance would, of course, be the case of a woman with a flat pelvis, with a conjugate diameter of $2\frac{1}{2}$ in. to $3\frac{1}{2}$ in., who had already lost one or more children, and who desired to have a family without the risk and ordeal of undergoing repeated Cæsarean section.

DISCUSSION.

The PRESIDENT said that the Section was indebted to Mr. Hey Groves for this original suggestion of an operation, which, if feasible, might prove of considerable obstetrical importance. It was always difficult to discuss the merits of procedure on theoretical and anatomical grounds alone, but from this demonstration it would appear that this operation might be performed with safety and facility.

Dr. AMAND ROUTH congratulated Mr. Hey Groves upon his very ingenious operation, which was somewhat allied to a double pubiotomy, and like that operation was intended to procure a permanent enlargement. He doubted, however, if this would be so, as much callus would be thrown out. For this reason he thought it would be inadvisable to operate before or during pregnancy. If no callus were formed the loosened fragment might be elevated by the recti muscles, and as the ring of bone forming the pelvic brim was deficient, it might happen that the gap would in time be reduced in size, and the pelvis would then become actually more contracted. If no firm union between the fragment and the pelvis resulted he feared locomotion would be subsequently impaired. If the operation were done during labour it was almost certain that the outer cut edges of the pubis would be further separated during the passage of the head, and if so the loosened fragment would slip back and resume its original position. If the operation should prove to be of practical value he thought the loosened fragment of the pubes should not be displaced forward till after the birth of the child. He feared, however, that the operation would prove no more advisable than a double pubiotomy, which had many disadvantages.

Dr. MAXWELL congratulated Mr. Hey Groves on the ingenuity of his suggested operation, but thought it lay open to serious criticism. He wished Mr. Groves could have given the meeting some accurate observations on the increase in superficies of the superior strait that resulted from this novel form of enlargement of the pelvis as compared with that following a median or unilateral bony section. This, after all, was the point of greatest importance in operations of this type. Another argument against the operation was the question of excessive callus formation, a point which had not escaped the notice of those who had followed the after-histories of cases of unilateral pubiotomy. To prevent injurious callus formation it would be necessary to secure the immobility of the patient for many weeks. Again, he could not accept an extensive compound fracture of the pelvis as a substitute in any circumstances whatever for Cæsarean section of classical or modified technique. Finally, granted that a considerable increase in pelvic dimensions resulted from this bilateral section of the pubes, would it ever be safe to employ extraction by version or forceps with these two severed ends of the horizontal rami of the pubes projecting into the pelvic basin, and inviting perforation or laceration of a thinned-out vagina and lower uterine segment if any resistance to delivery occurred.

Mr. HEY GROVES, in reply, stated that he did not consider that there was any reasonable ground for anticipating the formation of excessive callus, because there was neither the contact of divided bone surfaces, nor any source of irritation, such as mobility between the fragments. From a mechanical point of view the two essentials of success were the correct inclination of the saw cuts in the pubes and the minimal division of the ischia. If these two points were carried out the bone would become firmly locked in its new position, where it would be securely held by all the structures which pass across the obturator foramen.

Six Cases of "Utriculoplasty" for Uterine Hæmorrhage, one of which was followed by Pregnancy and Labour.

By VICTOR BONNEY, M.S.

WITHIN the last ten years a class of case has been generally recognized in which severe uterine hæmorrhage, intractable to treatment by drugs or curettage, co-exists with a uterus neither much enlarged nor the seat of tumour formation. This condition, known as "hæmorrhagic metritis," "chronic fibrotic metritis," "fibroid metritis," and other similar terms, has up to now been chiefly treated by hysterectomy.

In the *Transactions of the American Gynæcological Society* for 1909 appeared an article by Howard Kelly, entitled "Amputation of the Corpus to preserve the Menstrual Function." The American surgeon practises two operations. The first consists in a transverse amputation of the uterus through the corpus sufficiently high to preserve enough mucosa for the continuance of menstruation. The second consists in the excision of a wedge-shape piece of the whole thickness of the uterine wall, having its base at the fundus and its apex at or near the internal os. The two halves of the remaining portion of the uterus are then sutured together in the middle line so as to form a miniature uterus, or, as Kelly terms it, a "utriculus," whereby a portion of the corporeal mucosa is conserved sufficient for the continuance of menstruation, but not large enough to allow of excessive loss. It is with the second of these two operations that this paper is concerned.

In September, 1909, shortly after perusing an epitome of Kelly's paper, a case came under my care, which appeared suitable for this operation, and since that date I have performed it on six occasions.

TECHNIQUE OF THE OPERATION.

The technique I have employed appears identical with that of Kelly, whose detailed account I have only lately read. The abdomen being opened, the uterus is pulled up, and the area of contemplated excision is marked out with the point of the scalpel. This area, which includes the whole thickness of the uterus, is wedge-shaped, having as its base the whole breadth of the fundus, nearly as far outwards as the tubo-uterine junctions, whilst its apex is on a level just above the upper limit of the movable peritoneum on the front of the uterus. The uterus being steadied, the demarcated area is then excised with the scalpel. More or less oozing from the cut uterine wall ensues, but the bleeding is usually controlled by the pressure of the sutures subsequently introduced and tied.

The excision as I have performed it in all but one of my cases leaves in each moiety of the remainder of the body of the uterus a strip of mucous membrane continuous above with that lining the corresponding Fallopian tube, and below with that lining the cervical canal. If, however, as in the exception mentioned, the area of excision be extended rather more outwards, the whole of the upper part of the corporeal mucosa is removed, so that subsequently no communication exists between the uterine cavity and the tubes, and the patient is permanently sterilized. Before proceeding to apply the sutures the condition of these strips of mucous membrane is examined, and if the endometrium is thickened or otherwise abnormal they are curetted with a sharp Volckmann's spoon. I have done this in three of my cases. The ease and precision with which the curettement is effected is in striking contrast to the ordinary operation for this purpose. The two moieties of the uterine body are now united to one another, first by a row of six or seven mattress sutures, and afterwards by a continuous and more superficial suture. Any oozing points left after this are secured by additional sutures; the abdominal wound is then closed in the usual way.

The result of the operation is to leave a uterus much reduced in size, especially in breadth, so that at its upper end the tubo-uterine junctions are nearly in contact.

I have ventured to apply to it the term "utriculoplasty" as best describing its object, Kelly's expression, "vertical resection," being less definitive, while such terms as "partial excision" or "partial amputation" would render the operation liable to be confounded with subtotal hysterectomy.

CASES.

The following are the brief notes of the patients on whom I have performed this operation:—

Case I.—Aged 29; had previously borne two children. She was admitted into the Chelsea Hospital for Women, under one of my colleagues, in April, 1909, having been losing more or less continuously since the preceding February. Previously to this the menses had been very excessive since the birth of the last child four years before. She was very anæmic. The uterus was curetted thoroughly, but the loss recurred a fortnight afterwards, and continued intermittently until September, 1909, when she was re-admitted into the hospital under me, being by this time extremely exsanguined. My first intention was to perform subtotal hysterectomy, but on account of her youth I decided to adopt Kelly's procedure. Utriculoplasty was therefore carried out. Her recovery was satisfactory, except for a definite attack of broncho-pneumonia during the first week after the operation, and she left the hospital in good health. For the first three months following the operation the menses were regular and painless, but only endured for one day, the anæmia rapidly disappeared, and her general health much improved. The period in January, 1910, failed to appear, and shortly afterwards it became evident that she was pregnant. She was kept under careful observation for fear of eventualities, but none ensued. The uterus continued to enlarge normally until the thirty-second week, when premature labour came on. The child was born alive, and Dr. Osborne, who attended the confinement, wrote me that the leading feature was unusually strong pains with slow advance of the head, in spite of the small size of the child. Forceps were not required however. There was slight *post-partum* hæmorrhage. The puerperium was normal. As the patient is still nursing her child the menses have not yet returned, but when I examined her a short while back the uterus had completely involuted to the size of the utriculus, which I had fashioned eighteen months before. Her general condition is excellent.

Case II.—Aged 34, married, but sterile. Was sent to me for continued bleeding, for which she had been twice curetted in the Soho Hospital for Women without avail. She was a stout masculine-looking woman, and very anæmic. I operated in the Middlesex Hospital in May, 1910. The uterus was large, but contained no myomata. I performed utriculoplasty, having very thoroughly scraped away a thickened uterine mucosa before suturing the uterus. Her recovery was uneventful. The immediate effect of the operation was to abolish the periods altogether for six months, during which time she lost her anæmia and became robust. Since January of this year a blood-stained discharge has appeared which increased in spite of treatment up to a few weeks, since when it has declined, and is now slight and intermittent. Her general health is exceedingly good.

Case III.—Aged 32, married. She had suffered for two years from excessive and long-continued hæmorrhages, for which she had been curetted in the Grosvenor Hospital in November, 1909, and in St. George's Hospital in March, 1910, on both occasions without any effect. In July, 1910, she was admitted to the Middlesex Hospital, where I operated on her. The uterus was enlarged, but contained no myomata. A number of pelvic adhesions were present. I performed utriculoplasty, and the patient made a good recovery. I have seen her at intervals since. The periods are regular each month, lasting about six to seven days, and requiring the use of from six to eight diapers. Her general health is good.

Case IV.—Aged 39. She had been operated on for uterine hæmorrhage in the New Hospital for Women in 1908, but without success. In June, 1910, she was curetted in the Middlesex Hospital, but again the operation failed in its intent. In October, 1910, she was re-admitted under me. On opening the abdomen I found a large uterus adherent by many fine adhesions to the neighbouring parts. No myomata were present. Utriculoplasty was performed, the lining membrane being curetted, and she made a good convalescence. Since leaving the hospital she has had periods, lasting from seven to nine days, but not excessive in amount. She was a miserably anæmic, thin woman before I operated on her, and her condition leaves much to be desired still, but she is very much better than she was.

Case V.—Aged 37. She had suffered from excessive periods for some years. Two years ago she was curetted in a general hospital in London, and was better for a while, but the condition returned. In August, 1910, profuse loss began which lasted without intermission till November, when she was admitted to the Middlesex Hospital. On opening the abdomen I found a number of adhesions whose presence had not been suspected. These were broken down. I performed utriculoplasty, and as the utriculus had a tendency to retrovert, I ventrofixed it. Since the operation menstruation has been regular, and normal in amount, while her health has very satisfactorily improved.

Case VI.—Aged 30, unmarried. A very anæmic woman of the masculine type. She had had almost continuous loss for two years, for which she had been curetted at Eastbourne without success. I operated in March of this year. On opening the abdomen, the uterus, though not greatly enlarged, was found to be the seat of multiple myomata. I performed utriculoplasty, including in the wedge most of the myomatous nodules, the remainder being separately enucleated. The area of excision in this case included all the upper part of the corporeal mucosa. She has recovered in a satisfactory manner. The operation is, of course, too recent to judge its ultimate results, but it illustrates its application to a myomatous uterus.

THE VALUE OF THE OPERATION.

In estimating the value of the operation, it is necessary to compare it with hysterectomy, by which latter proceeding all the cases described might have been alternatively treated. This comparison may be discussed under three heads: (1) The maintenance of the menstrual function; (2) the possibility of pregnancy; and (3) the relative risks of the two operations.

(1) *The Maintenance of the Menstrual Function.*

As has been shown, the immediate effect of the operation in the first five cases was to relieve the patients of the excessive hæmorrhage from which they had suffered. In Case II hæmorrhage recurred slightly at the end of six months, but there seems a good prospect of an ultimate successful result. Cases I, III, IV, and V remain entirely satisfactory, while Case VI is too recent to judge of the effect of the operation. Whether these results will be permanent can only be proved by time. In the three cases published by Kelly, where the operation had been done for hæmorrhage, the patients were satisfactory at the end of three years, one year, and one year respectively.

The question as to the value of preserving the menstrual function has in this connexion to be specially considered. Experience shows that the occurrence of the menstrual loss is by no means a necessity to healthy feminine existence. Women who have never menstruated are occasionally met with, the anomaly being associated with no evil results. In others long periods of amenorrhœa from time to time occur, without obvious ill-effect to the general health. The results of hysterectomy for myomata also show that perfect health is compatible with permanent cessation of the menses. Most women regard menstruation as an unmitigated nuisance, and view the prospect of its cessation after hysterectomy with at least indifference, but more often with actual relief. On the other hand, a certain proportion of patients from motives of sentiment dislike the idea of an enforced menopause, even though it be only "apparent," because the ovaries are retained.

The investigations of A. Giles, and the experience of other surgeons, go to prove that in spite of conservation of the ovaries, the removal of the uterus does in a good number of cases bring on the flushings and nervous symptoms of the true menopause some years earlier than in normal women.

It may be asked how the preservation of a portion of a wholly diseased organ can be justified. In reply, another question may be propounded, namely, as to how far these hæmorrhagic uteri are diseased? In spite of much research it must be admitted that the criteria of "chronic fibrotic metritis," "uterine arterio-sclerosis," and other similar conditions are very ill established. Of a number of such cases that I have examined, comparatively few have furnished histological evidence of inflammatory change.

The whole process of menstruation—why the loss begins, and still more why it leaves off—stands practically unexplained, and until we have a fuller understanding of the normal phenomenon our knowledge of these cases of excessive menstruation will remain defective.

The fact that after utriculoplasty the conserved portion of a profusely bleeding uterus may revert to the performance of normal menstruation proves, I think, that the uterine wall in these cases is not always so markedly abnormal.

(2) *The Possibility of Pregnancy.*

The slight advantages to be gained by preserving the menstrual function in these cases would not by themselves weigh against the certainty of permanent cure which hysterectomy offers, unless the patient herself, particularly if comparatively young, was very desirous of avoiding the latter operation. To be able, however, to leave the patient in possession of a uterus capable of child-bearing is a great gain. That such is possible is proved by my first case. I must confess that the first time I performed the operation I greatly questioned whether it would not be better to prevent the possibility of pregnancy by ligating or removing the tubes. But I considered that the operation would be deprived of the greater part of its advantage over hysterectomy by so doing, and bearing in mind the successful manner in which the uterine scar after Cæsarean section usually withstands the strain of subsequent pregnancy, I determined to leave the tubes intact. This patient became pregnant less than four months after the operation—i.e., before the uterine scar could have consolidated—yet the pregnancy was normal, and the labour successfully accomplished. Of my four other married patients none have become pregnant since the operation. Kelly in his paper says: "What would happen in a case of pregnancy with such a reduced uterus I do not know." The case that I here put on record answers the question. The fact that the uterine body, though reduced to half its normal size, yet possesses growth-power sufficient to meet the needs of pregnancy is very remarkable.

(3) *The Relative Risks of Utriculoplasty and Hysterectomy.*

My six cases have all made rapid and excellent recoveries from the operation. Theoretically, utriculoplasty for "fibrotic metritis" or small corporeal myomata should certainly not be associated with more risk than subtotal hysterectomy under similar conditions. In either case the site of the operative field is aseptic to begin with, and infection from without can be guarded against. Inasmuch as utriculoplasty requires the use of a good deal of ligature material the strictest antiseptic precautions are necessary. There is less risk of profuse post-operative hæmorrhage after utriculoplasty than after hysterectomy, because no large vessels are divided ; but on the other hand, there is more liability to oozing between the stitches. The parietal wound should not be closed until the uterine suture line is entirely dry. Finally, the risk of pulmonary embolism, a fatality much more commonly associated with hysterectomy than with any other operation, should be very small after utriculoplasty, because no trunk veins require ligature during its performance.

CONCLUSIONS.

The cases I have recorded seem to show that the operation of utriculoplasty is a legitimate alternative to hysterectomy in the treatment of intractable uterine hæmorrhage, due to the condition variously known as "hæmorrhagic metritis," "chronic fibrotic metritis," and other similar terus, and also that it is feasible to apply it to a uterus the seat of small multiple myomata.

It would appear to be preferable to hysterectomy when the patient is still of child-bearing age, and the cervix is reasonably healthy, and especially if the patient herself objects to an enforced abolition of the menses. In the reverse of these conditions removal of the uterus is the more suitable operation, by total hysterectomy if the cervix be unhealthy, by subtotal hysterectomy in all other cases.

In the event of pregnancy following utriculoplasty it would, I think, be desirable to induce the labour some weeks in advance in order to lighten the work of the uterus.

DISCUSSION.

The **President** said that it appeared from Dr. Victor Bonney's remarks that he resorted to the operation mainly in cases of what was termed "fibrosis of the uterus." The great difficulty here was determining beforehand the pathological condition causing the hæmorrhage. Take, for example, an adenomatous uterus. There was no evidence from the sections of any of the excised portions of uterus what was the pathological change present in these cases. Also, in multiple small myomata, these nodules were generally scattered throughout the uterus, and not confined to any one portion. In hæmorrhage from such a cause he (the President) would prefer myomectomy or hysterectomy, if enucleation were inadmissible, to utriculoplasty.

Dr. **Arthur Giles** said that he was greatly interested in Dr. Bonney's description of his cases of utriculoplasty. It seemed to him that the chief difficulty was to decide in what cases the operation was indicated; if one could be sure that the case was one of fibrosis, he thought the operation would not be advisable because, in his experience, the leaving of even a small remnant of the body of the uterus might be sufficient to keep up the hæmorrhage; with uterine fibroids, also, he would hesitate to adopt this procedure on account of the risk of the development of other fibroids later on. He would be glad to know if Dr. Bonney had particulars of the microscopic condition of the portion of the uterus removed in his cases.

Dr. **Griffith** said that the successful result of Dr. Bonney's first case, which was followed by the birth of a living child, was a sufficient justification for the operation, and might be hoped for again in younger women who suffered from intractable uterine hæmorrhage, and in whom there was no evidence of fibroids. In the latter condition he still thought supravaginal hysterectomy was the right treatment, otherwise minute fibroids might develop and necessitate a further operation.

Dr. VICTOR BONNEY, in reply, said the operation was a legitimate alternative to hysterectomy in those cases in which profuse hæmorrhage, unamenable to treatment by lesser measures, afflicted a comparatively young woman. As regards the explanation of its effect in arresting excessive bleeding, this was, no doubt, in part due to the reduction in size of the menstrual area that it effected. Beyond this, however, there were probably other factors. Until more knowledge of the normal menstrual loss was forthcoming—particularly as to what caused the flow to cease—any answer to such a question would be speculative.

Obstetrical and Gynæcological Section.

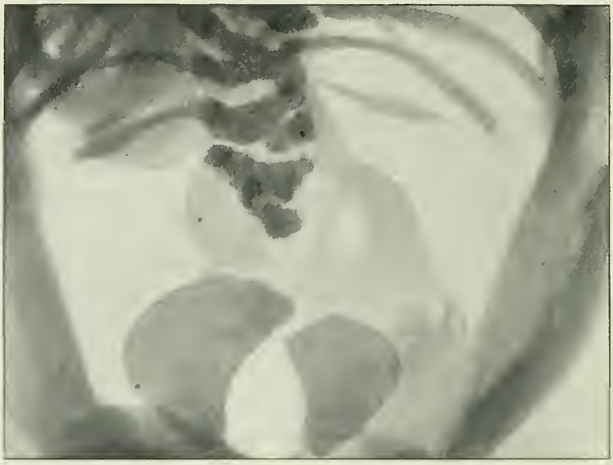
May 11, 1911.

Dr. H. MACNAUGHTON-JONES, President of the Section, in the Chair.

A Fœtus with Congenital Absence of the Sacrum.

By CLIFFORD WHITE, M.D.

THE specimen shows part of a full-term male fœtus. The mother was aged 39, and had had eleven healthy children previously. The child presented by the breech and was stillborn. It was not one of twins but was a single ovum, and there was no history of trauma during the pregnancy. The placenta was normal, but the child showed numerous abnormalities in the viscera. There was a hernia of small intestine extending 3½ in. into the umbilical cord. The large intestine ended blindly in a fibrous cord which terminated behind the bladder, the anus and rectum being absent. The kidneys were fused into a single median organ bearing two large suprarenals. The right ureter was normal but the left was only represented by a fine fibrous thread. The bladder and urethra were normal. The left testis and vas were normal, the right testis was attached to the right iliac fossa by a peritoneal fold comparable to the broad ligament, but sections of the gland showed typical testicular structure. No trace of the right vas could be found. Double talipes equinovarus was present. From the lower dorsal region a nerve ran down to the front of the thigh, which represented the anterior crural and great sciatic nerves. Microscopic sections of the thigh muscles show healthy muscular tissue to be present. The chief interest of the specimen is in the condition of the osseous system. The bones of the head, limbs, pelvic and shoulder girdles are normal. Although very irregular the vertebræ are represented down to the twelfth dorsal, but below this there are only two cartilaginous nodules, the upper measuring 2·2 cm. wide by 1·5 cm. high, and the lower 1·3 cm. wide by 1·5 cm. high; the lower piece articulates with the left iliac bone, which is thickened in the region of the ilio-pectineal eminence to receive it. The right iliac bone is not connected with the vertebral column except by ligaments. The ilia are in contact with each other posteriorly. The lower lumbar and the whole of the sacral and coccygeal vertebræ are thus absent. The radiograph shows that the lower nodule is entirely cartilaginous, but the upper contains bone.



Skiagram showing termination of spinal column above level of iliac bones, the nodule which articulated with the left ilium being entirely cartilaginous. The shadow below and to the left is from the soft parts which had not been removed when the skiagram was taken.

The case is exceptional in that the child was a single one. Most previously recorded cases (e.g., Ballantyne, Luton, Wertheim, and Litzmann) have been in one of twins.

The PRESIDENT (Dr. Macnaughton-Jones), having remarked on the very rare character of the abnormality, said that there could be no doubt of the absence of the sacrum.

Cervical Glands in the Uterus of the Fœtus at Term.

By Herbert R. Spencer, M.D.

The specimens were shown in fulfilment of a promise made at the October (1910) meeting of the Section.[1] At that meeting, in the discussion on a paper by Dr. Robinson, on "Vaginal Secretion in Infants," Mr. Glendining made the statement that "he did not think that the change in reaction could be accounted due to the mucous secretion from the glands of the cervix, as in the fœtus at term there were no glands to be found." "Dr. Herbert Spencer was astonished at Mr. Glendining's assertion, as he had often seen well-marked cervical glands, sections of which he would bring before the Section."

The first two slides were longitudinal sections of the entire uterus of a stillborn child at term. The uterus was anteflexed, the angle formed between body and cervix being somewhat less than a right angle. The portio vaginalis was considerably less than half the length of the cervix, had the usual papillary appearance, and was covered with stratified epithelium, which extended up the canal for about 3 mm.; above this the cervical canal, about 12·5 mm. in length, showed on the slide as a slit from which passed off at right angles, or obliquely, well-marked glands, about a dozen in each wall. The glands were shallow just above the stratified epithelium, but towards the middle of the cervix, and at its upper part, they penetrated deeply and were coiled, or branched, or racemose. The longest of the glands were over 1 mm. in length with ducts just visible to the naked eye. They were lined with columnar epithelium. The photographs rendered any further description unnecessary (fig. 1). They demonstrated structures which were as truly glands as those met with in the adult.

After stating that there were no glands to be found in the cervix of the fœtus at term, Mr. Glendining added that "he had examined a number of cervices of children at term, and had never encountered glands *beneath the squamous epithelium*." (The italics were Dr. Spencer's.) Whether this meant that Mr. Glendining had confused the portio vaginalis with the cervix, or did not know that the stratified epithelium only extended a short distance up the cervical canal, or what bearing it had on the question at issue, the speaker did not know, but as a reason for stating that there were no glands in the cervix it was entirely inconclusive.

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[1] *Proceedings*, p. 31.

Dr. Spencer was fortunate in being supported in his statement by Professor Bayer, of Strassburg, who had written by far the most important work known to the speaker on the anatomy of the uterus at different ages. Professor Bayer said that in the cervix of the newborn child widely branched glands were always present ("in der Cervix sind stets weitverzweigte Drüsen zu konstatiren")[1]; he also gave numerous illustrations of the glands, both at birth and for nearly every year up to puberty.

Dr. Spencer took this opportunity of showing a section of cervical glands from a newborn child in which the epithelium was several layers thick (fig. 2). This was not due to obliquity of the section, but to the gradual proliferation of the epithelium from the surface down to the recesses of the glands. Dr. Spencer first called attention to this condition at a meeting of the Obstetrical Society thirteen years ago,[2] as possibly the origin of some rare benign polypi in adults in which the glandular epithelium was similarly many-layered, but lacked the wild irregular proliferation of malignant disease. Professor Bayer (loc. cit.) stated that this proliferation of the epithelium sometimes in the newborn child formed a vacuolated syncytium-like mass.

Dr. Drummond Robinson said that he had been surprised to hear it stated that the cervix of the newborn child could not secrete because it contained no glands, for two reasons:—

(1) He had always been under the impression that the cervix of the newborn child did contain glands.

(2) Even if there were no definite glands, it did not follow that there was no secretion. The vagina of the adult contained no glands, and yet the vagina produced a secretion in considerable quantity.

He proceeded to show on the epidiascope sections from a sixth-month and a seventh-month fœtus. Structures were seen in the lining of the cervical canal which had the appearance of glands, and he presumed that they were such.

Mr. Glendining said that he still continued to deny the presence of glands in the cervix of a fœtus at term. The idea that glands existed

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[1] "Allgemeine Geburtshülfe," 1908, i, p. 92, and pl. xi.

[2] *Trans. Obstet. Soc. Lond.* (1898), 1899, xl, p. 336.

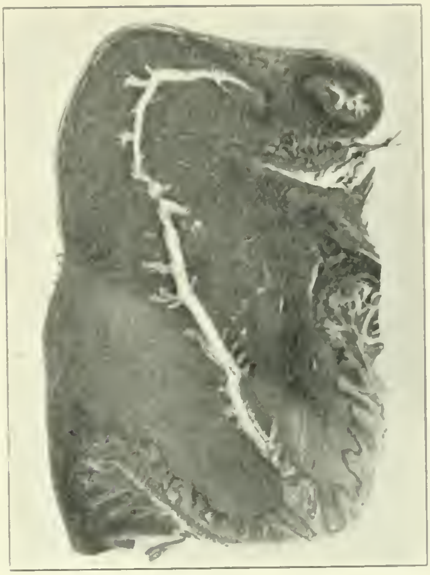

FIG. 1.

Dr. Spencer's specimen of the uterus of a newborn child (× 4). The top of the figure is near the internal os (the uterus being acutely anteflexed) and the fundus at the right upper corner. Numerous glands are seen opening into the cervical canal. In one of the coiled glands no less than four turns have been cut across.

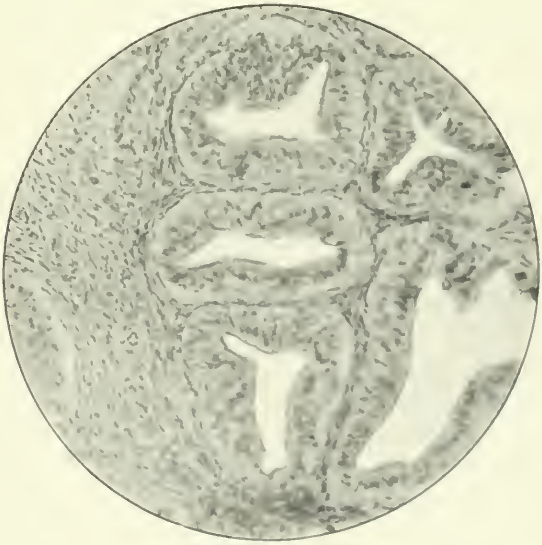

FIG. 2.

Photomicrograph of the glands in the cervix of a newborn child, showing the epithelium in several layers (high power).

at that period arose, he believed, from failure to understand the method of development of the cervical canal, so that evident and complicated folds were readily mistaken for glands, but were in reality transitory folds.

He showed a section of a fœtal cervix in the sixth to seventh month of gestation, in which the folds were simple and as well developed as in the cases shown by Drs. Robinson and Spencer. Next were shown sections of a cervix of a fœtus at term, in which the sections were in some cases cut longitudinally and in other cases transversely. Some of these had been mounted and examined serially. The result showed an eminently complicated plicating of the cervical canal, to which it seemed almost absurd to deny the title of "glands"; and yet it would be seen that these apparent "glands" of one section, when traced through further sections, were found to spread out upon a widening surface of the canal lining. In other places such was the abundance of folding that a strikingly papillomatous condition of the internal surface resulted. He next showed the cervix of a child aged 2 in which the complicated foldings (the normal condition of the cervix at term) had in a large measure disappeared, and were replaced by crypts with a definite opening upon a plane surface, and which showed in their deeper parts small buds, the early stages of ramification.

The review of the preparations of the cervix at these three stages of evolution alone demonstrated, he thought, that glands did not exist in the cervix at term, but if accessory evidence were necessary it would be found in the staining reactions to muci-carmine, which were in brief the following:—

In the fœtus (1) at the sixth month there was no definite selection of carmine on the part of the cells; (2) at term the reaction was indefinite; (3) at two years the crypts and their small buds stained a bright pink colour, while the surface epithelium was less brightly stained.

Lastly, the explanation of the divergence of opinion obviously lay in the failure to recognize the method by which the canals of the body became of larger lumen by proliferation and downgrowth of the surface epithelium, and the consequent irregular infolding of the surface that resulted, and which, when once the natural size had been attained, all subsided and left the normal natural lumen. The same process was to be seen in operation in the bladder and vagina of certain sections shown. In the cervix this proliferation happened to occur at about term.

DISCUSSION.

The PRESIDENT said that he had always taught that there were glands in the fœtal cervix, and certainly, in Klein's and Noble Smith's splendid "Atlas of Histology," published as far back as 1880, these glands were described. He did not think that Mr. Glendining had satisfactorily proved that glands were not present.

Dr. HERBERT SPENCER, in reply, said that Mr. Glendining's demonstration on the screen of early fœtuses, fœtal bladders, transverse sections through the uterine cavity and sections of epithelium of the lower part of the cervix, had no bearing at all on the question in dispute, which was whether glands did or did not exist in the cervix of fœtuses at term. The speaker said they did exist and showed them, and was supported by the high authority of Professor Bayer. Mr. Glendining had not shown a single section of the whole of the cervix at term, but stated that they did not exist—but that they were folds, not glands. Everyone knew that the glands were developed by infolding of the epithelium, and most gynæcologists knew that folds of the vagina, portio, cervix and corpus existed in the newborn child, and were obliterated as the child grew. He (the speaker) had called attention to these folds, and had illustrated them. He had also directed attention at the same time to Friedländer's observations on the cervical folds.[1] If Mr. Glendining would cut longitudinal sections of the whole uterus through the middle of the organ where normally no transverse folds existed, he would find the glands still present. He proposed that the Pathology Committee should express an opinion as to whether the coiled and branched structures he had shown were glands or not.

Ovarian Teratoma.

By J. E. GEMMELL, M.B.

THE specimen consists of the ovary, tube and part of a cyst containing broad ligament from the right side, the ovary definitely separated from all other structures. The larger of the cysts in the broad ligament, emptied before removal from the abdomen, contained 6 pints of straw-coloured fluid, alkaline in reaction, specific gravity 1022, copious albumin, no sugar, no mucin, no pseudo-mucin. This points to simple œdema, and absence of a lining of the cyst cavity confirmed this.

The pedicle of the specimen, which was removed in one piece, measured 3¼ in. in length and 1 in. thick, and included the uterine end of the Fallopian tube and the round ligament. It was very œdematous, as was the tube and all the broad ligament and ovarian ligament separable from the cystic areas. The Fallopian tube was 1½ in. long, the fimbriæ prominent, but there was no marked congestion. Cystic areas: The larger, rising well into the abdomen from the posterior wall; the smaller (3 in. by 3½ in. by 2 in.) from the anterior wall; both sessile and peritoneal except for the presence of a few bands of fibrous tissue, coursing over the surface. There were a few very small peritoneal cysts to be seen near the ostium of the tube. The ovary, somewhat pyramidal in shape, measured 3½ in. by 2¾ in. by 2¼ in., and circumference along the free border 5½ in. In outline there was a distinct likeness to a small fœtus, with cranium, back, and lower limbs. The surface is pale and irregularly nodular, with small follicular cysts on the surface, especially towards the hilum, where they are numerous and prominent, making the consistence boggy. As a whole the ovary is hard, with close to the surface and opposite the middle free border a still harder object, rounded in outline, and somewhat egg-shaped. This reached to ⅛ in. of the surface, lay obliquely in the ovarian substance with the broader end more central. The ovarian substance was not attached to it over the dome, but was adherent at the sides and in its deeper aspect, where it was firmly adherent to the stroma surrounding. This body had a close resemblance to a fœtal vertex, was smooth and hard, especially at the broader end. Calcification was present over all, but more so at its broader end, where a distinct resemblance to occipital and parietal sutures could be made out. The inner wall of this closed cavity was quite smooth; the contents, which almost filled the cavity, were a yellowish material of the consistency of thick gruel.

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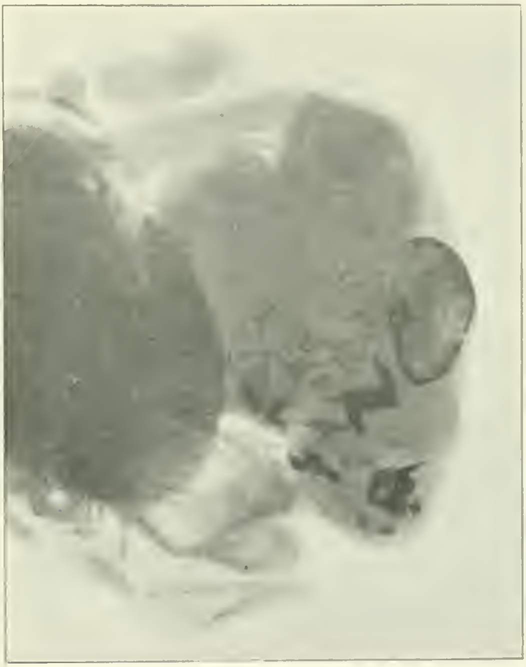
[1] *Trans. Obstet. Soc. Lond.* (1898), 1899, pp. 336, 337.

Dr. Hugh MacLean, of the Biochemical Laboratory, University of Liverpool, examined this material and reported: "When this substance was dried there was so little remaining that it was impossible to institute very elaborate tests. It was, however, found to contain a marked amount of cholesterol, and a substance soluble in 80 c.c. alcohol and falling out in the cold. This substance in its general properties resembled 'protagon' or some similar brain lipoids. On boiling it with weak sulphuric acid, a very marked reducing sugar was obtained giving a very distinct reaction with Fehling's solution. Since it is known that protagon contains sugar as one of its constituents, there is strong evidence that the substance obtained was really brain lipoid, and almost certainly the material itself was brain. I know of no other organ that would furnish a lipoid of this nature; so the material may be held to be of a nervous origin and nature."

The calcified membranous sac in which this substance was found measured 1 in. long by ¾ in. broad. Towards one pole of the ovary was found a peculiar rounded structure enclosed in a firm fibrous capsule which was full of a soft gelatinous substance, seen under the microscope to be thyroid gland tissue of an adenomatous type. The other pole of the ovary was occupied by an irregular deposit of calcareous nodules and masses without any definite arrangement but resembling fœtal limbs. The X-ray photograph, for which I am indebted to Mr. Thurstan Holland, shows these well, but there is no evidence of ossification in any of these areas. Interspersed between these calcareous nodules was a further deposit of jelly-like tissue, which under the microscope is seen to be thyroid gland tissue.



Skiagram showing outline of fœtal skull and calcareous tissues without definite formation but, as a whole, giving outline of a fœtus.

This specimen, therefore, is a compound ovarian teratoma, with an attempt at the formation of a skull cavity, with brain in that cavity and with thyroid formations and deposits of lime.

Remarks.—The teratomata or embryomata are so classed because they contain more or less abundantly structures similar to those found in the embryo. Such tumours are generally cystic, and may be simple or compound, where, in addition to epidermal tissues, there are others derived from the mesoblast and hypoblast. They are usually unilateral, are of slow growth, remain latent for a long time, and give rise to symptoms first during the period of sexual activity. The ovary is the commonest seat of teratomata, and in the compound forms have been found all the more complicated structures, such as mucous membrane, glands (mucous and salivary), muscle, nerve, brain substance, rudimentary eyes, fingers, mammæ, ribs, &c. It is very rarely that any proportion or relationship of parts is preserved, but there are recorded cases with development of extremities and genital parts, so that a definite fœtus may be inferred. This specimen I believe to be unique as being the only one in which there has been found definite brain tissue enclosed in a bony cavity. Further tissue resembling nerve and brain substance has been recorded in many instances in *malignant* embryomata; this case was undoubtedly innocent.

In conclusion, I wish to thank Dr. Leith Murray for his dissection and microscopic sections, Dr. MacLean for the analysis, and Mr. Holland for the skiagram.

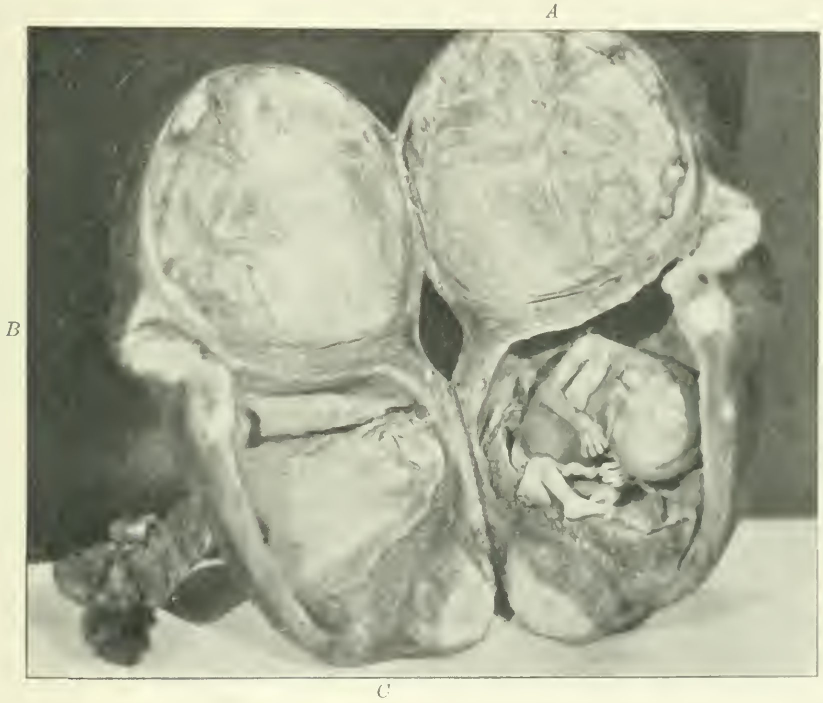
BIBLIOGRAPHY.

Adami and Nicholls. "Principles of Pathology," Philad. and New York, 1911, ii, p. 909.
Bland-Sutton. "Surgical Diseases of the Ovaries and Fallopian Tubes," 1896.
Frank. *Amer. Journ. Obstet.*, New York, 1907, lv, p. 348.
Hicks and Targett. *Journ. of Obstet. and Gyn. of Brit. Emp.*, 1905, viii, p. 84.
Neuhäuser. *Archiv f. Gynäk.*, Berl., 1906, lxxix, p. 696.
Williamson. *St. Bart. Hosp. Reports*, 1907, xlii, p. 113.

Retroflexion of Gravid Uterus with Superimposed Fibroid.

By C. E. Purslow, M.D.

The patient, aged 39, had been married twelve months and was three months advanced in her first pregnancy. One week before admission to hospital she had been seized with severe pains in the lower abdomen, vomiting and pyrexia. She had been kept in bed and these symptoms had gradually subsided; there had also been frequent and painful micturition. On examination of the abdomen there was a hard symmetrical tumour reaching to the umbilicus; this was diagnosed as a fibroid. On examination *per vaginam* there was a soft swelling, typical of the retroflexed gravid uterus, filling the pelvis, the cervix was high up behind the pubes, and only the posterior lip could be reached by the examining finger. At the lowest part of the soft mass formed by the fundus uteri a small hard fibroid nodule could be felt: red degeneration of the larger fibroid was suspected. Abdominal supravaginal hysterectomy was performed, and the patient made an uninterrupted recovery.



Sagittal mesial section of the specimen. *A*, summit of tumour, near umbilicus; *B*, external os, at level of pubic crest; *C*, fundus uteri, in Douglas's pouch, near perineum.

A sagittal mesial section of the specimen has been made and this shows well the retroflexion of the uterus; the cervical plug of mucus is particularly well seen. There is commencing red degeneration of the larger fibroid.

Photographs of the specimen before and after section are shown, also a skiagram through the whole specimen which displays the outline of the fœtal skeleton.

DISCUSSION.

Mr. TARGETT said he had operated on a similar condition some years ago, but in that case the fibroid was so much softened that the relation of the pregnant uterus to the fibroid was mistaken. Subtotal hysterectomy was then performed, but subsequent experience had shown that myomectomy was specially suitable for these cases, as it often did not disturb the gestation and so enabled the patient to have one child. Hence, in a first pregnancy he thought it was worth while for the patient to take the risk of a second operation (hysterectomy) for the sake of the child.

Dr. McCANN said that it was now well established that myomata could be successfully enucleated from the pregnant uterus, and he had performed this operation on six patients without interrupting the pregnancy. However, on carefully examining Dr. Purslow's specimen the myoma was found to be attached low down to the cervix and lower uterine segment; and for that reason he regarded hysterectomy as the correct treatment in his case. The position occupied by the myoma in relation to the cervix and lower uterine segment was a most important factor in deciding what operation should be attempted, for the severity of the hæmorrhage resulting from ineffectual attempts to enucleate the tumour might seriously jeopardize the patient's life, and hysterectomy would then become more dangerous. When the myoma is situated away from what may be termed the "dangerous area," enucleation is an operation which gives excellent results.

Dr. DRUMMOND MAXWELL recalled that several years ago Dr. Russell Andrews had brought before the Society a case where the physical signs and symptoms were almost identical to the present case.[1] In both the uterus was markedly retroverted by a large fibroid situated on the anterior wall, but in Dr. Andrews's case retention of urine was the symptom of urgency. Dr. Maxwell asked why myomectomy had not been at least attempted, since the toleration of the pregnant uterus to this operation was universally recognized. This operation had been carried out successfully in Dr. Andrews's case, the patient subsequently going to full term; the tumour removed being practically twice the size of the present specimen. No one would recommend the removal in addition of the smaller fundal fibroid (in the present specimen), which could not be regarded in any way as a danger to subsequent labour.

The PRESIDENT drew attention to the fact that the fœtal sac was placed between two fibromata, the larger one above and the smaller one below. This would, of course, influence the decision as to myomectomy.

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[1] *Trans. Obstet. Soc. Lond.* (1906), 1907, xlviii, p. 313.

The Uterine Mucosa in Menstruation and Pregnancy; the Action of the Chorionic Cells and the Function of the Decidua.

By James Young, M.D.

STRUCTURE OF THE UTERINE MUCOSA AND THE CAUSE OF THE OPENING-UP OF THE VESSELS DURING MENSTRUATION.

To understand the manner in which the maternal tissues react to the stimulus of the engrafted ovum, a clear conception of the minute structural features of the uterine mucosa, especially of the vessels and the interglandular connective tissue or stroma, is essential. In the resting state the stroma consists of a mass of cells densely packed together and intimately united with their neighbours by means of protoplasmic strands. This description is the one found in the text-books. The blood-vessels, with which the mucosa is especially richly supplied, have a flattened intima, and, in vessels thicker than capillaries, a varying number of layers of concentrically arranged supporting elements.

But this description is incomplete; a more intimate acquaintance with the finer structure is necessary. As I have elsewhere shown, this is obtained by a study of the changes which occur in menstruation. In the premenstrual phase the mucosa is thickened chiefly by a free œdematous escape into the stroma, the cells of which are spread apart. There is also some blood-escape in the proximity of the vessels, which are often markedly expanded. As usually described this fluid- and blood-escape is dependent upon the fact that before and during menstruation there occurs a congestion of the uterine vessels, an increase in the intravascular pressure, and a mechanical squeezing of the blood fluid and cells out of the vessels by a process of diapedesis or subsequent to a rupture of the vessel wall.

This explanation has for long been accepted. A careful scrutiny of the microscopic changes reveals its insufficiency. I have already published a preliminary account of this part of my investigations.[1]

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[1] Proceedings of the British Medical Association, 78th Annual Meeting, London, *Brit. Med. Journ.*, 1910, ii, p. 1214.

In the plates which I show representing vessels in the menstruating mucosa remarkable changes are seen (fig. 1). There is a swelling up of the intimal elements by the accumulation of a clear fluid in their interior, and a displacement of the nuclei and cell substance to the periphery. These changes are always found in a greater or less degree in any condition associated with an escape of the vessel contents, and always in the premenstrual and menstrual states. They are more marked in the vessels which I show first, where there is a thickening of the walls and a forcing into prominence of the changes because of the resistance offered to the fluid-escape. After excluding the possibility of a fatty degeneration or of post-mortem and other irrelevant changes, it is clear that they are associated in some way or other with the fluid-escape. It is seen that there has occurred all along the inner aspect of the vessel wall an active imbibition of the blood fluid, and these appearances, I believe, provide an index of the manner in which the œdema occurs, which characterizes the mucosa in the premenstrual and the menstrual phases. This belief is confirmed by a careful examination of the microscopic changes in the normal mucosa. My specimens are for the most part obtained by curettage, and their preservation is excellent.

In this section of the mucosa, which I throw on the screen, the appearances are well shown (similar appearances are seen on the coloured plate). We see that the stroma, which under ordinary circumstances consists of fairly well defined cells united with their neighbours by means of comparatively simple protoplasmic processes, has become completely altered. The nuclei are spread apart, and the protoplasm is beset with a vast multitude of rounded clear spaces of varying sizes, each separated from their neighbours by a complete sheet of protoplasm. The perinuclear portions of protoplasm are equally involved, and the condition is associated with a marked swelling of the originally small cell. The change has led to the breaking up of the protoplasm into fine films, which are sometimes only just recognizable with the highest magnification. At first sight the appearance is somewhat like a fine network formed by interlacing fibrils, and this has hitherto been the interpretation given. We now know, however, in view of the observations I have recorded, that this is an erroneous description. This so-called network, which appears under such conditions, is an index of the functional changes for which the stroma is so perfectly adapted. The fine protoplasmic processes are not filaments, but films, which completely enclose the fluid spaces. I have pointed out that the communications between the adjacent cells in the resting state are also complete protoplasmic sheets.[1] They correspond to the films separating the clear spaces in the intimal cells seen in fig. 1.

The changes are amenable to one explanation and to one only, that there has been a widespread imbibition of fluid from the blood-vessels in response to some change in the stroma. What is the nature of this change? It has been shown within recent times, chiefly on experimental grounds, that to explain an œdematous escape it is often necessary to recognize the existence of an alteration in the tissue colloids, which enhances their affinity for fluid, and this I believe is what is occurring in the stroma in response to the stimulus of the substance (probably ovarian) which is responsible for the menstrual changes. The intimate chemical changes time will not permit me to touch upon.

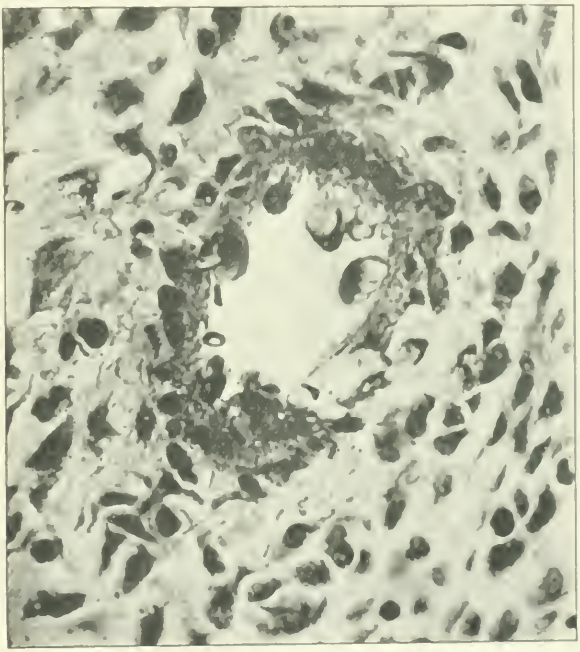

FIG. 1.

Menstruation.—Vessel in mucosa showing marked imbibition by intima. Note bulging of cells into lumen. At one place wall teased out by the fluid and a red cell is escaping. Surrounding tissues œdematous.

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[1] *See* footnote, p. 291.

The changes which I have described enable us to see the stroma in a new light. During these processes any cellular distinctions previously present become levelled and the tissue is seen to consist of a uniform protoplasmic mass. It has for long been recognized that, in connexion with menstruation, the uterine mucosa is markedly soft and pulp-like (Möricke, &c.), and the ease with which the displacement of the tissue occurs is clearly dependent on this consistence. The soft, mobile nature of the stroma protoplasm explains at once the manner in which the vessel walls are teased out and the ease with which the surrounding stroma is displaced. That the stroma consists of a spread-out and undifferentiated connective tissue is confirmed by the discovery that into its composition there does not enter any specialized form of tissue. Except in the very deepest levels there is a complete absence of elastic, ordinary fibrous and muscular tissue in the stroma and its blood-vessels. These points I have elsewhere referred to and I have shown that, for some reason, the vessels, which enter the mucosa from the muscular coat, throw off their supporting coats almost as soon as the stroma is reached. This peculiar structural feature must have a bearing on the functional changes which occur during menstruation and pregnancy.

How does the blood-escape occur? There can be little doubt that it is determined by the same factors as lead to the fluid-escape. It is easy to understand how, after the fluid opening out of the vessels and stroma the more solid elements escape, and this they do in response to the stroma alterations that tend to drag the fluid into more and more distant levels of the stroma to satisfy the increased affinity for fluid. These observations show that throughout its entire extent the stroma is a *potential blood sponge.*

Mode of Action of the Chorionic Epithelium.

Before passing on to describe the bearing of these observations on any attempt to explain the changes in the pregnant mucosa, I intend to show some slides to bring out the mode of action of the chorionic cells on the maternal tissues. An understanding of this action, taken in conjunction with the special structural features of the mucosa as revealed in the menstrual changes, will be found to provide a clue to many of the secrets hitherto wrapped in mystery. In the first place it would seem unlikely that a special adaptability, by which at any part of its surface the vessels and stroma can open out, resulting in a flushing of the mucosa with blood, should be overlooked, and, as the orthodox conception demands, that the developing ovum should obtain its blood supply by an entirely different process. The mode of action of the chorionic epithelium is easily studied in the maternal tissues which are the seat of an abnormal fœtal invasion—e.g., the pregnant tube, chorionepithelioma, &c.

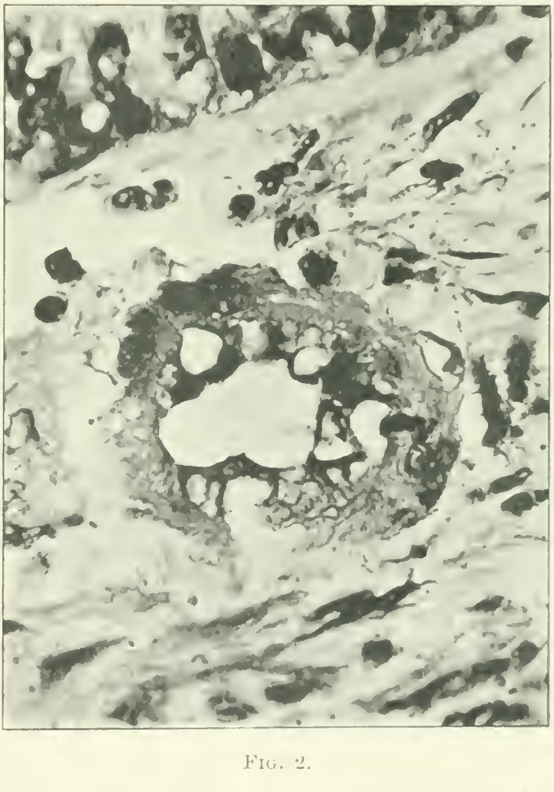

FIG. 2.

Pregnant Tube.—Vessel at considerable distance from ovum, showing mode of opening-out of wall. Note imbibition by intima, the œdematous spaces in the degenerating wall, which at one part is almost completely opened-out. Surrounding tissues œdematous.

In the *pregnant tube* there is present a marked degenerative softening. This is most marked in the proximity of the chorionic elements, where the disintegrative process often culminates in a wholesale solution of tissue. But it is also present at a long distance from the actual site of the fœtal cells. One of the most prominent changes, in addition, is a widespread œdematous and hæmorrhagic escape from the vessels throughout the tubal wall, which again, though most evident near the ovum, is always distinct at a considerable distance.

The orthodox description of the changes implies that the blood-escape, which occurs into the intervillous space, and the more distant œdema and hæmorrhage, are due to different processes. The former —i.e., the immediate cause of the opening-out of the mother's vessels— is, according to the ordinary conception, determined by a direct erosion and destruction of the vessel walls which the chorionic cells encounter in their growth. This allows of a mechanical liberation of the contained blood. That this is not so, and that the blood-escape in this condition is not dependent on two different processes, is suggested by the fact that, as the sections of the tube wall are traced towards the ovum, the blood and fluid leakage, which at the most distant levels are poorly marked, become progressively more and more evident until at the intervillous space there occurs the excessive opening-out of the vessel walls.

What is the cause of the more distant œdema and hæmorrhage? I throw on the screen a small vessel at a distance from the chorion (fig. 2). We see that its wall, which has been transformed into a homogeneous degenerating substance, is being teased out by the formation of clear œdematous spaces. These are present in and under the endothelium, which is often bulged towards the lumen. As in the menstrual mucosa, this appearance is dependent on an imbibition of the blood fluid. These changes have at one part led to an almost complete opening-out of the vessel wall. In the next slide this has occurred, and the red cells are seen to be escaping. In this section (fig. 3) changes in the connective tissue elements similar to those exhibited by the intima—namely, an active fluid imbibition—have occurred. It is easy to show that throughout the entire extent of the tubal wall there are occurring protoplasmic changes which lead to an increase in the affinity for fluid of the tissues. These changes explain the widespread œdema which is dependent on active dragging of the blood fluid into the tissues, and they explain the teasing apart of the vascular walls and the blood-escape. These changes, I believe, explain the gaping of the vessels in the neighbourhood of the ovum and the appearance which is invariably noticed—namely, that the blood cells are streaming across the spaces intervening between the vessels and the intervillous space, and this where there are no cells in the least resembling fœtal cells.

In connexion with this I wish just to touch on another point. In the menstrual mucosa it is often easy to demonstrate that the expansion

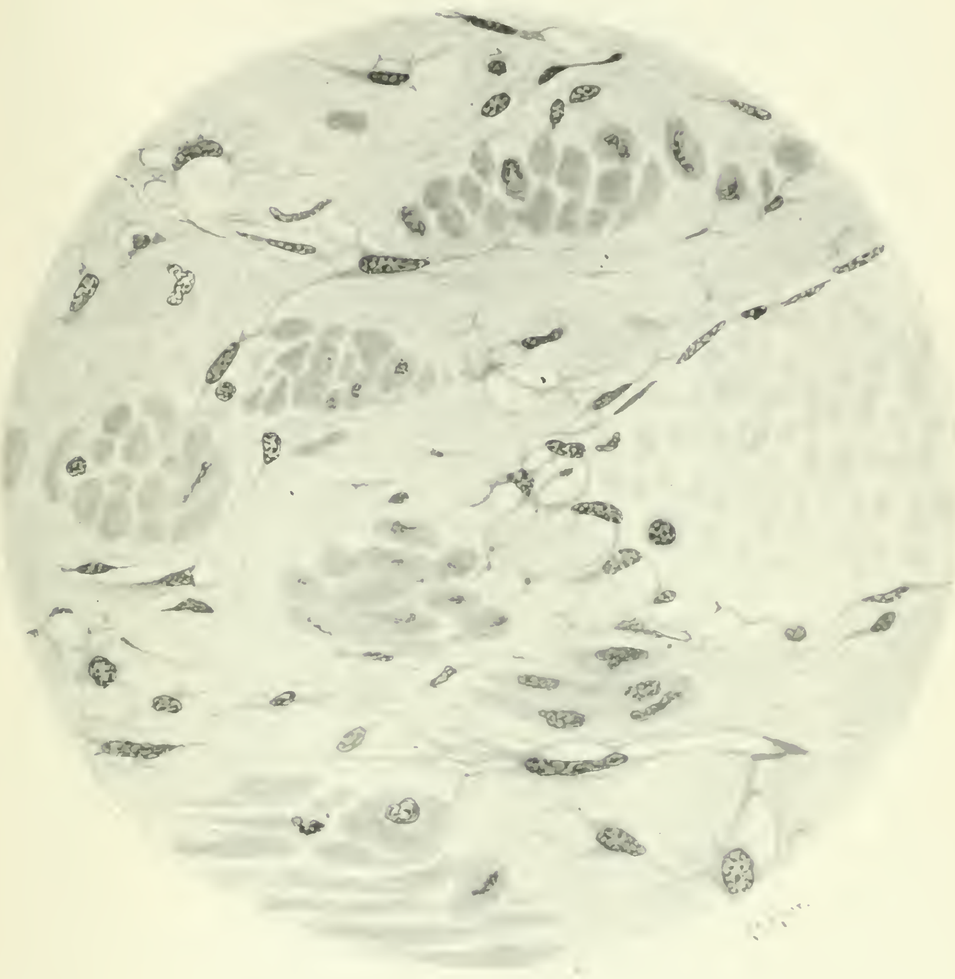

FIG. 3.

Tubal Pregnancy. Portion of tubal wall at long distance from chorionic elements, showing changes in intimal and connective tissue cells and the mode of expansion of the blood sinuses. Note the marked fluid imbibition by the cells, leading to the formation of intracellular fluid spaces, rounded, oval, or elongate on section. To the left side of the sinus the mode of expansion is seen; the displacement or disappearance of the thin protoplasmic films results in a carrying out of the lumen. The mode of formation of the new capillaries is also indicated.

of the vessels is dependent on exactly the same process as determines the blood-escape; the opening-out of the wall is due to an imbibition of the fluid followed by a displacement. In the pregnant tube the same factor, I believe, explains the sinus formation which occurs to such a marked degree. At this part we see the change in action (*see* fig. 3). It is interesting to note that in the process of expansion the connective tissue elements are embraced as a new intima. Here we see a swollen intimal cell in relation to a connective tissue cell. The disappearance of this fine film carries the lumen a corresponding degree outwards. In fig. 3 also, appearances are shown which suggest that the new capillary formation which occurs in the pregnant tube is due to exactly the same factors as determine the general fluid- and blood-escape. Here, however, the blood is being drawn along definite channels formed by an amalgamation of the intracellular fluid spaces, which are well shown. I refer to these changes because of their importance in connexion with the pregnant mucosa.

The same changes are revealed by a study of *chorionepithelioma*. Here, again, there are the same necrotic changes in the maternal tissues which, though most evident at the point of contact of the chorionic elements, are still present at a long distance. The same is true of the œdema and hæmorrhage. In this plate is shown in the upper part a mass of chorionic cells. At a little distance from this there is a free hæmorrhagic escape from a fine vessel. This is occurring through an apparently intact wall and certainly before the chorionic bud has reached it. The tissues are œdematous and in the vessel, in the lower part, is seen the process which, I believe, dominates the mechanism of the fluid- and blood-escape. The wall is swollen up to many times its wonted size, especially on the inner aspect. This is clearly due to a fluid imbibition. In the fine vessels the dragging of the fluid into the surrounding tissues in response to this change soon carries out the blood. This can occur in the thicker vessels only after their walls are teased out. But a careful study of the changes reveals without doubt that a direct infiltration of the chorion is unnecessary. In fact, so far is it from being true to state that the blood can escape only after the vessel walls are destroyed by the fœtal cells, that the converse is the case. The fœtal elements can often be seen growing in the direction of a vessel, from which the contents are already freely leaking. That the muscular elements share in the protoplasmic alterations that enhance the affinity of the colloids for fluid is shown in many places, where there is the same fluid imbibition by the degenerating

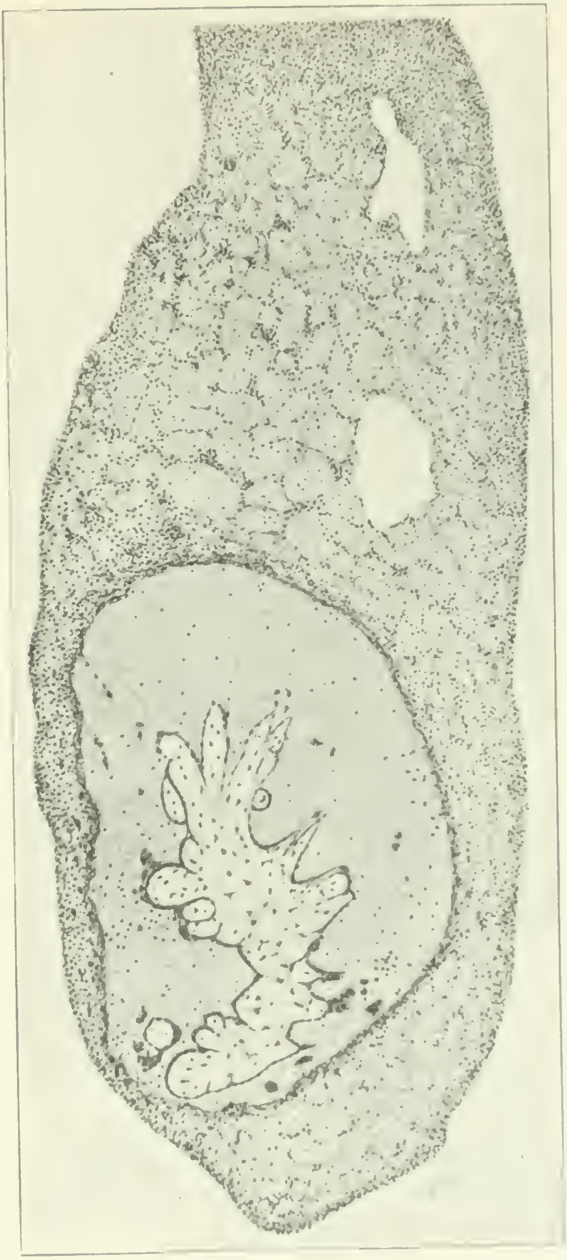

FIG. 1.

Early Human Ovum.—Elongated blastocyst lying in cavity in mucosa, surrounded by maternal blood. The surface is studded with rudimentary villi and is covered with a complete Langhans's layer and, in places, by syncytium. Marginal lamella of decidua necrotic. Mucosa opened out by fluid and blood, even at long distance from ovum. (× 65.)

EXPLANATION OF PLATE.

Early Ovum.— Section through the mucosa in the immediate proximity of the ovum (× 1,000). Above is seen the blood cavity bounded by the necrotic lamina. A degenerating cell (maternal) is seen. Note especially the changes in the stroma protoplasm. The "cells" have become swollen, some in a truly decidual fashion. The preponderating change, however, is seen to be a displacement of the protoplasm by the formation of an immense number of clear spaces of greatly differing sizes. The perinuclear protoplasm is involved in the process which is due to a widespread imbibition of fluid. Into the spaces formed red cells have passed. Between the "cells" there is the so-called network, which is seen to consist in reality of the fine protoplasmic films separating the fluid chambers. Scattered about degenerating stroma cells are seen. Similar changes are present during menstruation (*see* p. 292).

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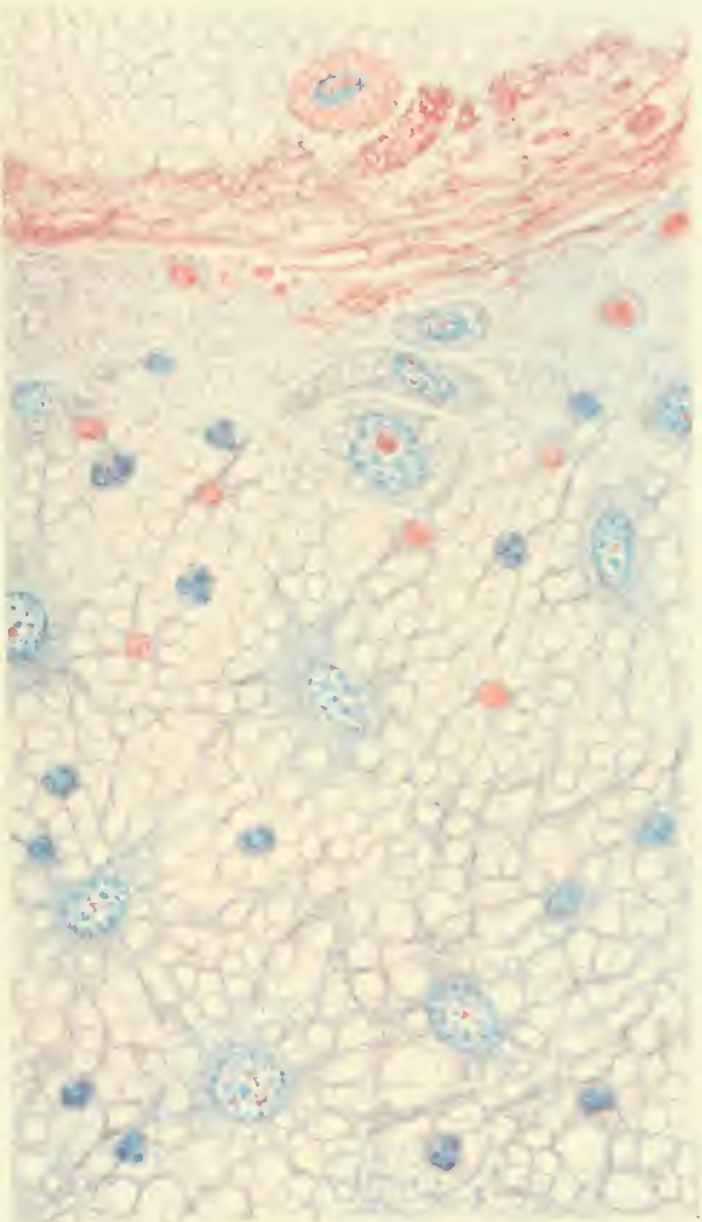

[illegible]

fibres, which in the process swell to many times their original size. In the case of the muscle, however, there is a speedy breaking-up and solution of the cell substance. This is comparatively rare in the intimal and connective tissue cells.

A study of the pregnant tube and the chorionepitheliomatous uterus, then, has shown that there is induced by the presence of the chorionic elements a widespread alteration in the tissues which leads to an active dragging out of the blood fluid and cells from the vessels. It is this change which leads to a flushing of the fœtal surface with the requisite maternal blood. It has for long been recognized that the changes in the pregnant tube, &c., all point to the existence of some bio-chemical materials or ferments, which are elaborated by the chorionic elements, and which spread into further and further levels of the maternal tissues. All the facts indicate that the changes which I have noted are caused by this secretion. A convincing proof is furnished by the extraordinary degeneration and solution of the maternal tissues which occur at the point of the chorionic contact, and by the fact that these changes spread into the surrounding tissues in ever-widening circles.

CAUSE OF THE MUCOSA CHANGES IN NORMAL PREGNANCY.

In the pregnant mucosa the blood-escape is due to exactly the same cause. Fig. 4 is a low-power section of a very young ovum, which I have recently obtained.[1] We see the irregular blastocyst with rudimentary villi lying in a cavity surrounded by maternal blood. The marginal lamella of the decidua is markedly necrotic. The maternal tissues are infiltrated with fluid and blood and this at a long distance from the ovum. The coloured drawing (*see* Plate) is a high-power section of a part of the decidua taken from the neighbourhood of the blood cavity. In it we see the deeply staining necrotic lamina, and we see remarkable changes in the enlarged stroma cells, changes similar to those present in the menstrual mucosa, and the cellular elements of the pregnant tube, &c. We see the fluid imbibition by the stroma elements as evidenced by the appearance of the clear spaces surrounded by the displaced protoplasm, and here, again, we have the appearance of the so-called network, the significance of which I have already noted. The same changes are seen in this section taken from the decidua, at some distance from the ovum, in another young specimen which I have had the opportunity of studying through the kindness of Dr. Teacher, of Glasgow. We note the intimal and stroma changes, to which I have already referred. Here we also see appearances which suggest the formation of new capillary vessels, which I believe are formed in the pregnant mucosa in exactly the same fashion as I have described in the pregnant tube.

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[1] I shall publish shortly a full description of this ovum, incorporating an expanded account of the menstrual changes and the action of the chorionic cells.

It is well known that round the young ovum one of the most remarkable changes in the mucosa is seen in the production of "distended capillaries" or sinuses formed by one layer of cells. These sometimes attain enormous proportions. They are found in all the young human ova, and they are an almost invariable condition in the lower animals. In view of what I have said, it is likely that they are formed, not by a mechanical expansion as the result of increased intravascular pressure, but in response to the tissue changes which lead to the œdema and which, throughout the stroma, are responsible for the increasing displacement of the soft protoplasm (cf. pregnant tube, menstrual mucosa). I believe, also, that the implantation chamber is formed in a similar manner.

FUNCTION OF THE DECIDUA.

If the preceding part of this thesis be proven, I have shown that the chorionic cells are concerned in the elaboration of a chemical substance, or substances, which spread into the maternal tissues and lead to the changes which result in the flushing of the fœtal surface with maternal blood fluid and cells. This escapes from the vessels, not mechanically, but is, as it were, actively drawn through the tissues towards the ovum. This chorionic substance, whilst exerting its most potent influence on the immediate maternal tissues, is continually spreading into more and more remote regions. This is seen in the pregnant tube and the chorionepitheliomatous uterus, where the maternal tissues are disintegrating and are ploughed up by œdema and blood and by the expanding blood sinuses. These changes are always marked at a considerable distance from the chorionic elements. In this plate I show a section of a vessel at a long distance from the ovum—this was situated in the ampulla, whilst the section of the vessel is at the inner end of the tube. We see the œdematous teasing apart of the supporting elements, which has so opened out the wall as to allow of a free hæmorrhagic escape. This is a change which is never found except in pregnancy, and it is amenable to one explanation, and to one only, namely, that the chorionic material has so altered the tissue constitution as to lead to the marked changes present. In these slides (figs. 5 and 6) I show sections of the pregnant tube at some distance from the chorionic elements. They are intended to bring out what I believe is the function of the decidua. We see the marked tearing up of the muscular coat by œdema, and, in places, by hæmorrhage. In the mucosa, the folds of which are turgid by a decidual change in the cells, we note a complete absence of this change. The decidual alteration has prevented it. In this slide we see the close

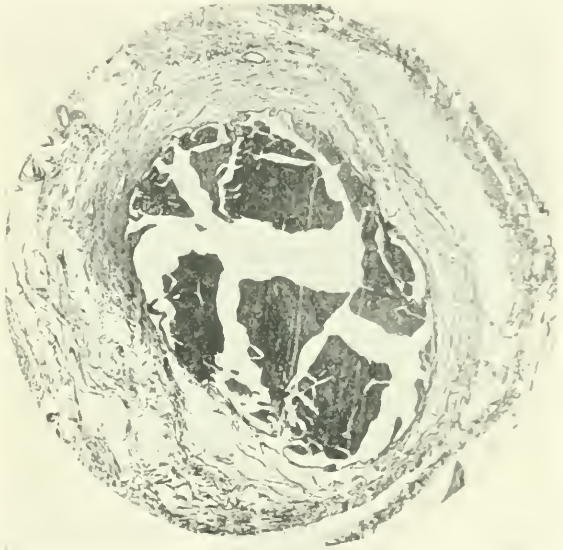

FIG. 5.

Pregnant Tube.—Transverse section of inner end of tube, in ampulla of which a two months' pregnancy had developed. Note the œdematous tearing-up of the muscular coats. To the right side and above there is a large track of hæmorrhage. The mucous folds are turgid with decidual cells. This is the only region of the tube where the œdema and hæmorrhage are absent.

packing of the cells and an absence of a fluid- and blood-escape (fig. 7).

I believe that these observations provide us with a clue to the function of the uterine decidua.

In the preceding part of this demonstration I have shown :—

(1) That the uterine mucosa is so specially adapted by the finely

adjusted constitution of its stroma as to allow of an immediate opening-out of the vessels and surrounding tissues, and a plentiful flushing of any part of its surface with fluid and blood. This is determined by an alteration in the protoplasm with an increased affinity for water. These changes I have described in menstruation.

(2) That the chorionic epithelium is endowed with the faculty of elaborating a secretion which is intended to act on the maternal tissues in such a way as to lead to these changes.

I have shown that these changes are, in the abnormal regions of chorionic activity, widespread. There is no attempt at a localization except in the decidually altered mucosa.

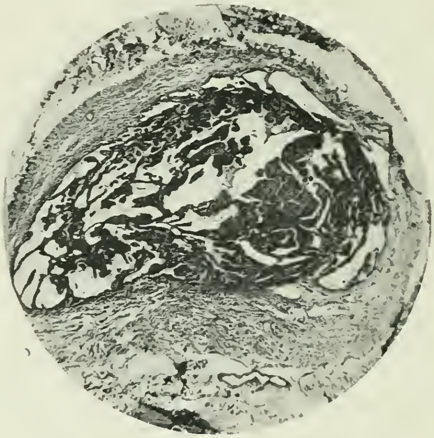

FIG. 6.

Pregnant Tube.—Oblique section through tube on inner side of gestation sac. Note again the opening-out of the muscular coats and the marked œdema round the vessels in the lower part. The mucous rugæ are swollen with decidual cells. Here there is an absence of the œdema, and an appearance resembling the decidua compacta and spongiosa of the uterus. The outer open tissue corresponds to the spongy layer, the inner more densely packed portions of the folds to the compacta. The indentations on the blood-clot present in the lumen suggest that before hardening the closely packed folds corresponded even more to the uterine compacta.

A study of the young ovum—e.g., that of Peters, Teacher and Bryce, and that which I have referred to—reveals the existence of similar maternal changes. There is an excessive opening-out of the vessels and a wholesale displacement of the stroma by fluid and blood, and by the expanding sinuses. Here, again, the changes are evident, even at a considerable distance from the ovum. In the older decidua on the other hand, these changes, as is well known, are localized to the region where they are necessary.

In view of these considerations, I would venture to assert that we are provided with a definite explanation of the decidua. Were it not for the alteration in the stroma elements during pregnancy, the uterine mucosa would, in response to the ever-widening spread of the extra-chorionic influence, be torn up by the œdema, blood, and increasing sinuses. Once the decidual change is definitely established, these changes are localized and inhibited. In view of the special structural feature of the stroma (c.f., menstruation) we can now understand the reason and the necessity for the decidual change occurring throughout the entire extent of the mucosa from fundus to internal os. In this way only we can account for the *decidua vera*. Moreover, it is the existence of maternal blood at any part which allows of a chorionic increase. By preventing the fluid- and blood-escape, the *decidua serotina* and the *decidua reflexa* prevent an excessive increase in the implantation cavity, and an uncontrolled outward growth of the chorionic elements. By this means the chorionic increase is localized to the intervillous space. This interpretation amply accounts for the absence of an excessive chorionic invasion by the chorionic elements. Any other action—e.g., a specific antagonism to the chorionic cells—is, in view of what I have said, unnecessary.

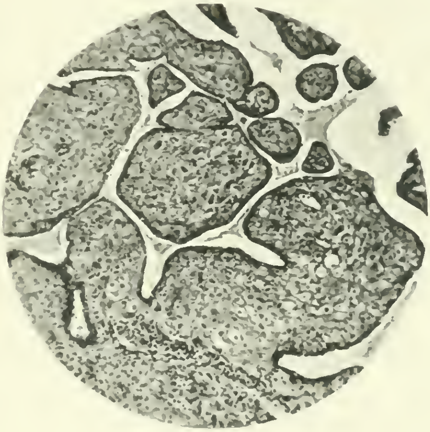

Fig. 7.

Pregnant Tube.—Mucous folds distended with decidual cells. Note the absence of œdema and hæmorrhage, and the fact that even the fine vessels present are well supported.

For much of the material employed in the investigations I am indebted to Dr. Haig Ferguson. For the early ovum I have to thank Dr. James Ritchie, Superintendent of the Laboratory of the Royal College of Physicians, Edinburgh, in which a large part of the investigation was carried out.

The PRESIDENT said that at that late hour there was not time to discuss, even if any present were prepared to do so, the conclusions arrived at by Dr. Young, which he had so fully reasoned out in the brilliant demonstration which he had given them. The thanks of the Section were certainly due to him for affording them an opportunity of seeing the beautifully prepared specimens with which he had illustrated his remarks. These would appear in full in the *Proceedings*, and the Fellows would have a better opportunity of studying the arguments which had been advanced. The labour and research required for such a demonstration were considerable. Dr. Young came from an old historic school, famed for its obstetrical research, and he might rest assured that he carried back with him the appreciation of the Fellows.

Obstetrical and Gynæcological Section.

June 1, 1911.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

After-history of a Case of Fibroid Tumour of the Uterus complicating Pregnancy.

By Arthur H. N. Lewers, M.D.

Dr. Lewers showed on March 11, 1909,[1] a specimen of interstitial fibroid tumour spontaneously expelled (February 1, 1909) from the uterus seven weeks and three days after delivery at the eighth month. It was a first confinement, and the child was stillborn. The temperature had risen to 102° F. the day before delivery, and the patient's condition was persistently febrile with occasional rigors till February 1, 1909, when a fibroid, the size of the fist, was suddenly expelled from the uterus with about 15 oz. of fœtid pus. Convalescence was at once established, and she went home on February 16. There had been no offensive discharge during the interval between the confinement and the expulsion of the tumour. If any operation had been performed, it would have had to have been abdominal hysterectomy. The patient was much averse to any operation which would prevent the possibility of a further pregnancy, and, as above mentioned, recovery occurred after the spontaneous expulsion of the fibroid.

Dr. Lewers mentioned the case in order to give the after-history. Soon after returning home the patient again became pregnant. She was re-admitted into the London Hospital under his care for the confinement. This took place on January 3, 1910, at full term, the patient being delivered of healthy twin girls. The patient left the hospital on February 19, 1910, after a normal puerperium. She was examined the day before leaving, and no evidence of any other fibroid could be found. When last heard of, about three months later, the twins were thriving.

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[1] *Proceedings*, 1909, ii, p. 229.

Carcinoma of the Body of the Uterus in a Patient aged 29.

By Herbert R. Spencer, M.D.

E. S., a single woman, aged 29, was admitted to University College Hospital on February 21, 1905, complaining of pain in the hypogastrium passing through to the back, bleeding, difficulty in micturition, and wasting.

Twelve months ago the patient began to bleed profusely every fortnight. Six months ago she had severe pains in the hypogastrium, and the bleeding became more copious. She consulted a doctor, who treated her with douches and rest in bed. As she did not improve she went to a London hospital and was admitted on December 20, 1904, and remained there till the first week in February. During her stay she was under the care of the senior obstetric physician, who curetted the uterus and diagnosed that she had a fibroid in the posterior wall. A week after leaving the hospital she was seized with severe pain, and was sent by her medical attendant, Mr. M. J. Rowlands, to see Dr. Spencer.

Menstruation began when the patient was between 12 and 13 years of age, and had been regular in time, rather scanty in quantity, and painless till twelve months ago. She had never missed a period. The bowels were constipated and the appetite was poor.

The patient's parents were living. The mother suffered from a weak chest. Two sisters were healthy; another sister died at the age of 21 of phthisis. Two or three of her mother's sisters had died of cancer, which in one of them affected the breast.

The patient was thin. Nothing abnormal could be felt in the abdomen. The cervix was small, the uterus was anteflexed, freely movable, enlarged and thickened antero-posteriorly as if by a fibroid in the anterior wall. The patient had been having severe attacks of abdominal pain. On the second night after admission to University College Hospital 10 gr. of phenacetin were given, and the patient passed a comfortable night, atropine fomentations having failed to relieve the pain. The severe attacks of pain recurred during the next two days, and suggested that the enlargement of the uterus might be due to malignant disease, although the patient was only 29 years of age. On February 27 the uterus was explored. The vulva and vagina were healthy, but the condition of the hymen and vagina indicated that coitus had repeatedly occurred. A laminaria tent inserted overnight permitted the introduction of the finger for 2 in. Hegar's dilators were then passed up to No. 17, and the curette was gently passed and brought away some soft, white, blood-stained growth, of which one piece was nearly as big as a marble. The growth did not at all resemble placenta; it could be pulped between the fingers. The finger introduced into the uterus felt a hard, irregular cavity, from which portions could be broken down. The case had all the appearances of cancer of the body: it did not look like chorionepithelioma.

On March 4, 1905, the uterus and appendages were removed by the vagino-abdominal method. The cervix was circumcised *per vaginam* by the galvano-cautery and closed by suture. Abdominal section was then performed and the uterus and appendages were separated from above by the galvano-cautery, and were withdrawn through the abdominal incision. A few bleeding vessels were then tied and a purse-string suture of silk was applied to the peritoneum. Salt solution was poured into the abdomen and the wound was closed by through-and-through stitches of silkworm gut—buried silk stitches for the fascia and an endermic stitch for the skin. The attacks of pain ceased after the operation and a normal recovery ensued, the wound healing by first intention.

The patient left the hospital quite well on March 29, and had remained well since. She had been seen and examined several times since the operation. She was last heard from in March, 1910, when she was quite well, five years after the operation.

The uterus measured 9½ cm. by 6 cm. by 5 cm. (*see* fig. 1). The peritoneum was quite smooth; the tubes and ovaries were normal. The portio was virginal, the cervical canal healthy, 1½ in. long. The whole of the anterior wall of the body, the fundus, and the posterior wall to within 1½ cm. of the internal os was occupied by a new growth which deeply invaded the anterior wall, penetrating to within 1 mm. of the peritoneum at one spot. One half of the hardened bisected uterus showed the surface of the growth to be somewhat papillary; it was degenerated, dark green in colour in places towards the surface. The growth was a carcinoma, being made up of narrow columns of epithelial cells with well-marked stroma. Extensive areas of the growth had undergone a change which at first sight gave the impression of a small round-cell sarcoma; but this was found to be due to necrosis of the growth with fragmentation of the nuclei.

The deeper-growing parts of the tumour presented the typical appearance of carcinoma, about the diagnosis of which there could be no doubt whatever (*see* fig. 2).

A section of the portion removed by the curette had been examined by the Hospital pathologist and reported upon as sarcoma. Dr. Spencer was of the opinion that this also was carcinoma, and he showed the section as an instance of the peculiar change which often occurred in tumours projecting into the cavity of the uterus (probably from œdema and infection), which caused a close simulation of sarcoma.

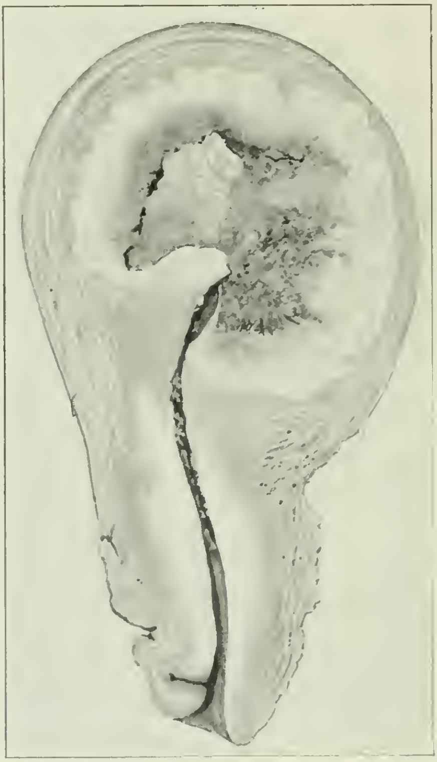

FIG. 1.

Carcinoma of body of uterus (actual size).

He showed the specimen on account of the youth of the patient, very few cases having been observed at such an early age; neither John Williams, nor Cullen, nor Lewers recording a case occurring before the age of 30, in their works on cancer of the uterus. The case also showed, he thought, the superiority of dilatation with laminaria tents over rigid dilators, and the advantage of digital exploration in cases of cancer of the body. Hegar's dilators, when used alone in such a case to dilate the cervix sufficiently to admit the finger, would cause laceration of the organ; and the use of dilators only large enough to permit the introduction of the curette might cause the cancer to be overlooked, as it did in this case in the hands of a very experienced gynæcologist.

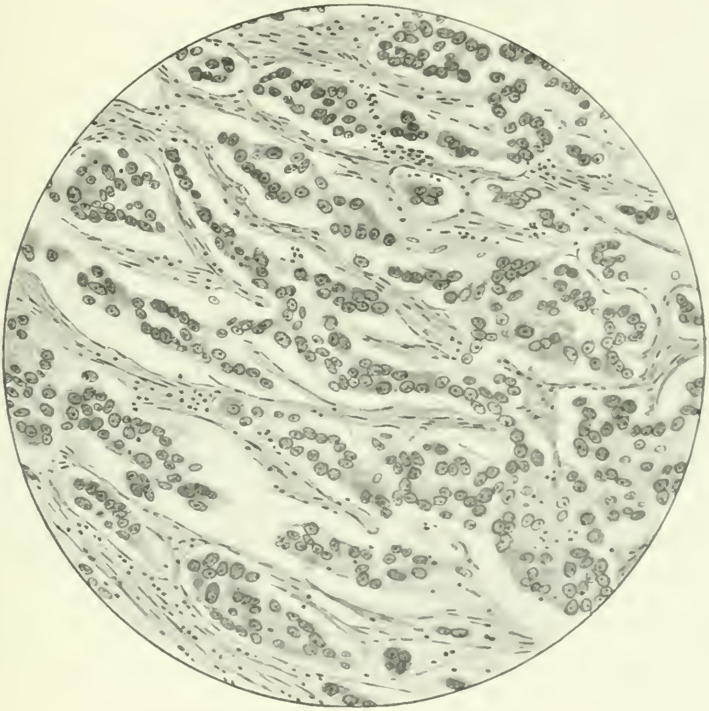

FIG. 2.

Microscopical section of growth (high power).

DISCUSSION.

Dr. HEYWOOD SMITH asked whether the patient was married or single, also whether, previous to the advent of the malignant disease, she had been under any treatments for the uterus, or had had any intra-uterine application.

Dr. AMAND ROUTH had observed this paroxysmal pain to be a prominent feature in cases of malignant disease of the body of the uterus, more especially in cases of sarcoma. He did not consider it necessary to dilate the cervix uteri with laminaria tents in such cases. He preferred dilatation with metal bougies under anæsthesia, a glycerine vaginal tampon having been introduced two hours previously to soften and relax the otherwise rigid cervix. There was no need in these cases to dilate so as to admit the finger, for a piece of the growth could be easily removed by a small curette, and a diagnosis could be thus ensured. Dilatation by tents caused the patient many hours' discomfort, and that slow method should, in his opinion, be very rarely employed in these cases.

Dr. BLACKER thought laminaria tents when properly used were of much greater value than glycerine plugs; he had often been disappointed in the result when using the latter for the purpose of softening the cervix. Tents were dangerous if used improperly, and an instance of this kind had recently come under his observation at the Great Northern Hospital. The house surgeon had inserted a tent into the uterus of a patient some few years over the menopause and had not noticed any undue difficulty in introducing it. When it was removed the following day under the anæsthetic a good deal of bleeding occurred, more than usually followed the removal of a tent, and a sound passed into the uterus entered up to the handle and its point could be felt at the umbilicus. A diagnosis of perforation of the uterus was made, and as the case was possibly one of cancer of the body the uterus was at once removed by vaginal hysterectomy. A perforation was found at the fundus uteri. The patient made a good recovery. In this case the bad result was not due to the tent but the manner in which it had been introduced. Great care should be taken to use no force and only to introduce the tent to just beyond the internal os.

Dr. HERBERT SPENCER, in reply, said he agreed entirely with Dr. Lewers on the subject of laceration of the virgin uterus by large hard sounds. He had tried the glycerine tampons recommended by Dr. Routh, but had not found that they produced the requisite softening of the tissues. In reply to Dr. Heywood Smith, he thought that the peculiar changes in part of the tumour removed by the curette were probably due to infection. He had met with cancer of the body where the hymen had two minute openings which precluded the possibility of an instrument having been introduced into the uterus; all the same, it was possible that the numerous intra-uterine operations now performed might be a factor in the increased frequency of cancer of the body.

Demonstration of a very young Tubal Ovum.

By Frances Ivens, M.S.

I am bringing this specimen before the notice of the Section as I believe it to be very young, and therefore possibly of some slight value in helping to throw light on points of interest in the early development of the ovum, especially the amnion, upon which, owing to lack of material, it has been necessary to theorize from the standpoint of comparative embryology.

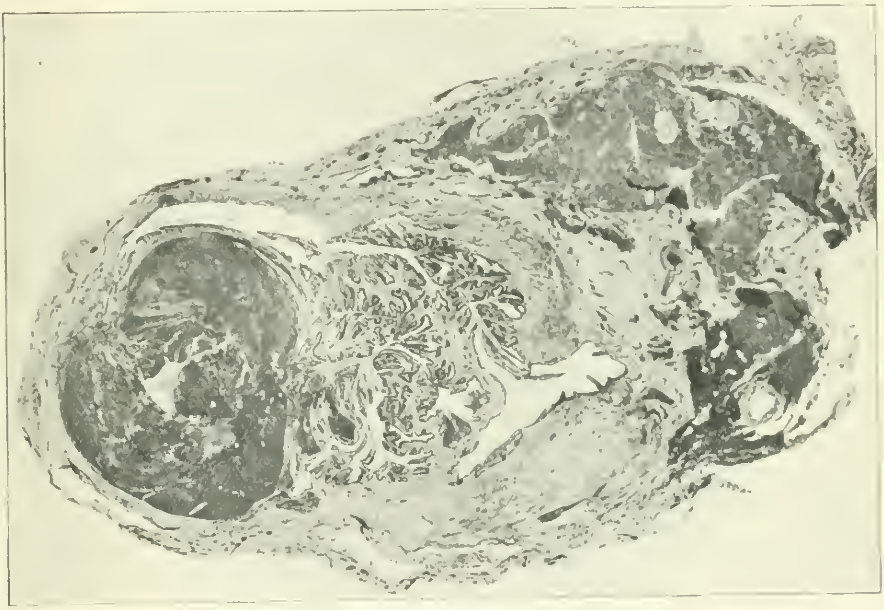

Fig. 1.

Complete section of tube with blastocyst, lying in blood-clot, and attached to muscular wall by plasmodial strand. (× 10.)

One cannot ignore the possibility that this ovum, which was found accidentally in the wall of the tube, may be pathological, and therefore an unreliable basis from which to draw correct conclusions, but as it presents no abnormal features the probability is that it is normal. The cells are clearly defined, as it was obtained fresh, it is complete, and the mesenchyme does not show the cellular appearance seen in some pathological ova.

History: The specimen was obtained from the right Fallopian tube of an 8-para, aged 30, admitted to the Liverpool Stanley Hospital on April 22, 1910, for retroversion of the uterus. The last pregnancy had terminated in an abortion two years previously. Menstruation had been regular but profuse, and the last period occurred exactly a month before the day of admission, when it began again as usual.

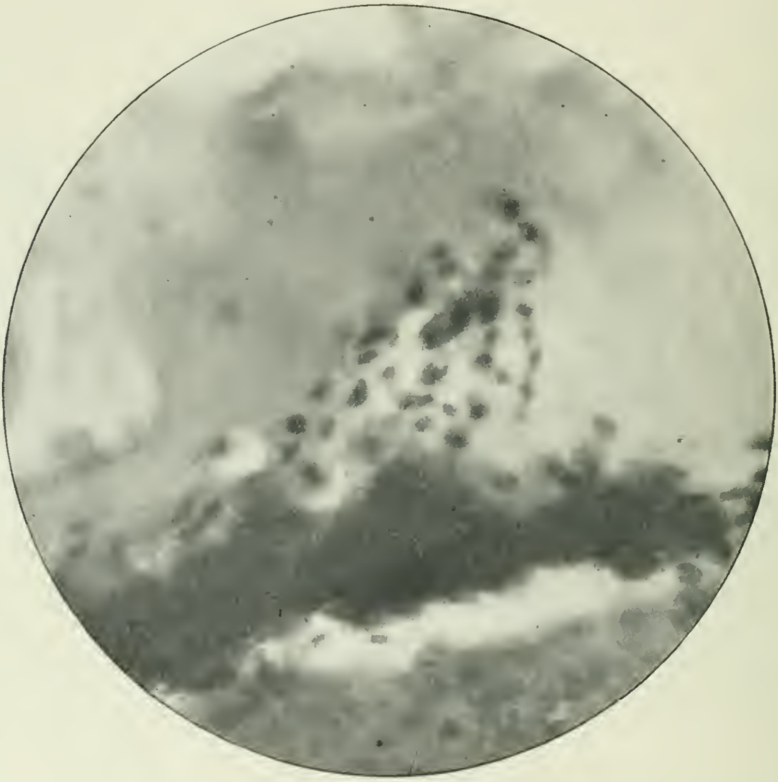

Fig. 2.

Section 4. Mesoblastic connecting stalk, attaching embryo to wall of blastocyst. (× 450.)

Operation: Laparotomy was performed on April 25, three days after admission. The retroverted uterus was attached by filmy adhesions to the appendages and rectum. The left tubal fimbriæ were partially glued together. The right tube was patulous, showing a small dark red thickening in the middle. As it looked like a tubal pregnancy the tube was removed. The uterus was fixed in position by Gilliam's operation.

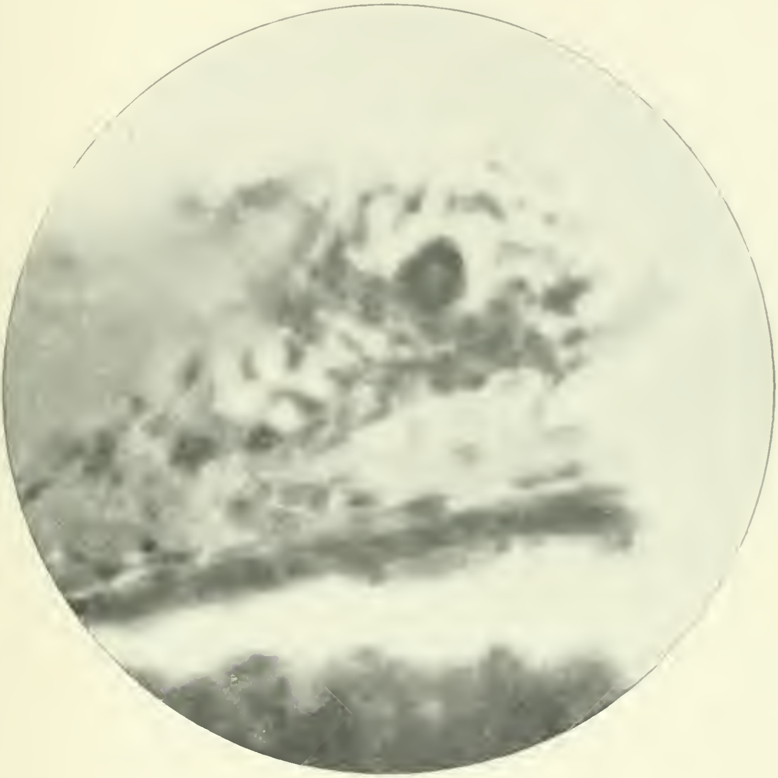

Fig. 3.

Section 8. Allantois tunnelling connecting stalk. (× 150.)

Specimen: The tube was at once placed in 4 per cent. formalin, and after fixing the thickened portion was cut out, mounted in celloidin, and cut into serial sections by Miss Wortham. The swelling measures 1·4 cm. in its longest diameter through the mesosalpinx. The plane of section is rather oblique, but the ovum can be seen in every section cut, and the embryo completely traced through about twenty sections $\frac{1}{100}$ mm. thick. The tubal lumen is rather flattened by a mass of clot, and the epithelial lining has disappeared for about one-eighth of the circumference. In the mesosalpinx there is extravasation of blood round the vessels. Anchored to the denuded muscular wall by a strand of plasmodium can be seen a triangular cyst—the ovum. There is no sign of decidual reaction in the tube, but some leucocytic infiltration at the point of attachment. From each angle of the cyst a strand of trophoblast passes out into the surrounding clot. There is very little branching, but a mesoblastic core can be seen in some of the strands.

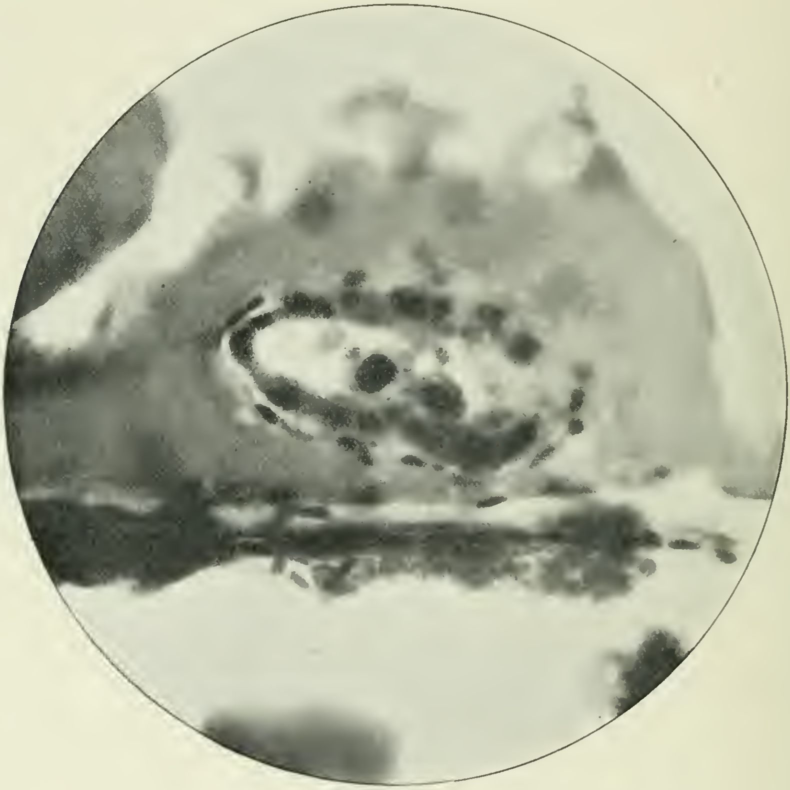

Fig. 4.

Section 9. Yolk-sac lined by cubical epithelium and covered by mesoblast. Two cells free in cavity. Wall of blastocyst (lined by cellular mesenchyme) shows cubical cyto-trophoblast. (× 450.)

The cyst wall shows the typical structure of cyto-trophoblast and plasmodic trophoblast. The cyst cavity or extra-cœlom is filled with a granular, very slightly cellular material, the mesenchyme or magma reticulare, but that lining the cyst is more cellular. The cyst measures 0·8 mm. by 0·6 mm. (internal measurements). The third dimension can only be roughly estimated by the thickness of the sections as about

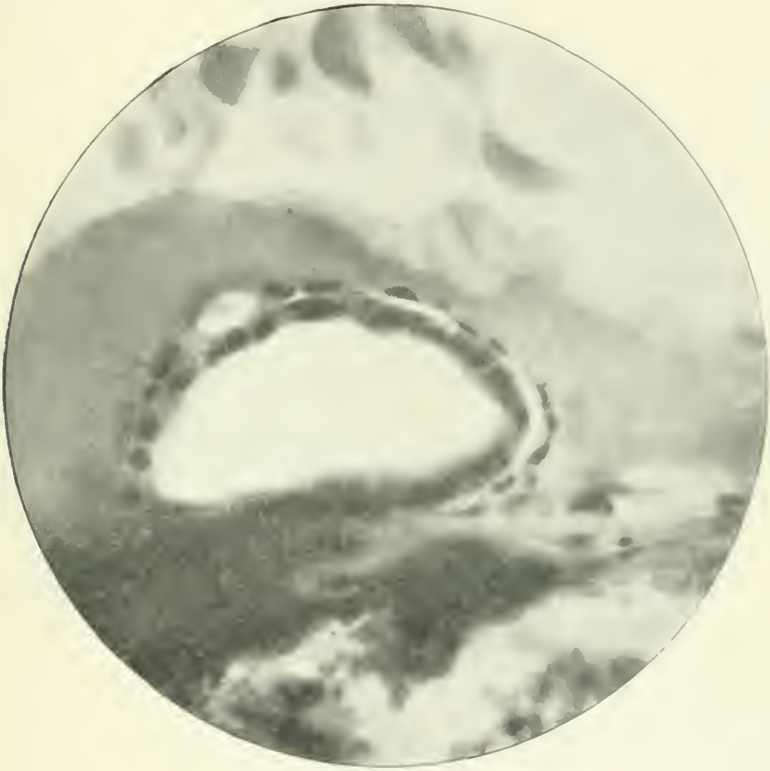

Fig. 5.

Section 12. Blood-island in wall of yolk-sac, lying between two layers. (× 450.)

0·8 mm. The embryonic rudiment, about 0·19 mm. in diameter, is attached to the cyst wall by cellular mesoblastic tissue. In nearly every section taken through it a prominent object is an oval or rounded sac, lined by well-formed cubical epithelium, and covered by a single layer of mesoblast. This is undoubtedly the yolk-sac. A mesoblastic stalk connects it with the cyst wall. It contains granular material and two large cells can be seen. In one section in the wall of the yolk-sac is a vacuolated cell, probably the beginning of a blood-island, from which a future vessel is to be formed. The mesoblastic stalk is partially tunnelled by a canal lined by columnar epithelium, which can be traced into the yolk-sac, and is evidently the beginning of an allantois. The amnio-embryonic cavity is represented by a cleft in the mesoblast, situated between the wall of the ovum and the yolk-sac. It is lined by one layer of epithelium, columnar towards the inner side, flattened towards the outer wall: this minute cavity can only be traced in about three sections. It shows no communication with the exterior through the chorion, the roof being

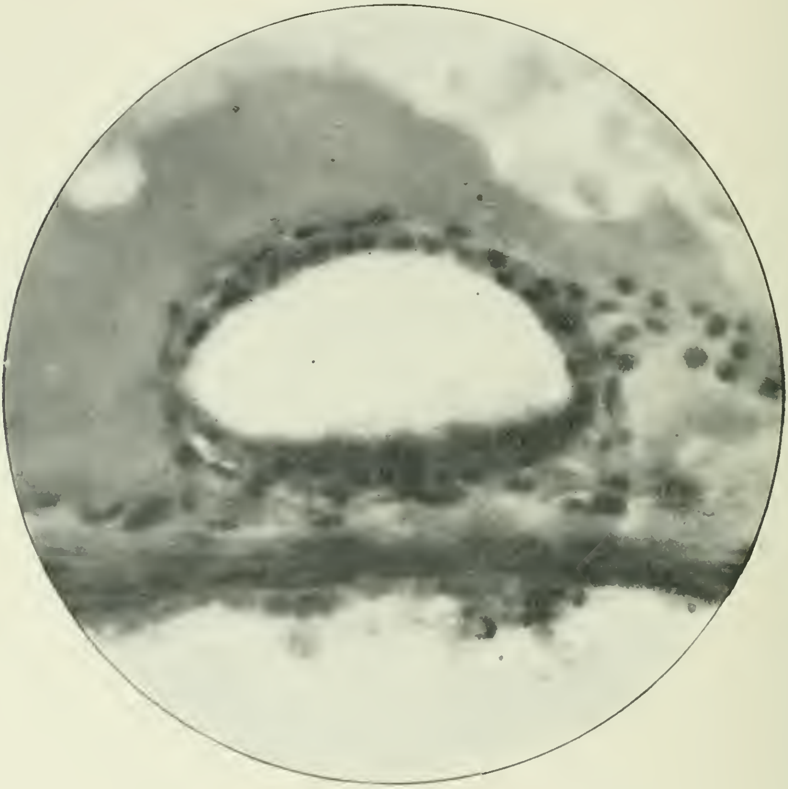

Fig. 6.

Section 13. Strands of mesoblast passing from yolk-sac out into mesenchyme. (× 450.)

attached to the lining wall by some mesoblastic tissue. The single layer of columnar cells in the floor of the amnio-embryonic cavity appears to be the ectodermal plate. Towards the caudal end (that near the allantois) is a lumen lined by columnar cells of which the nature is uncertain (? neurenteric canal). Strands of cells pass out into the mesenchyme from the mesoblast of the yolk-sac and connecting stalk, and vaguely indicate the formation of a cœlom.

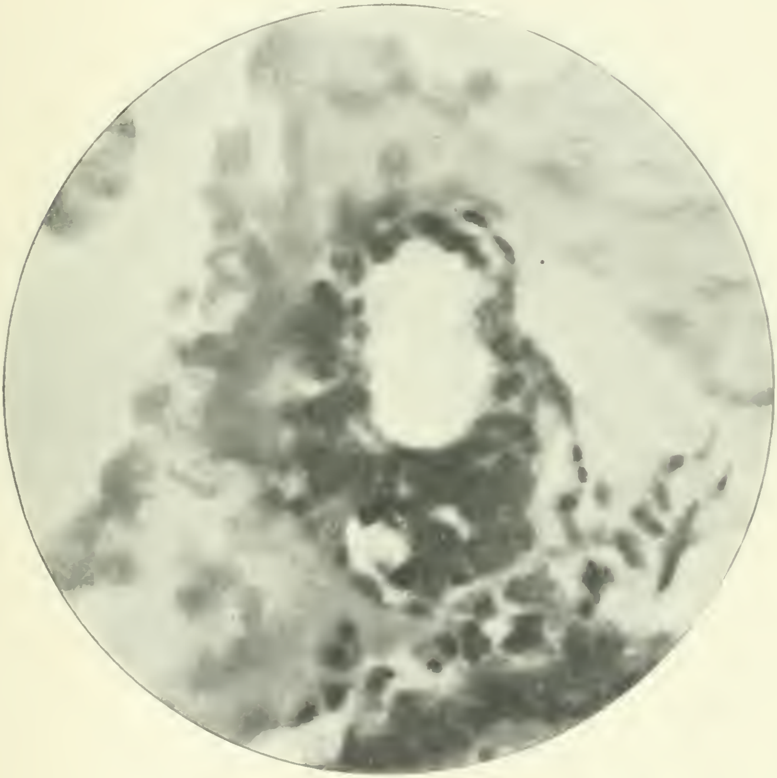

FIG. 7.

Section 15. Embryonic rudiment with yolk-sac and amnio-embryonic cavity. (× 150.)

Age: In determining the age of this ovum the history was of little value. Coitus had taken place at approximately weekly intervals, the last, five days before operation. Comparisons with the Teacher-Bryce and Peters's ova would indicate that this ovum comes somewhere between the two in age, and is probably about a fortnight old (on the basis of Teacher's determination). It is very slightly larger than the Teacher-Bryce [3] specimen (0·77 mm. by 0·63 mm.) but smaller than Peters's (1·6 mm. by 0·8 mm. by 0·9 mm.). Protrusions of mesenchyme seen in the primary villi also indicate that it is rather more advanced than the former specimen, while the development of the amnion and ectodermal plate appears less than Peters's, which also shows mesoblastic protrusions in the villi. The very definite mesoblastic attachment seen between the embryo and wall of the blastocyst in this specimen would negative the assumption that a free stage exists as a normal phase even at this early age.

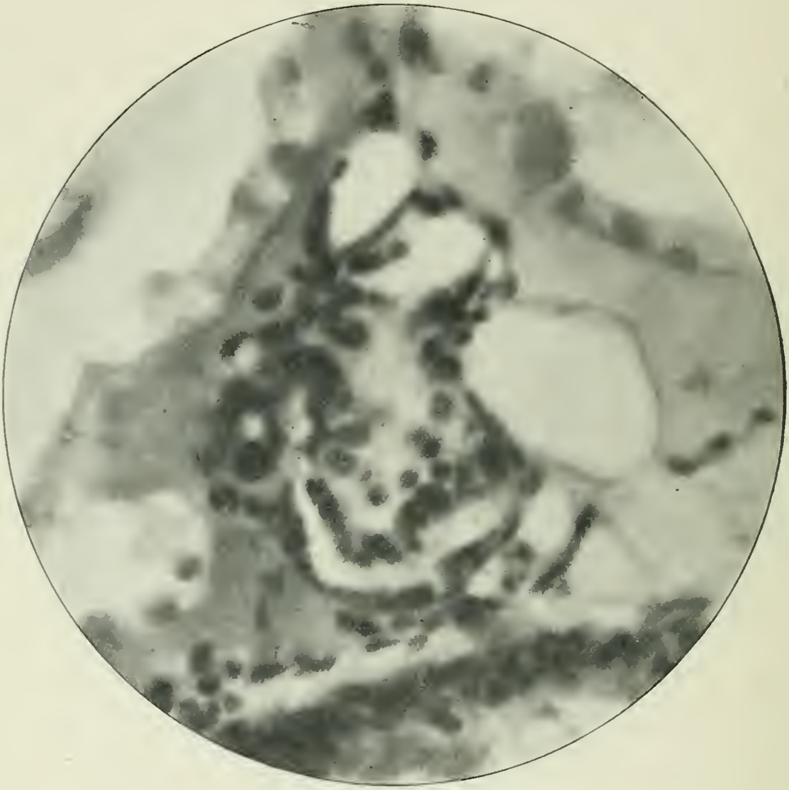

Fig. 8.

Section 17. Embryonic rudiment showing ectodermal plate, amnio-embryonic cavity, yolk-sac, ? neurenteric canal, and strands of mesoblast forming ? cœlom. (× 450.)

Development of amnion: The appearances in the sections support the view held by Keibel [1], that the amnio-embryonic cavity arises by a splitting of the formative cell-mass, and that amniotic folds never occur in man. This author considers the traces of an amniotic duct or connexion between the epithelium of the amniotic cavity and surface of the chorion (described by Beneke in human ova and Selenka in apes) to be only a "phylogenetic memory from dim ancestral times," and not left by a process of inversion. The method of formation of the amnion as seen in these sections strongly resembles that described by Sobotta [2] as occurring in the mouse, where a cleft arises in the formative cell mass. In this tubal ovum the chorion is already so well

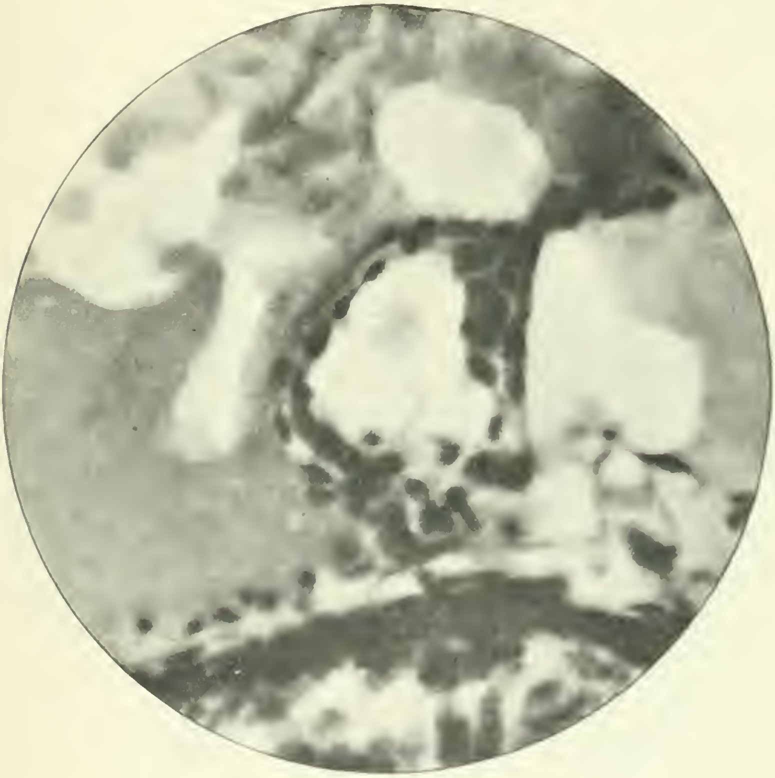

FIG. 9.

Section 18. Yolk-sac (smaller in section) with strand of attached mesoblast. Trace of amnio-embryonic cavity. (× 150.)

developed, while the amnion is comparatively minute, that it is inconceivable that the amnio-embryonic cavity has been formed by the rising up of folds, or that the ectodermal plate (as such) could ever have been free on the surface of the ovum.

REFERENCES.

[1] KEIBEL and MALL. "Human Embryology," Philad. and Lond., 1910, i, p. 55.
[2] SOBOTTA, "Die Entwicklung des Eies der Maus," *Arch. f. mikr. Anat.*, Bonn, 1903, lxi, p. 274.
[3] TEACHER. *Proceedings*, 1908, i, p. 264.

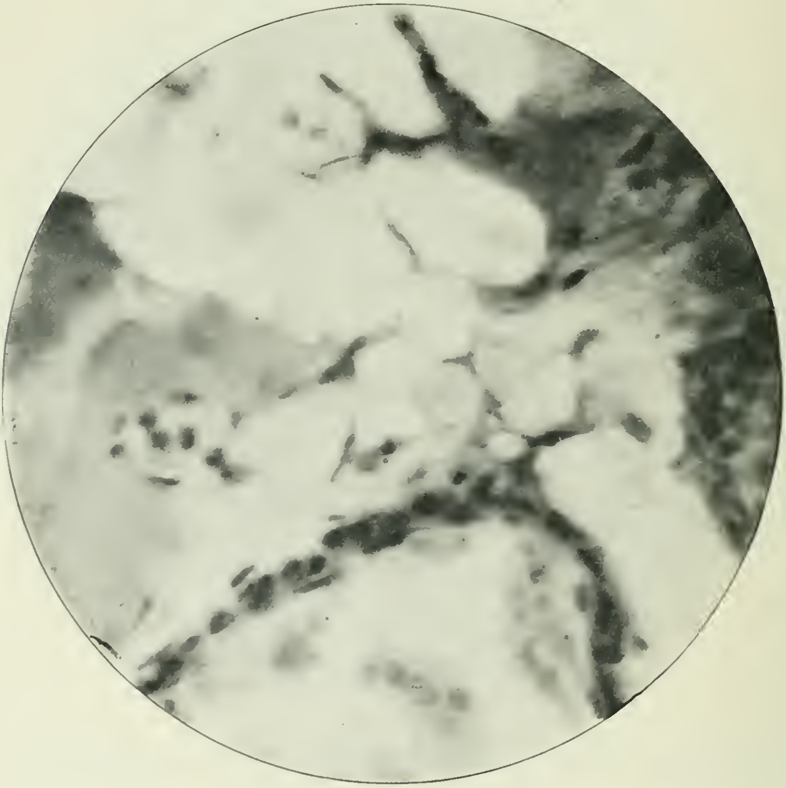

FIG. 10.

Section 22. A few cells showing termination of embryo. Branching cells of mesenchyme seen close to cellular wall of blastocyst. (× 450.)

THE PRESIDENT (Dr. Macnaughton-Jones) wished to convey the thanks of the Section to Miss Ivens for the very complete demonstration she had given them. Her devotion to research was on a par with her clinical and operative work, as shown in the paper she had just published on Hysterectomy for Myoma.

A Case of Gangrene of the Vagina probably due to the Chemical Effect of a Pink Rubber Ring Pessary.

By A. E. Johnson, F.R.C.S.

The following case illustrating the very rare condition of gangrene of the vagina deserves, I think, to be placed on record.

E. M. H., a well-nourished married woman, aged 37, was admitted to the Middlesex Hospital on November 20, 1909, under Dr. Victor Bonney, who transferred her to my care, and to whom I am indebted for permission to report her case. The patient was married in 1901, but had no children, though she twice had miscarriages. The first miscarriage was in April, 1903, and the second in September, 1904. There was nothing abnormal in the menstrual history.

After the second miscarriage the patient stayed in bed for a week, and her medical attendant afterwards told her that she was suffering from prolapse of the uterus. Some months later he inserted a pessary, which was changed and washed every month. A new instrument was obtained five times in four years. Some three weeks before her admission to Hospital she bought a new pessary at a large emporium in the West End of London and inserted it herself. Soon afterwards she began to suffer from pain in the left groin, and noticed an intermittent discharge from the vagina. A fortnight later the pain got so severe that on November 12, 1909, she had to stay in bed and send for her medical attendant. She was then extremely ill, and her temperature, on November 13, was 105° F. On November 17 the pessary was removed, and on November 19 a fleshy mass, about the size of a lawn tennis ball, was passed from the vagina. This proved to be the entire thickness of the mucous membrane covering the vaginal vault, the vaginal cervix, and the upper two-thirds of the vagina (*see* fig.). On the next day she was admitted to the Hospital in a very exhausted condition, and with a temperature of 104° F. She was examined under anæsthesia and the septic area was well cleaned and the vagina douched and packed with sterile gauze.

Treatment consisted of frequent vaginal douches and careful packing of the vagina with sterile gauze. This was carried out by the nurse, with the aid of a vaginal speculum. The patient, though her life was almost despaired of at first, recovered, and went to the Convalescent Home on December 23, 1909. Any tendency to prolapse was cured by the contraction of the scar tissue, and fifteen months after the illness the patient is in good health, and complains of no pain nor disturbance in the menstrual periods. The vagina is shortened, and greatly narrowed, but pervious up to the external os.

A perusal of the literature shows that the accidents which have been ascribed to the use of pessaries are numerous and serious. Many of them have even proved fatal. Neuegebauer,[1] in an exhaustive monograph on the dangers of vaginal pessaries, collected and analysed 247 cases of injury.

The case which has just been described is of a very unusual nature. On inquiry it was found that the patient had, on the last occasion, obtained a pessary of the same size as before, but of a new kind made of pink "rubber." This I was able to obtain, and found that it was very wrinkled and sodden, resembling an ordinary rubber ring pessary that had been in use for many months, instead of one which, in actual fact, had only been worn for little more than a fortnight. Its prematurely wrinkled condition suggested that some rapid destructive process had taken place in the vagina, and, in order to investigate this point Dr. Kellas made a careful chemical analysis of the used and unused pessaries of the same kind, and compared the results with those obtained by an analysis of the black rubber pessary which is in general use.

The pink pessary was found to consist of an outer pink layer of composition material and an inner black layer of purer rubber enclosing a watch spring.

The following is a summary of Dr. Kellas's results:—

	Weight without watch spring	Percentage of zinc oxide	Percentage of rubber	Percentage of residue not investigated
Red pessary (unused)	35·0 grm.	32·2	67·8	—
Red pessary (used)	34·6 ,,	32·05	67·95	—
Outer pink portion (unused)	—	42·2	57·8	—
Inner black portion (unused)	—	2·5	97·5	—
Black rubber pessary	—	—	98·86	1·14

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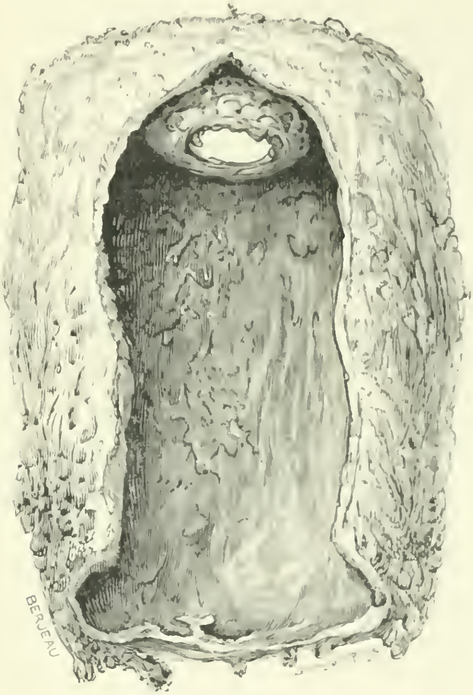
[1] *Arch. f. Gyn.*, Berl., 1893, xliii, p. 3.

It will be seen that there was a loss of weight in the pessary which had been used, and that the outer layer of the pink pessary contained a very high percentage of zinc oxide, while the black pessary in general use contains only a negligible residue, which consists of oxides of iron, zinc, aluminium, calcium, and magnesium, but which was not investigated further.



Case of gangrene of the vagina. ($\times \frac{1}{1}$.)

Dr. Kellas's report continues as follows:—

"A great many experiments were carried out to estimate the relative quantities of zinc oxide extracted on standing with various strengths of different acids. With 0·5 per cent. acetic acid at blood heat (37° C.), the pink pessary gave, in a few minutes, a solution containing sufficient zinc to readily give the tests for that metal. With acid of lower concentration—e.g., 0·1 and 0·2 per cent.—more time was required, but zinc could easily be detected on standing for a few hours at ordinary temperature. Experiments carried out with hydrochloric acid showed that the zinc was more rapidly extracted by that acid than by acetic of corresponding strength, as might be expected from the chemical relationships of the acids. After extracting repeatedly with hydrochloric acid at blood heat, it was found that further digestion seemed to extract no zinc, but after boiling or stretching the rubber, more zinc could be extracted. This seemed to indicate that the superficial zinc oxide alone could be extracted, the acid penetrating a very short distance into the rubber. The inner black portion round the watch spring would therefore be quite unaffected. On stretching, fresh surfaces were exposed, as the pink rubber had only moderate elasticity. On boiling, too, the rubber softened sufficiently to expose fresh surfaces.

"*(b) Behaviour of Black Pessaries.*—The quantity of material which could be extracted by boiling with acids from the black rubber pessaries was very small, and zinc could not, as a rule, be detected.

"*Conclusions from above Results.*—Keeping in view the escharotic effect of zinc salts the results seem to indicate that rubber containing a large quantity of zinc oxide should not be placed in contact with any part of the human body where acid secretions are likely to be formed. It might be pointed out also that zinc oxide is soluble in alkaline media, but the effect of alkaline solutions on the pessaries was not investigated."

Though I can scarcely claim to have advanced positive proof that the composition of the pessary was the cause of the disaster in this case, yet it appears the most likely explanation. This case may be an isolated one, but it is significant that, on trying to purchase more samples of these pessaries, it was found that they could no longer be obtained at the large emporium where the patient obtained hers, whilst at the makers', a well-known firm of instrument manufacturers, though some samples were sold to me, it was stated that they were no longer on the market.

DISCUSSION.

The PRESIDENT drew attention to the fact that the depth of colour in these substances was due to the vermilion they contained as much as to the zinc oxide, and that the sulphide of mercury present in the former might equally cause the vaginal irritation. Years ago he had exhibited in the Gynæcological Society a rubber pessary which had caused extensive vaginal erosion. It was embedded in the vaginal wall, and when taken out was covered with a thick calcareous coating. He showed some extremely light transparent celluloid rings made for Professor Schultze, of Jena, which he (the President) was in the habit of using. They were very strong, and could be readily moulded into any shape desired.

Dr. LEWERS said the slough shown in the first picture on the screen was exactly similar to the sloughs that used to come away in cases of advanced cancer of the cervix treated with tampons soaked in strong chloride of zinc solution (300 gr. to the ounce). Mr. Johnson's analysis of the material forming the pessary had shown it to contain oxide of zinc; it seemed possible that in this case a caustic zinc salt had been formed by some acid present in the vaginal secretion, and so caused the sloughing described.

Dr. HEYWOOD SMITH said that he thought if the sloughing had been due to contact of the vaginal wall with the pessary it would most probably have been limited to the upper portion of the vagina, and not have spread over the whole vaginal mucous membrane. He was rather, therefore, inclined to support the view that Dr. Eden had propounded, that the vaginitis was possibly due to some microbe introduced with the pessary. As to the question of innocuous pessaries, the first requisite was that they should fit; and in many cases, having procured a well-fitting pessary, he had a copy made in hollow aluminium, which could be worn without producing any sensation of its presence.

The Rectum in Gynæcology.

By E. STANMORE BISHOP, F.R.C.S.

THE lower end of the intestinal tract is in such intimate relation to the genitalia in both sexes that it is impossible to ignore its existence, or overlook the many ways in which it and they may act and react upon each other; frequently also the condition of the rectum may complicate operative work intended in the first instance for the relief of genital affections or the repair of genital injuries. Especially is this the case when those affections or injuries occur in women. Lying in close contact with each other, separated for some distance simply by their own walls, acted upon by the same muscles, actuated by the same nerves, it is inevitable that the rectum and vagina must participate in large degree in the same diseases, the same malformations, and the results of any injuries to the perineal region.

The functions exercised by the rectum are constantly being affected by the condition of the genital organs occupying the same cavity. A retroflexion of the uterus which has become adherent in Douglas's pouch will interpose a mechanical obstacle to the passage of fæcal material through the tube behind it; an adherent inflamed Fallopian tube or tubo-ovarian abscess in the same position will not only do this, but will render such passage extremely painful—so much so that the patient may chiefly complain of this painful defæcation. An abscess in the broad ligament will stiffen and render abnormally resistant one or more of Houston's valves, and no little of the suffering associated with the initial lesion may be due to the straining necessary to force rectal contents past the tender, narrowed areas so produced. And the mischief is reciprocal. One of the main difficulties against which the gynæcologist has to contend is habitual constipation in women, and in these days of abnormal activity on the part of the *Bacillus coli* it is hardly necessary to enlarge upon the dangers of leaving cultures of this organism for a long period in contact with the tissues, especially in the near vicinity of those which have already had their resistance decreased by the labours of other microbes. The condition known as muco-colitis is peculiarly characterized by its almost constant association with some morbid condition of the reproductive organs, and in many other ways, which will readily occur to all present, the condition of the rectum becomes of very great importance to the pure gynæcologist; so that I conceive that no apology is needed for the introduction of the subject of rectal conditions in the Section devoted to gynæcology.

For purposes of diagnosis the importance of the use of the rectum for bimanual examination may well be emphasized. When the condition of the os and cervix, and of the vaginal walls, has been determined by visual examination through a speculum, there is little more, except in certain cases where the necessity is obvious, to be learnt by touch that cannot with equal or greater ease be obtained by the internal finger within the rectum. I pass over its preferability in the unmarried, for even in those—the married women—in regard to whom no such question arises, the advantages of the rectal over the vaginal finger are great; the rectal finger can penetrate much further into the pelvis, since its range of observation is not blocked by the resistance of the fornices. The relative greater lateral mobility of the rectal tube allows the finger to travel with ease from side to side of the pelvis whilst passing behind everything therein contained except its own posterior wall. If the patient is in the lithotomy or left Sims's position, and the operator's left hand is used within, the palpating surface of the finger will face everything present in the pelvic cavity, and when opposed by the right hand upon the abdominal wall, will be able to the best advantage to fairly estimate the size, position, resistance, mobility, and all other important characteristics of any normal or abnormal content.

And the rectal finger is not only more efficient for the examiner, but it is also relatively safer for the patient. As the finger in this canal must pass behind a pyosalpinx or a tubal pregnancy, it recognizes with much greater ease the tense elasticity of the one or the soft throbbing bulge of the other. Warned by this, the opposing abdominal hand is prevented from exerting injurious pressure; and thus there is less risk of accidentally squeezing pus through the fimbriated extremity into the peritoneum, or of rupturing an ectopic sac.

Many things can also be recognized from the rectum, which the thicker and more resistant walls of the vagina render indistinct. A patient was sent to me in 1898 by Dr. Knapton, of Ruabon, a lady who had suffered for many years from pelvic pain, which was increased before and during the first few days of menstruation. For this reason both her ovaries had been removed during the preceding year by another surgeon, but the pain continued. *Per vaginam*, nothing but a small uterus could be felt, but with a finger in the rectum a calculus (shown) could readily be detected in the left ureter, in the lower end of which it was impacted. It was removed through the bladder, and all pain disappeared. It was a curious fact that even after its detection *per rectum* the stone was still unrecognizable through the vaginal wall. Lately I have again seen a similar case, which was of course verified before operation by means of the X-rays. In the earlier case such help was not available.

In another case, though of a different kind, the introduction of the rectal finger cleared up at once what at first appeared to be a considerable puzzle. A very able practitioner in Manchester some little while since requested me to see a patient who complained chiefly of acute pain in the anal passage, exacerbated by any attempt to defæcate. The surgeon had naturally explored for fissure, but neither he nor I could find any such condition. The exterior of the anus, and as far as could be seen by separating the buttocks and requesting the patient to bear down, was normal. No sentinel pile could be seen. The whole exterior of the perineal region was natural in colour and conformation. Vaginal and vagino-bimanual examination gave equally negative results. Nothing abnormal could be felt or seen. Yet the patient was in exquisite pain, and naturally became impatient of what appeared to be our entirely futile exploration. Directly, however, the finger passed into the rectum it recognized an abscess in the connective tissues lying laterally and behind, the upper limit of which was above the pelvic fascia and bulged strongly into the bowel above the sphincters. It had not as yet penetrated sufficiently near to the skin in the ischio-rectal fossa to be recognizable from without, and it lay entirely behind the vaginal wall, into which however—now that we knew where to look for it—we could recognize that it bulged slightly. At the operation a comparatively large tuberculous abscess was opened which had burrowed widely. I enquired of my friend why he had not previously inserted his finger into this canal. He said that he did not like to do so, partly because it was a dirty business, partly for fear of annoying his patient, but mainly because he had never been taught to do so by the teachers at his medical school. I have been surprised to find that many other practitioners give similar answers, and I venture to suggest that this method of obtaining information, which is sometimes very valuable, might well engage the attention to a larger extent than is at present the case of those who are responsible for the education of medical students.

The detection of effusions in Douglas's pouch and the recognition of their characteristics is much more easy through the thinner and more elastic rectal wall than through the vagina, though such diagnosis is rendered more certain if the rectal finger is opposed by one in the anterior tube. Tubercular nodules are easily recognized; the doughy, sluggish sensation produced by blood, the thinner, easily displaceable pus or serous fluid, the outline of a prolapsed tube or ovary, can be more easily followed than is possible through the thicker and more resistant vaginal fornix, and certain other points are also more readily detected. During the last month I was asked to see a patient with Dr. Lawrie, of Ramsbottom. Some months before a fibroid was said to have been removed by another surgeon. Dr. Lawrie was confident that when he first saw the patient a few days earlier a large mass occupied the right iliac fossa, and that there was relatively but a small amount of free fluid in the peritoneal cavity. On the morning of our visit, however, the abdomen was much distended and rendered quite tense by fluid which was evidently free, and through which no distinct mass could be felt. She had been tapped already some time before and dark-coloured fluid had been evacuated; it was not, however, clear, whether the fluid had been obtained from a cyst or from the cœlom, but a provisional diagnosis of some malignant or semi-malignant growth was made, and this assumption was greatly strengthened by the fact that digital pressure through the rectum upon Douglas's pouch produced well-marked pain. The abdomen was opened and a sarcomatous ovary found and removed.

It would be an impertinence if I were to say anything as to the ordinary modes of rectal examination before this Section, but in connexion with the question of mechanical assistance, perhaps I may be allowed to bring before your notice a modification of Allingham's well-known dilator, which I have had made for me by Messrs. Mayer and Meltzer, and which I find very useful. The short handles in the original I have had flattened and lengthened, so enabling the user to effect dilatation with more ease and at the same time to gauge the amount of resistance of the sphincter it is safe to overcome entirely by his own muscles and so to apply the distending force with greater security to the patient. Any degree of dilatation can be maintained by means of the ratchet and screw attached in the new model, whilst the operator's hand is at rest; care should, however, be taken, never to use this attachment for the purpose of applying the force it is capable of exerting, as by it the muscular fibres might easily be torn. With the proper use of this instrument there is no danger of this, and an admirable view can be obtained of the lower rectum, and various operations upon its walls can be carried out in full sight.

Another aid to examination has comparatively lately been added by the introduction of the proctoscope. With the aid of the inflating rubber bellows attached, the bowel may safely be explored in the knee-chest position as far as the sacral projection, which is sufficient for most purposes. Mr. Mummery, I understand, succeeds in getting much further, and has figured the appearance of the left common iliac as it shows its pulsation through the posterior wall of the sigmoid. One admires his skill, but for practical purposes and in the hands of most men, I believe it is unsafe to attempt to pass the instrument so far. Even with this restricted use, much information may be obtained. The varying conditions of Houston's valves, inflammatory or ulcerative states, as well as growths of all kinds, can be clearly seen, and their extent, size, tendency to bleed, or otherwise react to the touch of a probe, can be more correctly estimated than in any other way. The elasticity, or *au contraire*, the stiffness and inability to dilate, of the rectal walls, can be gauged by inflation. The slightest resistance to the introduction of the tube when once past the sphincter should cause the removal of the obturator and should be overcome, if it can be overcome at all, in full sight, by altering the axis of the cylinder whilst inflation is going on. It should never be attempted whilst the obturator is in place, since the rectal wall, if diseased, may very easily tear without much warning. It is always wise to pass a finger into the rectum before using this tube, as the condition and nature of any obstruction is thus previously recognized.

Perhaps the most common rectal complication in gynaecological cases is that of hæmorrhoids, with or without prolapse. Hæmorrhoids are no doubt not very important in themselves, though often very troublesome to their unfortunate possessor, but they, as well as prolapse, may be very significant of pelvic disease higher up; it is therefore of the greatest importance that the presence or absence of any such disease should be determined before any operation for their relief is undertaken; and here again the value of the rectal finger in bimanual examination is emphasized. An impacted subperitoneal or other fibroid in Douglas's pouch, carcinoma of the cervix infiltrating the broad or sacro-uterine ligaments, still more carcinoma of the rectal wall itself, or inflammatory conditions implicating the broad ligaments, may all, by impeding the return of blood, produce sensations which suggest to the patient, and may be described by her as, "an attack of piles." All these possibilities should be considered, and, if present, their full force estimated, before any operation is undertaken for the relief of this outward manifestation of their presence.

Though perhaps somewhat outside the scope of this paper, may I be permitted to say a few words as to one small, though, as I think, important anatomical fact which appears to have been overlooked in the discussions which from time to time arise about hæmorrhoids?

Hæmorrhoids are, as everyone knows, dilated venules of one common plexus, the internal hæmorrhoidal, which empties itself partly into the portal system through the superior hæmorrhoidal, and partly into the systemic system through the middle and inferior veins of the same name. Why do they dilate? Of course we all know the stock answers: obstructions above, systemic or portal, the fact that they are terminal veins without valves to support the column of blood above, the constant maintenance of the upright or sedentary position, &c. But without minimizing any of these, is there not another reason? We all know the tough submucosal coat of the stomach and intestine, that resistant structure upon which we depend for the security of our sutures in such operations as gastro-enterostomy or enterorrhaphy; we know that sutures which do not include this coat will probably tear out before solid union has taken place, but that if they grip this coat they may be relied upon to remain firm and keep the tissues in apposition until solidarity is obtained. In this portion of the gut, however, where is this resistant coat? At or near the point where the middle hæmorrhoidal veins pierce the muscular coat this tough submucosal layer meets the loose subcutaneous layer which has been carried inwards by the proctodæum to blend with the lower gut during the development of the fœtus; they meet and there unite; the perineal portion of the hæmorrhoidal plexus, that in which piles are found, lies below this line, and consequently in subcutaneous connective tissue—not submucosa. These veins naturally thus find much less resistance to their dilatation than they would were they seated in or supported by the tougher submucosa. And this fact is of importance in their surgical treatment. The extent to which the stratified epithelium, which marks the presence of skin, extends up the anal canal is shown in this photograph. It will be seen to run up to the level of Morgagni's valves. And the same factor—the presence of imperfectly developed skin and subcutaneous tissue as a lining to the anal canal below Morgagni's valves—explains the intense pain experienced when one of these valves is torn down and the well-known ulcer or fissure results. The tissue laid bare is not submucosa which is supplied by sympathetic nerves, but connective tissue which contains systemic sensory nerves. Such fissures often cause symptoms which are, by the patient, referred to the genitalia, and many obscure cases of pelvic pain which cannot be cleared up by any examination of the genital organs have been demonstrated to have their true cause in such ulcers, which, if they not have extended to the anal margin, may not be suspected and cannot be seen without a speculum. And, in their surgical treatment, is not the electric knife, which not only divides but kills any exposed nerve-end and which occludes all open lymphatic spaces, indicated rather than the simple knife, which does neither?

Recto-vaginal fistulæ are not frequent nowadays, at least in the North. When they do occur, they usually follow imperfect attempts at perineorrhaphy, especially if the rectal sphincter has been torn through and the surgeon, operating at the time of parturition, has failed to recognize this or to properly approximate and retain in apposition the divided ends of the muscle. In such cases the formation of a fistula is generally the least of the damage done to and around the vaginal canal; the fibres of the levator ani and the thin but firm layer of pelvic fascia which normally intervene between the two canals, together with the transversi perinei, have all been split and have retracted to the sides of the pelvis. Fortunately, the measures adopted to repair the one lesion suffice to close the other. Almost every gynæcological authority has his own method of reconstruction. I think that, during my life, I have tried all the classic methods, and have had more or less—frequently less—success with all. But some years ago Dr. Donald, of Manchester, kindly demonstrated to me a method, somewhat like that of Hegar, but more complete; since this, as the advertisements say, "I have used no other." As it may not be as well known as its merits deserve, perhaps you will permit me shortly to describe it.

It consists in the removal of a triangle superposed upon a second but truncated and much wider triangle of mucosa from the posterior vaginal wall; and the re-union of the tissues remaining in a special way. One important element in the technique is the introduction of sutures, *pari passu*, with the denudation, each suture being used to lift and draw forward the tissues next to be divided; that is, so far as the superior triangle is concerned. I believe that Dr. Donald also does this with the lower, but, personally, I have found it easier to complete the removal of the lower area of mucosa before introducing the lower sutures.

The apex of the upper triangle is placed in the middle line 3 cm. to 4 cm. below the external os, which is drawn forward by a vulsellum; from this point a line is drawn with a knife downwards and outwards on either side to points in the sulci on either side about 5 cm. below the apex; the mucous membrane is detached from this area from above downwards for about 2 cm. and a suture applied through the cut edges; this suture is left long, and is utilized to draw the tissues forward whilst the next segment is freed; another suture is applied in the same way, the first cut short, whilst the second is used as before; the size of the triangle and the acuteness of its angles depend upon the amount of dilatation of the vagina. When the whole of this triangle has been removed and the gap left by it closed by sutures which only grip the opposed edges of the mucosa, the last suture being held so as to draw the parts sutured upwards and forwards, forceps are applied to the edges of the vulva at opposite points on the level to which it is proposed to close the vaginal opening. From these points to the central suture a line is drawn with the knife through the mucosa on either side, and all mucous membrane below these lines is removed up to the skin edges in one piece. The tissues so laid bare are depressed in the median line by the left forefinger, which thus keeps the rectum out of the way and produces a V-shaped cavity, the sides of which are now united by silkworm gut sutures which, entering the mucosa on the left side, are passed deeply into the tissues on that side, picking up the pelvic fascia, emerge just above the rectum, cross over this and re-enter the tissues on the opposite side from below upwards, picking up the pelvic fascia on that side and emerge on the mucosa. When this suture is tightened it draws the walls of the V-shaped gap together, re-uniting the torn edges of the pelvic fascia. A succession of these sutures is placed in a similar manner until the skin edges are reached. These silkworm sutures are left long—any superfluous amount being cut away—so as to facilitate their removal three weeks later; the skin edges are united by catgut. Of course, if the sphincter ani has been divided originally, its divided ends are sought for and united just before the last two or three silkworm sutures are placed. In this way not only is a firm, thick perineum obtained, but any recto-vaginal fistula is also closed in the most effective way.

The mortality of this operation is *nil*, and, so far as my experience goes—an experience which dates back six or seven years—the results are perfectly good and lasting. In one case only was dyspareunia complained of, and this was evidently due to the fact that the outer opening had been closed too much. This can not only be easily avoided, but can be remedied with but little difficulty. In all such operations during the last two years, Webster's method of shortening the round ligaments has also been done in order to correct the retroversion which sometimes remains. By this combination almost the same *status quo ante partum* would appear to be reproduced, an ideal which all aim at in plastic work. Above all, the uterus, whilst supported at the proper height and in the proper position, is not in any way "fixed." It has not lost its mobility nor its power of equable and safe development; the two fatal objections, to my mind, against all forms of ventrofixation. I am happy to be able to say that the additional round ligament shortening has also had no mortality, nor has it been in any case followed by ventral hernia; whilst in the, as yet, few cases in which a subsequent pregnancy has occurred, the labour has been normal, and no breaking down has occurred of the perineum so built up.

I find that I have left myself no time in which to refer to the rectum as an avenue by means of which treatment of gynæcological disorders or post-operative shock may be carried out, but inasmuch as there is but little that must not be perfectly well known to the members of this Section, that is the less to be regretted. Before sitting down, I should wish to thank the Section for the patience with which it has listened to what I feel has been a very inadequate presentation of this subject, the relation of the rectum to gynæcology.

DISCUSSION.

The PRESIDENT regretted that at so late an hour it was impossible to discuss as it deserved this important practical communication of Mr. Stanmore Bishop. Howard Kelly in his recent work on medical gynaecology had drawn attention to the importance of a study of the rectum and its examination in gynæcology. It was unnecessary to emphasize the fact that in many cases examination of the rectum revealed either an affection of the bowel itself, or elucidated some pelvic complication outside it. Many a blunder would be avoided if the bimanual recto-vaginal or recto-abdominal examination were carefully carried out. No gynæcological examination could be said to be complete for purposes of diagnosis in which the rectum was not included. This was so obvious that it was superfluous to dwell upon it. In regard to recto-vaginal fistula, he (the President) had successfully closed the largest fistulæ, after vivifying the edges and freeing a fairly large margin of the vaginal wall, by closing the bowel opening by in-and-out sutures of twenty-day gut, carried through the rectal tunic so as to completely invert the denuded rectal edges. A purse-string suture was then placed, not too tightly, round the closed aperture, and finally the vaginal flaps were united over this by silkworm gut or "celloidinzwirn."

Mr. MUMMERY, in answer to a question of the President with regard to the treatment of prolapse of the rectum, said that he had found very excellent results from the new operation which he described in the *Lancet*[1] about two years ago. This consisted in making an incision behind the anus, opening up the posterior rectal space and packing it lightly with gauze, allowing the wound to heal by granulation, the amount of separation and the area of the wound depending upon the size of the prolapse. Mr. Mummery stated that he had now operated upon nine cases by this method, and so far had not had a recurrence; and though of course it was a little early yet to say that no recurrence had occurred, one very severe case had been done now nearly six years, and in another case, although the prolapse had existed for certainly over fifteen years, no recurrence had occurred, though a considerable interval had elapsed since the operation. He believed this operation to be the best for prolapse of the first and second degrees, both on account of its simplicity, the absence of any danger, and the fact that it cured the condition by reproducing as far as possible the normal condition of the parts. With reference to Mr. Stanmore Bishop's paper, Mr. Mummery said that he did not approve of the dilator shown, and that personally he was very much opposed to the use of any form of mechanical dilator, preferring to use fingers alone, and that in his own practice he found it very rarely necessary really to stretch the sphincters at all. With regard to the use of the sigmoidoscope, it was certainly difficult at first to pass the instrument into the sigmoid, but it was merely a question of practice and of learning the trick, when it became comparatively easy. When an instrument such as his own was used, there was no fear of damaging the bowel if ordinary care was taken and the instrument passed only by sight. The value of being able to see up to the middle of the sigmoid flexure could hardly be over-estimated, as, during the last year, he had seen no less than six cases in which a lesion, otherwise undetectable, in this situation was diagnosed by means of the sigmoidoscope, and subsequently removed *per abdomen*. Mr. Mummery asked the opinion of the Society with regard to a form of stricture of the rectum, which he had not infrequently seen as the result of parturition. It occurred at a spot about 3 in. up the bowel, and he believed it to be due to pressure of the head during prolonged labour against the lower edge of the sacral prominence.

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[1] *Lancet*, 1910, i, p. 641.

Obstetrical and Gynæcological Section.

July 6, 1911.

Dr. H. Macnaughton-Jones, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

Gentlemen,—Before addressing you on a special subject which I desire to bring before you to-night, I will say a few words on the work which has been done in the Section during the past session; and first, a few pathological communications deserve notice.

The debated question of the possibility of primary abdominal gestation was discussed by Dr. Blair Bell through an exhaustive examination of the genitalia in the instance of abdominal pregnancy in the rabbit. His deduction was that such primary implantation may occur in the human female.

Dr. Arthur Giles's specimen of "Hydatid of the Uterus" was the first instance of this rare condition recorded in this country.

The occurrence of disseminated glands in the vaginal wall and their embryological derivation were elaborately investigated and discussed under the term "Adenomatosis Vaginæ," by Mr. Victor Bonney and Mr. Glendining.

Dr. Blair Bell's originality of conception and exhaustive research before formulating his conclusions were exhibited in his paper on the bearing of hæmatocolpos on hæmatometra, the coagulability of menstrual blood, the calcium content, the bacterial origin of lactic acid, and the presence of columnar epithelium in the vaginal lining.

A most valuable pathological contribution to our knowledge of the degrees of infiltration of the lymphatic glands of the pelvis in cancer of the uterus was made by Dr. Archibald Leitch. The general trend of the author's conclusions was in favour of extirpation of the definite danger-zones, including the glands and broad ligaments.

Dr. Drummond Robinson's observations on the vaginal secretion in infants, in which the acidity and sterility of the secretion during the first forty-eight hours were proved, led to the discussion as to the existence or non-existence of glands in the fœtus, and the specimens subsequently shown by Dr. Herbert Spencer, Dr. Drummond Robinson, and Mr. Glendining, were referred by the Pathological Committee to Professor Keith and Dr. Arthur Thomson for their report. They, however, declined to decide the question.

For Professor Schottländer I showed a uterine polypus undergoing sarcomatous degeneration, and drawings of a sagittal section of an entire uterus in which a healthy fundal cavity separated the carcinomatous crater of the cervix from a solid carcinomatous polypus growing from the summit of the fundus.

In embryology we have had three original and remarkable demonstrations. Miss McIlroy, from early embryonic ovaries, showed the transitional phases through which these oögonia passed in their evolution into oöcytes, the cells of the stratum granulosum, the interstitial and follicle cells; and Miss Frances Ivens a beautiful series of sections showing the evolution of the ovum in a very early tubal pregnancy. The latter was equalled, but hardly surpassed, by Dr. Young's demonstration on the uterine mucosa in menstruation: the action of the chorionic cells and the function of the decidua.

Other interesting pathological conditions were shown in Colonel Dimmock's "Columnar-cell Carcinoma of the Ovary," Dr. Clifford White's "Carcinoma of the Vulva," Mrs. Scharlieb's "Fibroma with Twisted Pedicle," Dr. Lewers's "Sarcoma of the Vulva," Miss McIlroy's "Hæmatosalpinx due to Torsion of the Tube," Mr. Drew's "Sarcoma of the Uterus," Mr. Percival Coles's "Case of Supposed Pseudohermaphroditism," Dr. Drummond Maxwell's "Thyroid Tissue in Ovarian Embryoma."

CLINICAL.

An important clinical communication was that of Dr. Donald and Dr. Shaw, in which the advantage of curetting in endometritis was emphasized. There were some rather debatable views advanced, but there was one point on which I am glad that the authors insisted—namely, the discredit the operation of curettage is apt to gain from an inefficient and incomplete so-called "scraping of the womb."

Three interesting instances of morbid conditions met with at exceptionally early ages were recorded in Mr. Paterson's "Case of Suppurating Ovarian Cyst in a Girl aged 10," Dr. Blacker's "Fibromyomatous Polypus in a Girl aged 17," and Mr. Herbert Spencer's "Carcinoma Uteri in a Patient aged 26." A rather unusual instance of the "Complete Disappearance of an Ovarian Cyst the Size of a Fœtal Head, associated with Vesicular Degeneration of the Chorion," was recorded by Dr. Drummond Maxwell. Dr. Barris brought the occurrence of "Acute Intestinal Obstruction from a Fibromyoma" before us, and Mr. Cameron three most interesting clinical conditions: the first, "Rupture of a Gravid Uterus through an Old Cæsarean Section Scar," the second "An Instance of Twin Tubal Pregnancy," and the third "A Well-marked Example of an Osteo-malacic Pelvis." I showed a "Multiple Myomatous Uterus in which Exhaustive Hæmorrhages had occurred, which led to Double Phlebitis after Hysterectomy."

OPERATIVE.

The introduction of a previously determined discussion into our proceedings was a new departure. The debate which followed Dr. Amand Routh's highly practical analysis of all the arguments for and against the performance of "Cæsarean Section in Cases of Contracted Pelvis in Women exposed to Sepsis," fully justified the innovation. The views of the Fellows, and the general conclusions to be drawn from the discussion, do not require any comment here. One remark only I should like to make—namely, that from all the clinical evidence adduced there appears to be still a place for other obstetrical measures in certain cases of contracted pelvis even when sepsis is suspected.

A most important operative discussion followed Mr. Childe's report of twenty-three cases in which Wertheim's operation was performed for cancer of the uterus. Immediate results were excellent, recovery from the operation following in twenty-one cases. As to permanency of cure, no definite conclusions can be drawn from this group. Only one of the number had been operated upon over five years before the paper was written, three over four years, one barely four years, one close on three years, two over two years, five under two years, three under eighteen months, and seven under one year. Mr. Childe warmly advocated Wertheim's method, but discountenanced the dissection of the glands.

Mr. Alban Doran's paper on "Subtotal Hysterectomy for Myoma Uteri," in which he completed an exhaustive analysis of a hundred cases operated upon by him, raised the question of the desirability of leaving healthy ovarian tissue whenever possible, as well as a portion of the endometrium. While the adoption of the former precaution one may say is universally accepted, the same cannot be said with regard to the latter suggestion, the futility of the attempt to carry it out in a large proportion of cases being obvious.

Mr. Hey Groves's demonstration of his suggested operation for the treatment of flat pelvis led to rather an unfavourable criticism of this method as compared with other operative procedures. There can be no doubt, however, that the steps and the technique of the operation were most ingenious.

Mr. Victor Bonney strove to make a plea for excision of a portion of the anterior uterine wall as suggested by Kelly's utriculus operation in place of hysterectomy in certain cases of hæmorrhage. He gave the name of "utriculoplasty" to this alternative procedure. We must know much more of the differentiating indications for this substitution before we can define the conditions under which we may practise it.

Interesting operative cases were those of Dr. Lewers, of removal by hysterectomy of an interstitial uterine gestation sac, in itself a rare occurrence, and Dr. Herbert Spencer's most successful ovariotomy where a suppurating ovarian tumour had perforated the bowel, which, with the omentum and appendix, was removed at the same time.

The Relation of the Appendix to the Causation and Surgical Treatment of Affections of the Adnexa.

By H. Macnaughton-Jones, M.D.

I suppose all will admit the importance of the relations that morbid states of the appendix bear to the pelvic organs of women, and that an acquaintance with them is essential alike to the gynæcologist, the general surgeon, and the practitioner. It is because I do not find in the past *Proceedings* of this Section, or of the Amalgamating Societies, that these relationships have been specially dealt with that I venture to ask your attention to a brief consideration of them.

At the outset I must disclaim any pretension to an attempt to throw fresh light on a matter so thoroughly sifted by many whose experience in this direction has enabled them to collate much valuable material and to draw conclusions based on the observation of a very large number of cases. Rather my desire is to briefly review from the pathological, clinical and operative standpoint the influence our present knowledge exerts on the diagnosis and operative treatment of appendical complications in affections of the female genitalia. Nor is there time, even were I so inclined, to weary you with any lengthened details of special cases. There are many present who in a large operative experience have doubtless met with a great variety of conditions in which the appendix was included in the operative area, as also various abnormalities in its size and position. I may mention a few of several examples which have occurred to myself.

I operated upon a child, aged 10, in which there was a doubt as to diagnosis. She suffered from colicky pains, but the bowel had acted with aperients. Under anæsthesia, a hard mass was felt above the left fornix; there was a malformation of the uterus, it being abnormally small. At the operation a long appendix was found dipping down to the pelvic floor, to which it was adherent, and here there was a circumscribed abscess. The uterus and ovaries were not developed. In the upper bend of the sigmoid there was a mass which proved to be a hard concretion. The appendix was removed, and with drainage the girl made a good recovery.

Quite recently I operated upon a child, aged 11, for undoubted symptoms of chronic appendicitis. The thickened and inflamed appendix, with fæcal contents, and a dilated end, was found firmly adherent to the right ovary.

Some years since, in operating for a right pyosalpinx, I found a very long appendix which stretched across the abdomen and was adherent to a diverticulum of the bowel.

Operating for adnexal trouble in another case, in which there were appendical symptoms, I sought for the appendix and could not find it. It was represented by a thin fibrous cord some 3 in. in length which had formed an adhesion with the adjacent bowel. I had only once seen a case in which the appendix was absent; this was in Professor Krönig's clinic in Freiburg. Dr. Cuthbert Lockyer reports on the structure I refer to: "There is no trace of gland tissue, nor of any epithelial nests in this interesting specimen. It differs from the usual peritoneal inflammatory bands that we occasionally have to examine in the fact that its structure is systematized into outer longitudinal and inner annular bundles of fibromuscular tissue; then, where the lumen should be, is seen a loose areolar fibrous network carrying blood-vessels."

I wrote at the time to several operators with special experience in appendical affections, and though all had seen rudimentary appendices, or the appendix reduced by inflammatory action to a short thin cord, none of them had come across an instance of complete absence.

I throw on the screen the drawing of a typical case of appendical complication, in which the appendix held two large and hard concretions. The appendix was matted to the tube and ovary, and one concretion, which I returned into the appendix, is seen at its severed end. There was no suspicion of appendicitis previous to operation.

On other occasions I have found an appendical abscess with perforation complicating adnexal disease in which the discovery was not made before the cœliotomy. On two occasions I have had to perform appendectomy after previous cœliotomies—on one occasion after removal of the adnexa, and on the other after hysterectomy.

A case in which I failed to unravel the cause of the post-operative peritonitis, and the only one of the kind that has occurred to me, was the one which taught me the lesson never to close an abdomen without examining the appendix.

I operated on a married woman who suffered from severe dysmenorrhœa, aged 38, for chronic endometritis and cystic ovaries, resecting both of the latter after curetting the uterus. The operation was straightforward. Everything went well, and the bowel had moved, when on the sixth day symptoms of peritonitis set in, and the abdomen became suddenly tympanitic. Exploration of the pelvis revealed nothing. I inserted a suprapubic drainage tube, and began rectal irrigation, but the tympanites continued. I re-opened the abdomen and explored, but discovered nothing beyond a greatly distended bowel. Enemata still acted. She died on the thirteenth day after operation. At the post-mortem I found a circumscribed retrocæcal abscess, the appendix having quite sloughed away. I had no history of appendicitis in this case, neither was there any sign or symptom pointing to its occurrence. The condition of the ovaries and uterus quite explained her symptoms, but there must have been an old appendical condition, probably the dormant remains of a pericæcal abscess, which the operation roused into activity.

Before discussing the point of diagnosis I may say a few words as to the course of the infection or inflammation, whether from appendix to ovary, or vice versa. Hartmann quotes Treub, Stratz, Dutilh, Olshausen, as explaining this through the continuity of the "appendiculo-ovarian" ligament, described by Clado in 1892, who located in this the vessels, spermatic nerves, and lymphatics of the ovary and uterus, and recognized at the time by Broesike, Waldeyer, Nagel, and Martin. Waldeyer, in his standard work on the pelvis (1899), in a footnote, states that it is superfluous to describe the ligament of Clado, at the same time saying that in this he (Waldeyer) is in agreement with Nagel, writing in 1897, and that it is merely an extension of the suspensory ligament of the ovary. This view, however, was expressed five years after Clado's description. In Döderlein and Krönig's work,[1] published in 1907, they write:—

> "The frequent association of appendix and adnexal disease is to be attributed to the appendiculo-ovarian ligament, which connects the appendix directly with the suspensory ligament of the ovary. One can easily see that a retrocæcal abscess may sink along this ligament towards the genitalia, but we believe that it is generally concerned in the extension of extraperitoneal rather than of intraperitoneal abscesses.
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> "The close connexion of the right adnexa with the appendix makes it possible that inflammatory processes in the adnexa may involve the appendix, and vice versa. The anatomical connexion is due to the appendiculo-ovarian ligament, which springs from the infundibulo-pelvic ligament. Parametritic disease may extend along this ligament to the cellular tissue between the mesentery of the cæcum and the iliac mesentery of the appendix, or in the

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[1] "Operative Gynäkologie," 2te Aufl., 1907.

opposite direction, and thus an inflammatory process may extend from the adnexa to the gut.

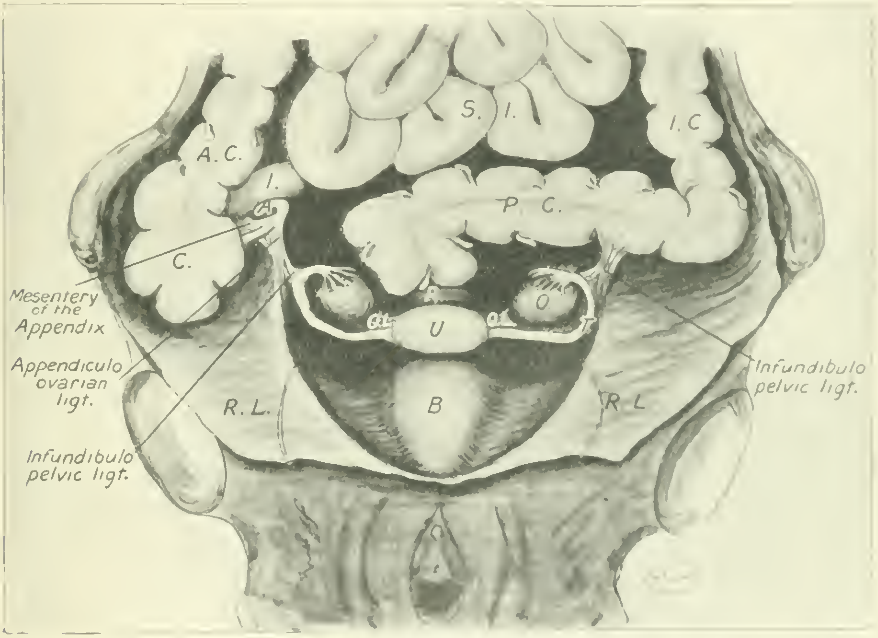
"The adnexa of the right side are generally involved, but when the appendix is abnormally long the inflammatory disease may extend to the left adnexa. The primary condition is generally a chronic adhesive appendicitis. Suppurative processes may extend from the appendix to the adnexa, or from the adnexa to the appendix either along the lymph tracts or by direct rupture from the one into the other.

"When the appendix is long a peri-appendicular abscess may extend into Douglas's pouch, or even when it is normal in length and position the abscess may spread down into this pouch. But only a small proportion of abscesses in Douglas's pouch is of this origin."

On the other hand, Hartmann quotes Futh, who regards this ligament as only seen exceptionally, and its existence he states is denied by Barnsby, Craig, Poirier and Cuneo, and Kelly, who affirm that there is no such connexion between the blood-vessels and lymphatics of the ovary and those of the appendix, and that this so-called "ligament," when it exists, is simply a fold of peritoneum passing behind the meso-ovarium. Hartmann himself leans to the opinion that the inflammation extends through the serous peritoneum; this more easily happening in women through the descent of the appendix into the roomy female pelvic basin, assisted possibly by the pressure of the corset and the relaxation of the peritoneal folds following labour.

I have written to various anatomical authorities in the United Kingdom and have had replies from all. Some are unable to give any decided opinion, but the following abstracts from answers enable us to form a fairly accurate ideal as to British views on the question.

Professor Robinson, of Edinburgh, does not like to pronounce absolutely as to whether it is a definite structure containing vessels and nerves—he rather doubts its constancy, as it would appear to be a secondary connexion between the appendix and the ovary developed after the descent of the cæcum into the iliac fossa. Professor Reid says the material he is possessed of is not sufficient to settle the point. Through Professor Stirling (Manchester) I have Professor Elliott-Smith's opinion: "It exists in some bodies and is occasionally wanting." "When it exists it contains lymphatics and nerves." Professor Fawcett (Bristol), who has made a special study of the appendix, says: "It is not in any sense constant, and is certainly not identical with the peritoneal stalk of the ovary. In some cases the fan-shaped stalk of peritoneum descending into the pelvis has the ovarian vessels in the hinder margin, and the anterior edge may run between the meso-appendix and the right ovary." He considers it much more important to examine the course of the lymphatics, as this does not imply any peritoneal fold, and that modern investigators have quite failed to establish Clado's description of the lymphatic path alluded to; so also has Professor Jamieson, of Leeds. Professor Paterson (Liverpool) has been kind enough to send me a drawing of a dissection made after receiving my question *re* the appendiculo-ovarian ligament. This I show you (*see* fig.) In his opinion "*the normal fold which contains the ovarian vessels, nerves, and lymphatics is the infundibulo-pelvic ligament* (which is much neglected by anatomists but which is constantly present)—a peritoneal fold extending from the pelvic brim to the outer end of the Fallopian tube; the so-called appendiculo-ovarian ligament is a continuation of the infundibulo-pelvic up to the appendix." Dr. Addison (Bartholomew's) writes: "I have never



Drawing of a dissection by Professor Paterson. *C.*, colon; *A.C.*, ascending colon; *I.*, ileum; *S.I.*, small intestine; *I.C.*, iliac colon; *P.C.*, pelvic colon; *R.*, rectum; *U.*, uterus; *B.*, bladder; *O.*, ovary; *O.L.*, ovarian ligament; *R.L.*, round ligament; *A.*, appendix; *T.*, tube.

seen anything myself beyond a trivial peritoneal fold passing from the base of the meso-appendix to the pelvic brim near the ovary and it did not contain vessels. Certainly such a fold as Clado described must be very rare." Professor Symington (Belfast) writes: "I do not believe in an appendiculo-ovarian ligament. The fold of peritoneum containing ovarian vessels, &c., which passes from the pelvic brim downwards to the ovary is not, in my experience, continued upwards and outwards into the meso-appendix. The ovarian vessels certainly do not take that course, and I think the evidence as to the lymphatics is equally clear, but I have not personally made any special injection of the lymphatic vessels. The difference in the development of the appendix and the ovary and of their blood-vessels is a strong argument against such a connexion between the ovary and the appendix as is described by Clado. I had a good specimen in the dissecting room last winter of the inflammation of an appendix involving the outer end of the Fallopian tube, but in this case the appendix was hanging down into the pelvis and there was no necessity to assume a lymphatic connexion to explain an adjacent organ becoming involved. Great care requires to be exercised in the examination of the peritoneum in connexion with folds of the membrane. Nothing is easier than to manufacture them by gentle traction."

Professor Alexander Macalister (Cambridge) says: "There is a peritoneal fold, present even in early foetal life, descending from the neighbourhood of the meso-appendix to the ovarian folds. In or close to this (not apparently always or indeed often in it) descend vessels and nerves, and the ascending lymphatics pass. The stream of vessels, &c., here lies at the attached border of the fold in the subperitoneal connective tissue, but not actually in the fold, at least in the majority of cases, but the fold is usually related to this line of lymph and blood-vessels."

Dr. Reid (Cambridge), who has devoted special attention to the genesis of these peritoneal folds between the appendix and the ovary, has published an important paper on a special peritoneal fold present in 55 per cent. of twenty fœtuses examined.[1] Five of the eleven were females. This is the genito-mesenteric fold, passing downward from the inferior surface of the mesentery in the right half of the abdomen. The anterior border of the fold is in most cases free, and the fold itself is usually continuous below with the suspensory ligament of the ovary. In some cases the ileum may be adherent to its right side, the adhesion occurring close to the ovary, which thus comes to be intimately bound to the ileum. This is not a pathological adhesion, and the continuity of the fold with the suspensory ligament of the ovary is not constant. The two may be separate, although lying in line one above the other. There were also certain definite relations of the appendix to the genito-mesenteric fold, and these relations of the mesentery, ileum, and the fold, as well as in some cases the cæcum, must have an important bearing on the future spread or arrest of inflammation from the cæcum and appendix to the ovary. As Dr. Reid says, the genito-mesenteric fold must be of considerable interest from a surgical and gynæcological point of view.

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[1] "Studies of the Intestine and Peritoneum in the Human Fœtus," Part I, by Douglas G. Reid, M.D., Demonstrator of Anatomy, Cambridge University, *Journ. Anat. and Phys.*, 1911, xlv, p. 73.

It seems clear from these opinions that we must in some cases at least look for another track of infection than by this ligament. I shall refer to this again.

Some significant pathological conclusions may be drawn from the report of MacCarty on 5,000 cases occurring in the Mayos' Clinic,[1] and Murphy's "General Surgery."[2] First, that obliteration of the lumen of the appendix begins more frequently as a pathological rather than as a physiological process at any time from the fifth year of life, and progressively increases with advancing years to complete obliteration. Second, there is the interesting fact that in this series of 5,000 cases carcinoma occurred in 0·44 per cent. of all cases, and 1·6 per cent. where there was partial or complete obliteration due to destruction of the mucosa and destruction of scar tissue; 73 per cent. were females. Murphy comments on interstitial hæmorrhage as a dominating feature of appendicitis. These hæmorrhages may be only discernible to the microscope, but they furnish foci for necrosis and ulceration. H. Crossen[3] refers to a form of appendicitis which he met with on two occasions in women. There were adhesions between the wall of the appendix and the cæcum. In the wall of the latter was an extensive infiltration forming a somewhat large mass simulating an intracæcal growth. Through exceptional mobility of the cæcum this mass dropped down into the tubo-ovarian region. In both cases removal of the appendix was followed by slow but complete absorption of the infiltration.

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[1] *Journ. Amer. Assoc.*, Chicago, 1910, lv, p. 188.

[2] Murphy, "General Surgery," 1911, ii, p. 118.

[3] *Surg., Gyn., and Obstet.*, Chicago, 1910, xi, p. 126.

Döderlein and Krönig comment on the unreliability of statistics that bear on the correlation of appendicitis and adnexal disease, since a number of operators choose the vaginal route when the appendix is not exempted. Krönig, in the work I have referred to, quotes these figures (p. 369) :—

> "Dührssen found it in 3 per cent.; Amann in 280 laparotomies found the appendix diseased in seventeen, that is, 6 per cent.; Kelly in 200 laparotomies found the appendix affected in twenty-five; and Hermes found the same condition in 53 per cent. of the seventy-five cases he observed."

And he adds :—

> "These considerable differences in the frequency of the coincidence of appendicitis with gynæcological troubles are due to the fact that some remove the appendix when there is the slightest adhesion to the adnexa, others only when there is definite disease in the appendix. Since we (Krönig) for some time have in gynæcological operations removed every appendix, and have had it examined microscopically, we have become convinced of the frequency of appendicitis in women; in about 50 per cent. of the cases there were evidences of either acute or recovered appendicitis.
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> "A right-sided pyosalpinx in young patients, in whom there is neither clinical nor bacteriological evidence of gonorrhœa or tuberculosis, is almost certainly due to infection of the tube from a primary appendicitis.
>
> "An abscess in Douglas's pouch results relatively frequently from an appendicitis; its diagnosis may be difficult, as the same objective signs occur with suppurative hæmatoceles and ovarian cysts. We agree with Veit that the presence of pus in Douglas's pouch is not always due to a paratyphlitic abscess, but it would be, in our opinion, incorrect to deny the great frequency of this cause of abscesses in Douglas's pouch."

Krönig puts the definite question "whether gynæcologists should remove a normal appendix in every case in which they open the abdomen." His answer is as follows: "We always extirpate the appendix, because, in the first place, we cannot be always certain whether it is normal or not; in the second because, in our experience, 50 per cent. of women have at some time or other an acute appendicitis; and thirdly because the appendix is functionless in adult women. We believe that it is essential to remove the appendix in every case in which it is adherent to the adnexa, or when there is any evidence of disease in it."

Schauta, writing in 1907, says:[1] "When the appendix is long, and lies deep in the pelvis, diagnosis of these complications is very difficult. It must depend largely on the history. As to the preference to be given to the abdominal or vaginal route in adnexal operations, Krönig and Schauta differ. While they both open abscesses in Douglas's pouch through the posterior vaginal arch, Krönig prefers the abdominal to the vaginal route in operations on the adnexa. And if, in operating for appendicitis, the adnexa have to be examined, he practises the paramedian or median incision.

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[1] "Lehrbuch der gesammten Gynäkologie," Leipz. u. Wien, 1895, ii, p. 371.

On the other hand, Schauta writes: "The question whether, owing to the possibility of these complications, the abdominal route should be preferred to the vaginal in radical adnexa operations must be answered in the negative, though a laparotomy would be preferable when the signs and the history pointed to involvement of the appendix."

In this decision I cannot acquiesce, as I always prefer the abdominal route. It is different in the instance of an abscess localized in Douglas's pouch. It certainly should be, as he says, "opened by a posterior colpotomy, and the appendix afterwards removed by the ordinary method when the acuter symptoms have subsided." In such cases we cannot" (as Hermes argues) "at the same time remove the appendix and be certain to evacuate all the pus. On the other hand, the abdominal route is much more dangerous, as in exposing the abscess in the pelvis many intestinal adhesions must be broken down and a communication with the abdomen may be easily made. But even by this method it is not always possible to remove the appendix. And when the abscess is drained by a posterior colpotomy, the intestinal adhesions remain undisturbed and infection of the peritoneal cavity is consequently avoided. Further, incision of the abscess in the deepest point is the most favourable for drainage; it must be admitted that there is considerable danger of recurrence, but an operation on the appendix in the interval is quite without danger."

A most exhaustive communication on this subject has appeared from the pen of Professor Henri Hartmann, published in the *Annales de Gynécologie et Obstétrique*, 1908, v, pp. 65, 144. Out of 1,707 cases, operated upon by various surgeons, 1,195 were males, and 512 females, showing the greater preponderance of appendicitis in the male. On the other hand, Hartmann believes that this apparent preponderance, arrived at from the statistics of German surgeons, is due to the fact that a large number of cases is treated by gynæcologists, for in the Pathological Institute of Munich, since 1854, of 1,800 autopsies, deaths from peritonitis following perforation of the appendix were more frequent in the female than in the male. He quotes Hunter Robb, who systematically practised removal of the appendix in 370 cœliotomies. Histological examination made in every instance showed that in 136 the adnexa were healthy, and in twelve of the latter there were inflammatory lesions of the appendix. In 148 cases of affections of the adnexa there were twenty-five instances of appendicitis, that is, in 8·82 per cent. the appendix was involved, while the adnexa were sound, and in 16·89 per cent. both the appendix and adnexa were diseased. Further, Peterson, who removed the appendix systematically in 500 cases of cœliotomy, for affections of the genitalia, as the result of microscopic examination, found that 47 per cent. of these had lesions which were the result of appendicitis.

Howard Kelly, in his recent work on "Medical Gynæcology," 1909, devotes a chapter to the conditions of association between appendicitis and pelvic diseases, which is well worth perusal. In the ten years previous to 1904 he removed the appendix in 240 cases, the majority of which were combined gynæcological and appendical affections. In ninety of these the appendix alone was affected; in sixteen only was it removed as a prophylactic measure. Out of 300 operations for cystoma, the appendix was adherent to the tumour at the right side in seven, and at the left in three instances. He remarks that dermoids and cysts in which torsion has occurred are more apt to cause adhesions, and hence more frequent involvement of the appendix, and some inflammatory changes in its walls. It is, as he remarks, a common experience that the appendix is rarely implicated in myomata, while in ectopic gestation it is often found adherent, and in 10 per cent. of the extra-uterine pregnancies in Kelly's clinic these adhesions or acute inflammations were present. Such appendical implication would explain those septic cases of extra-uterine pregnancy in which a septic cavity is found in the wall of the sac, or a septic abscess communicates with it. This happened in a case of my own in which the appendix and sac were both incorporated.

I have sought, for the purpose of this address, the opinion of some leading gynæcologists in Europe and America, most of them personal friends, as to their views on removal of the appendix. Their replies I briefly epitomize.

I take first the opinions of a few American confrères. I find that Kelly practically always removes the appendix unless it has been converted into a thin cord by previous disease. This step he has been an advocate of for several years.

E. Montgomery, of Philadelphia, writes: "I am in the habit of removing the appendix in every abdominal operation, whether it seems diseased or not. It requires but a few minutes' additional time, it apparently does not add to the gravity of the procedure, and it saves the patient subsequent trouble. The only case in which I would consider the retention of the appendix would be where the patient has undergone a prolonged and serious operation, and it seems undesirable to subject her to further interference." He refers to the fact that in one case he had to re-open the abdomen two weeks after an ovariotomy for acute appendicitis, and in other cases at a later period.

Dr. Polk, of New York, in every case of cœliotomy, the condition of the patient permitting, examines not only the appendix but the surface of the kidneys and under surface of the liver, and the appendix is removed if it shows, either by touch or sight, evidence of disease or malformation, such as extreme length. If it is of normal length, and if free of enlargement at the extremity, or any other evidence of disease, it is not disturbed.

Dr. H. J. Boldt, of New York, exposes the appendix for examination in every cœliotomy for pelvic disease, and removes it in nearly all instances. Of several hundred appendices thus removed and examined, more than 75 per cent. of them were reported to be in a condition of inflammation, usually catarrhal. The only exception to non-removal is when the previous intervention has been of such a serious character, and consumed so much time, that it would be inadvisable to prolong the operation for the purpose of removing a seemingly normal appendix.

Dr. William Mayo, of Rochester, Minneapolis, says: "We always examine the appendix when a cœliotomy is performed for affections of the pelvic viscera, and, if diseased, remove it, unless for some reason the condition of the patient will not permit of this being done. We do not practise or advise the removal of a normal appendix."

Dr. J. B. Murphy, of Chicago, always examines the appendix, but is not in favour of its removal as a routine procedure in pelvic operations. He has had but one case of appendicitis sixteen days after a hysterectomy. If there were "a clean-cut history of previous appendicitis this would point to its removal."

Professor Lapthorne Smith, of Montreal, examines every appendix at the beginning of an operation, and adds, "The more we think of the dangers of the appendix, especially during the puerperium, the better it would seem that we should remove it whenever we have the abdomen open for other reasons," and he quotes some cases in support of this view.[1]

Turn we now to some German colleagues :—

Professor Paul Zweifel, Leipzig, does not lean to removal of the appendix in every case, though he always examines it, ablating it only when after macroscopical examination it is found diseased. The secondary inflammation spreads through the lymphatics.

Dr. Zurhelle, in the absence of Professor Fritsch, of Bonn, says that the appendix is examined in every operation on the pelvic organs, and is removed in all cases where there has been a history of appendicitis, if it is macroscopically changed or adherent, but not in every cœliotomy.

Professor Bumm seldom removes the appendix. He rather objects to the necessary prolongation of the operation, and considers that after appendectomy the risk from bacterial flora is increased ; also that the median incision is inconvenient. Therefore he only removes it when it is injured in the operation, or when well-marked disease is present.

Professor Schottländer joins absolutely in the late von Rosthorn's rule, one which is followed by all his best pupils, and was published after his death in the Transactions of the International Congress at Budapest in 1909, and also in the *Monatsschrift für Gynäkologie*, October 1, 1909, that in every cœliotomy performed for affections of the pelvic organs, the appendix should be examined, and if macroscopically involved, is to be removed. Otherwise, not yet knowing whether it may not have a peculiar function, appendectomy is not indicated.

This also is Professor Wertheim's opinion, but he much more frequently selects the vaginal route.

Professor Döderlein considers it most important to take the appendix into consideration in every abdominal operation for pelvic disease, removes it in inflammation, when adhesions are present, or any diseased condition ; but he is of opinion "that removal of a healthy appendix is an absolutely unnecessary attack."

Professor Schauta examines the appendix in every case of laparotomy, and removes it only if there is any unhealthy appearance.

Professor Schultze, of Jena, is in favour of removal of the appendix in pelvic operations of women unless this is contra-indicated by some special complication or through prolongation of the operation. He does not always examine it in vaginal cœliotomy.

Professor Dmitri de Ott, in every cœliotomy and colpotomy when the abdominal cavity is opened, examines the appendix, but does not remove it unless there is some lesion or abnormality.

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[1] *Amer. Journ. of Obstet.*, New York, 1909, lix, No. 6.

Professor Mangiagalli examines it in every case, and removes it if it is adherent, if there is fluctuation in its free extremity, if its walls are hyperplastic, or if it contains a foreign body or concretion.

Professor Jacobs, of Brussels, examines the appendix in every cœliotomy for affections of the genitalia, removing it when there is any disease, if it is inflamed, adherent, or contains a foreign body. He draws a distinction between the inflammation which is secondary to contact with diseased adnexa, and that which is a primary infective affection of the appendix itself. While recognizing the comparative frequency of appendical complications in diseases of the adnexa, he does not agree with the view that the appendix should be removed in every cœliotomy performed for such.

Professor Pozzi, while deeming its examination necessary in every case, and removing it in case of a lesion or excessive length, thinks it quite unnecessary to remove it in all other cases.

Taking the practice in Ireland as expressed by Dr. John Campbell, of Belfast, and Drs. Tweedy, Jellett, and Kidd, of Dublin, the appendix is examined and its condition ascertained in every cœliotomy, but it is not removed either as a prophylactic or routine measure; while, as representative of the practice in Scotland, Sir Halliday Croom, after an experience of over 2,000 cœliotomies, never removes the appendix unless it is adherent or the seat of disease. It is always examined. If asked in a doubtful case as between ovaritis and the appendix, he would say that it was generally appendicitis. This also is the practice of Dr. Munro Kerr.

Dr. Murray, of Liverpool, who has had a large experience in this direction, writes to the same effect.[1]

Of 269 abdominal sections performed by Christopher Martin in 1910 (thirteen of these being for appendicitis alone), in 16 per cent. of the remaining 256 laparotomies (90 per cent. of these being for affection of the genitalia) it was necessary to remove the appendix.

As to diagnosis, we are not here discussing those cases in which the typical signs and symptoms of uncomplicated appendicitis, whether of the more acute or chronic type, are present. Rather am I referring to those in which the appendical symptoms are masked by the adnexal, and the presence of adnexal inflammation or disease overshadows the appendicitis, as well as that class of case in which the affection of the adnexa is masked by the symptoms of appendicitis.

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[1] Since writing the above I have had replies from Professor Bossi (Genoa) and Professor Gustav Klein (Munich). Both inspect but do not remove the appendix unless it is diseased or in some way implicated.

It is well known that many times an appendix has been removed in which but comparatively trifling changes have been detected, and in which a subsequent operation has been necessitated through a continuance of the symptoms due to an ovarian or tubal condition overlooked at the time. The liability to error occurs because no gross changes in the adnexa have been discovered by vaginal examination or possibly have not been sought for, and the predominant partner in the trouble has excited and demanded all the surgeon's attention. This exactly occurred in a case which many years since I had under my care, and in which I was doubtful of the appendical complication, not finding evidence sufficient in the adnexa to explain the symptoms. A distinguished surgeon was brought in, and I urged the need for immediate operation. He did not see his way to accept this view, and his attitude was supported by a physician colleague. Feeling, as I then said, that the time for "masterly inactivity" had passed, I retired from the case. A few days afterwards operation was decided upon, and I was invited to be present. When the abdomen was opened a large appendical abscess was found, and the bowel had ruptured. The issue was fatal.

In regard to diagnosis as between the two points at which we may locate the base of the appendix, McBurney's or Lanz's, the researches of A. Garan would appear to prove that the former is 2 in. above, and that the latter approximates more closely to the base. McBurney's point, he considers, lies rather over the inner medial portion of the ascending colon.[1] Still, as Murphy points out, the cutaneous hyperæsthesia over McBurney's point, due, as he believes, to the distribution of the ileo-cæcal segment (including the appendix) to this zone (Meisel) is the most important of the three classical clinical signs—namely, pain, rigidity, and hyperæsthesia—which indicate that the appendix is affected. Murphy leans to this explanation of the hyperæsthesia rather than to the occurrence of lymphangitis (Lennander), excitation of the sympathetic ganglia, or inflamed lymph nodes. Olshausen's diagnostic distinction is only of use in some cases to aid us in distinguishing the seat of the exudation. He lays stress on the fact that the peritoneal exudation in appendicitis is not as wide as in salpingitis, though it may extend to a greater length.

The various conditions and complications found when the appendix is involved with adnexal inflammation render it most difficult to say which of the two has taken the primary part in the morbid process. The appendix may be simply adherent to the affected adnexa. It may be diseased and adherent, there may be a swollen appendix above the inflamed adnexa, though free from them ; a pyosalpinx may be formed with an appendical abscess or perforation, or the purulent focus may lie outside and free from either the appendix itself or the adnexa. Hartmann instances such a condition where there was a collection of pus isolated in the recto-uterine cul-de-sac, following appendicitis, the cæcum being at the level of the superior pelvic strait. Here the adnexa formed part of the anterior walls of the abscess ; they were healthy, and the lumen of the tubes was not obliterated. In such a case there could be no doubt where the mischief began, nor can we have any hesitation in deciding when the left adnexa are healthy, and there is an inflamed appendix or abscess at the right side, to which the adnexa are adherent. Or, contra, inflamed adnexa or pus tubes may be at both sides, and a simple adherent or inflamed appendix. So, when we find a distended and inflamed appendix pathologically altered and adherent to the inflamed adnexa, and that it contains a foreign body or concretions, we may have little doubt as to where the mischief originated. But when the morbid process has been of old standing, and has advanced beyond a certain stage, both the adnexa and appendix being involved, it is very difficult to pronounce definitely in which situation the inflammation commenced, and from whence it spread.

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[1] *See* J. B. Murphy, "General Surgery," 1911, ii.

It must be remembered that once adhesions arising from inflamed adnexa have bound down the appendix, or possibly strangled it, pathological changes begin from congestion of its tissues and vessels which lead up to the secondary appendicitis. Thus Hartmann, in his communication, differentiates between three types of case on which he has operated. In the first, the adnexa and appendix were both mutually and seriously involved ; in the second, the sections of the ablated tubes showed the characteristic changes of chronic salpingitis, *while the mucous and muscular coats of the appendix were normal*, the serous and subjacent tissue alone being involved, and infiltrated with a mass of embryonic cells and leucocytes. Thus an ascending genital inflammation had attacked the external tunics of the adjacent appendix. In the third type, microscopic examination showed gross changes in the appendix, while the tubes were little affected, the serous covering, and the subserous cellular tissue alone being affected. He argues that a secondary infection of the tubes or the appendix in the peritoneal cavity, or through the peritoneum, is thus made clear. From the fact that in a considerable number of cases in which the appendix is unhealthy the adnexa of the left side only are affected, he concludes that the infection travels by the pelvic serous peritoneum. Of his thirty-nine cases operated upon for adnexal affection in which the appendix was involved, there was disease in both sides in twenty-one, in the right side in ten, and in the left in eight.

I need not delay to insist that the bimanual and recto-vaginal examination made with the pelvis elevated, under anæsthesia, is essential in making our diagnosis in these doubtful cases. Unfortunately, anæsthesia sometimes cannot be availed of. In two such cases recently seen in emergency I was certain in one, from the presence of a large pelvic exudation, and the collateral abdominal signs and symptoms, that we were also confronted by an appendical abscess. This turned out to be the case. In the other the whole antecedent history, and the rapidity of the onset of the attack, pointed to appendicitis, while vaginal examination revealed a bilateral pelvic exudate and a fixed uterus. Operation disclosed at once a perfectly normal short appendix pushed upwards with the cæcum by a large pelvic purulent collection, and a double pyosalpinx, all the pelvic contents being matted together. The patient made a good recovery.

An interesting gynæcological association in appendix troubles is their occasional relation to dysmenorrhœa. This may be due to the involvement of the ovary by adhesions, or otherwise, through the affected appendix (Sonnenburg and Kruger), or be due to a functional and nervous disturbance (Fraenkel). After appendectomy the dysmenorrhœa has disappeared (Hermes, Brandt, Beahan, Moraller). We cannot either attach much importance to Delagèniere's view that if the appendix be the source of the pain it occurs before or at the beginning of the period, whereas in the ovarian it supervenes rather before its close.

MacLaren drew special attention to the causal relationship between appendicitis and dysmenorrhœa,[1] pointing out that dormant appendix disease was the undiscovered cause of dysmenorrhœa in certain cases. The more characteristic signs, according to Soupault and Jouaust,[2] of dysmenorrhœa due to appendicitis are: (1) The occurrence of severe pain before and during the onset of the catamenia; (2) the presence of gastro-intestinal symptoms or muco-colitis; (3) symptoms of pain, discomfort, and pressure in the right iliac fossa, disappearing with the advent of menstruation; (4) the rise in temperature in the rectum as compared with that in the axilla with an accompanying increase in the rapidity of the pulse. From all the evidence before us, it would appear that this source of dysmenorrhœa is due possibly, as Soupault surmises, to an inflamed and adherent appendix which is attached to the ovary as a result of previous appendicitis.

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[1] *Amer. Gyn. and Obstet. Journ.*, New York, 1900, xvii, p. 14.

[2] *Bull. et Mém. de la Soc. méd. des Hôp. de Par.*, 1903, xx, p. 1307.

You will draw your own conclusions from the general tenor of these opinions, and your individual experience.

So far as I know, the usual practice in the London clinics is not to remove the appendix unless it be abnormal or diseased. I think we may arrive at these deductions from our present knowledge:—

(*a*) In a fairly large proportion of affections of the internal genitalia, both the appendix and the adnexa are involved in the inflammatory or morbid process.

(*b*) The inflammation may spread either from direct contact of the appendix with the adnexa through the peritoneum, the lymph channels, the appendiculo-pelvic ligament, or such a structure as that described as the appendiculo-ovarian ligament.

(*c*) Infection from the appendix to the adnexa has to be more specially feared when there is an appendical abscess with gangrene or tuberculosis, and from the adnexa to the appendix when there is right-sided pyosalpinx, malignant disease, tuberculosis, or gonorrhœa. Such a complication has to be remembered in the puerperium.

(*d*) The differential diagnosis as between appendical and adnexal inflammation, whether acute or chronic, must depend on the preponderance of the classical signs and symptoms characteristic of each which are present, the earlier history and progress of the case being taken into consideration with these. Kelly quotes R. T. Morris as placing abdominal rigidity foremost in the clinical signs indicative of appendicitis. Still, it is clear that in a spreading perimetritis, in which the appendix is secondarily involved, this test must be fallacious. As a rule we may say that an acute inflammation occurring in a young person, in which there is ambiguity as to its seat, will, in the majority of cases, be due to appendicitis. The fact that it is the left adnexa which are found diseased does not exclude the possibility of this complication, though it is more frequent when those of the right side are affected. The appendix, therefore, should be examined in every case.

(*e*) When the abdomen is opened for a grave appendical trouble the adnexa should be examined.

(*f*) Appendical adhesions are a frequent cause of post-operative and recurrent pains.

(*g*) The appendix should be removed in every instance where abnormality or disease is found.

It should not be left if it be of unusual length, when the right adnexa are removed, or when, from the character of the operation, there is a probability of future adhesions forming which may involve the appendix; indeed, it is doubtful if it be not wiser to remove it in every cœliotomy for right adnexal disease.

Operative action arising out of these conclusions will always be dependent on the severity of the operation, and the influences its prolongation may have in lessening the chances of the patient's immediate recovery.

A Case of Carcinoma of the Rectum occurring in a Patient from whom Malignant Papillomatous Cysts of both Ovaries had been removed six years previously.

By Mary A. Scharlieb, M.S.

Mrs. R. Patient was first seen on September 29, 1904. Aged 51. Married at the age of 29. One child, aged 21. Menopause, May, 1904. She complained of much irritation of the bladder, abdominal pain and enlargement. Her mother died at the age of 69 of abdominal tumour, one sister of a tumour, and another sister had cancer of the breast. On examination there was a fluctuating abdominal swelling the size of a six months' pregnancy. *Per vaginam* a soft mass was felt behind the uterus. At the time of operation two ovarian cysts were found; that on the left side contained 4 pints of reddish-brown fluid, also abundant papilliferous growths, pinkish-white in colour, and very friable. On the right side there was a smaller ovarian cyst also full of papilliferous growths. The peritoneum of the uterus was so damaged by adhesions that supravaginal amputation had to be done.

The pathological report was that the growth was a malignant papilloma arising in the oöphoritic portion of the ovary, the cubical epithelium covering the papilliferous tufts showing enormous proliferation, and the cells having trespassed into the very cellular fibrous tissue skeleton of the growth.

Patient had a bad time of it, but she eventually made a good recovery. Heard of at intervals as being very well and strong.

November 2, 1910: The patient reported that she had been well for five and a half years, but for three months she had suffered from constipation and diarrhœa, and on October 29 had passed much blood from the bowel.

Examination under $CHCl_3$: A large ulcerating cancerous mass was felt in the anterior rectal wall 3 in. or 4 in. from the anus. The upper border of the growth could not be reached, but it appeared to be oval in shape and not to surround the bowel.

Operation, November 7, 1910: Removal of the lower 8 in. of the bowel by the Aldrich-Blake method, slightly modified to include the stump of the uterus; many glands were also removed.

Pathological Report: A pedunculated gangrenous growth, 3 in. long and 2 in. wide. The mucosa is intensely injected where it has come in contact with the tumour. Adherent to the bowel by a mass of fibro-fatty tissue is the stump of the cervix which looks healthy and has not been sectioned. Sections have been taken (1) of a terminal lobe of the growth; (2) from the pedicle of the same; and (3) from the neighbourhood of the pedicle where it adheres to the bowel. The terminal lobe is capped by a wide zone of necrosis, but there is some cancerous tissue discernible in its centre. The pedicle and the third section show the presence of columnar tubules invading muscle. The growth is a columnar carcinoma such as arises primarily in the bowel.

The lower part of the bowel, which had been fixed in the anus, sloughed, and left inguinal colotomy had to be performed on November 15. For a time the patient did well, but ultimately died.

A Case of Adeno-carcinoma of the Uterus and Ovaries.

By MARY A. SCHARLIEB, M.S.

MISS J. W., aged 69. Puberty at the age of 13. Menopause at the age of 38. Nothing abnormal until Christmas, 1909: since then vaginal hæmorrhage at intervals. From the middle of February, 1911, to the date of the first visit, March 7, 1911, the hæmorrhage was severe, coming with gushes and accompanied by large clots. All other functions and organs apparently normal. On examination the uterus appeared to be large and not freely movable: it was thought that multiple fibroids existed.

Panhysterectomy was performed on March 8. Each ovary was enlarged to the size of a fœtal head at about eight months' development:

the pedicles were easily secured, and they were delivered unruptured. It was thought that the ovaries had undergone cysto-adeno-carcinomatous degeneration. On splitting the uterus open it was found to be filled with a soft mass of new growth, and a nodule in the anterior wall appeared to be of the same nature.

Pathological Report: The sections of the ovarian growths prove each to be malignant. In the one tumour the section shows an abundance of dense fibrous stroma with relatively little gland tissue. Alveolar spaces are present containing the tubular processes of an adenomatous growth. In the majority of these spaces the epithelium is quite simple, but in some it is proliferating in a complex manner. The other ovary presents much more definite signs of malignancy. In this the glandular tissue predominates, and the stroma of fibrous tissue is reduced to a supporting framework for the former. The tubular processes are not so simple; the epithelium forms large masses of spherical cells. The picture thus presented is typically that of adeno-carcinoma.

The uterine wall is invaded by an adeno-carcinoma consisting of tubular processes which maintain their adenomatous conformation to a striking degree. This is the growth which is described under the name of adenoma malignum.

Additional Report: There is a striking similarity in the histological features of the uterine growth and those of the ovaries, so much so that it was thought probable that the Fallopian tubes would show signs of the transference of cancer cells between these organs. Sections have been made from each tube, but they show no involvement in the malignant process. It is probable that the ovarian growths are secondary and that the uterine cancer is the primary lesion, but the question of a primary bowel carcinoma should be borne in mind, especially as the microscopical characters of these tumours are similar to those of cancer of the alimentary tract.

The Fallopian tubes contain no sign of cancerous deposit.

DISCUSSION.

The PRESIDENT (Dr. Macnaughton-Jones) said that in his view malignant degeneration co-existing in the uterus and the adnexa might begin quite independently in either. He had shown at the Gynæcological Society sections from a case in which the adnexa of both sides, with the exception of one Fallopian tube, were carcinomatous, while the uterus was healthy; also a specimen of two large carcinomatous ovaries, each nearly the size of a small cocoanut, where the uterus was free. It was not uncommon to find, as in another case which he had exhibited, extensive papilloma of both tubes without involvement of the uterus. As to the rectal case, he had little doubt that it had arisen independently, and it was an interesting fact shown by statistics of the Mayos' clinic in Rochester, U.S.A., that some 10 per cent. of the cases of cancer of the rectum had originated as hæmorrhoids and had been treated for these up to a comparatively short time before operation. He (the President) was certain that if hæmorrhoids and such conditions as fissure or polypus of the rectum were dealt with early, there would be less necessity for the more extensive and serious operations required through the comparatively late discovery of the malignant condition.

Dr. CUTHBERT LOCKYER said the thanks of the Section were due to the exhibitor for bringing forward two specimens of great interest. In the case of Miss J. W. the question arose as to what relationship existed, if any, between the malignant ovarian growth and that in the body of the uterus? Four propositions might be put forward: (1) That the extensive uterine carcinoma was the primary growth and that the disease in the ovaries was secondary to it; (2) that vice versa, the ovarian cancer was primary and bilateral and that the uterine cancer was secondary thereto; (3) that both ovarian and uterine growths were metastatic to some undiscovered primary lesion of the gastro-intestinal tract or abdominal viscus; (4) that the ovaries and the uterus were both involved in a malignant process simultaneously, there being no question of the one being primary and the other secondary, and likewise no question of another growth being situated elsewhere to which as in proposition (3) these growths bore the relation of metastases. The speaker favoured the last view; he did not think the first explanation tenable, because whilst the tubular adeno-carcinoma of the uterus represented a fairly common type of uterine growth he had never met with a case of this disease where it had been proved that it caused secondary deposits in the ovary. It was known to spread by the lymphatics and cause involvement of lymphatic glands but not of the ovaries. Dr. Lockyer had shown a case where bilateral ovarian cancer had spread by the perivascular lymphatics of the Fallopian tubes to the uterus, but here there was no large growth involving the whole uterine mucosa and deeply eroding the muscle as in Mrs. Scharlieb's specimen. Mr. Glendining had published a similar case showing spread of cancer from the ovaries via the lumen of the tubes to the uterus. German writers also had drawn attention to the same condition—i.e., the transference of cancer from ovaries to the uterus through the medium of the tubes—so that it was an established fact that cancer of the uterine body secondary to ovarian carcinoma had been met with: but the objection to this being the explanation of the condition before them was the very advanced stage to which the uterine cancer had reached whilst no involvement of either tube existed (a point Dr. Lockyer had specially investigated). He was willing to admit, however, in connexion with the latter objection, that he had examined recently the uterus and a large ovarian growth which had been removed thirteen months after bilateral salpingectomy for tuberculous salpingitis. Here the ovarian growth was an adeno-carcinoma of papillary type, and the uterus was the seat of cervical and corporeal cancer, proving that if there was an association between the uterine and ovarian growths the tubes were non-essential for the spread of the disease from one organ to the other, but the speaker's own view was that the disease had arisen *simultaneously* in both ovaries and uterus, and that this was not an example of pelvic metastasis from some primary source higher up such as had frequently been suggested by observers who spoke of implantation by gravitation of cancer cells. Dr. Lockyer had seen pseudo-mucinous degeneration of .the appendix vermiformis in connexion with pseudo-mucinous cystadenoma of the opposite ovary, both innocent conditions, but suggesting that the epithelium in different parts of the body was prone to the same degenerative change, and he favoured the view that malignant change in the epithelium might show the same propensity and demonstrate itself synchronously in more places than one, and holding this opinion, Dr. Lockyer dismissed the idea of either growth in Mrs. Scharlieb's case being metastasis.

As regards the case of rectal carcinoma, it was an interesting fact that many years before there were bilateral papilliferous ovarian adeno-carcinomata, but in the speaker's opinion this had nothing to do with the occurrence of columnar cancer in the rectum—i.e., the rectal growth need not be considered as a recurrence.

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